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NATURE

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> "*To the solid ground*
> *Of Nature trusts the mind which builds for aye.*"—WORDSWORTH.

THURSDAY, MAY 7, 1891.

FOSSIL INSECTS.

The Fossil Insects of North America, with Notes on some European Species. By Samuel H. Scudder. 2 Vols. 4to, Illustrated. (New York: Macmillan and Co., 1890.)

THE name of Mr. S. H. Scudder is familiar to students of every branch of zoology through his invaluable "Nomenclator Zoologicus." Though that work alone would be sufficient to earn the gratitude of zoologists, yet the author's claims to especial distinction really rest on the results of his investigations into the structure and distribution of fossil insects, and more particularly those of North America.

The magnificent work before us, containing considerably more than a thousand pages of letterpress, and illustrated by no less than sixty-two beautifully-executed plates, as well as by numerous figures in the text, contains, in a collective form, practically the whole of the author's contributions to the history of North American fossil insects, together with much important information relating to those of Europe. In reality, however, it treats of more than is revealed by its title, since the author includes under the head of insects not only the animals usually thus designated (which he distinguishes as Hexapods), but likewise the Myriopods and Arachnids. Since the issue of the work is limited to 100 copies (each separately numbered), it is probable that it will soon acquire an adventitious value above that which it possesses from its intrinsic merits. Apart from the author's admirable account of fossil insects (in the larger sense of the term) contributed to Prof. von Zittel's "Palæontologie," the work is the only one giving an exhaustive history of the subject, and is therefore invaluable to all interested in this branch of study. And the excellent manner in which the volumes are turned out demands a meed of praise alike to author, artists, and printers. Indeed, the only serious fault in the book is that in the first volume no explanation of the plates is given otherwise than in the text, at the close of the articles they severally illustrate.

The first of the two volumes treats exclusively of the pre-Tertiary insects, and consists of a reprint of upwards of twenty articles and essays published in various serials, dating from December 1866 to September 1890. The second volume, which is a replica of the one recently issued by the U.S. Geological Survey of the Territories, formerly under the charge of the late Dr. F. W. Hayden, contains practically the whole of what has been written concerning the Tertiary fossil insects of North America, in which field the author, with one small exception, is the sole worker.

In the first volume, as we are informed in the introduction, the whole series of essays shows the manner in which the author's views have been gradually modified in certain respects with increasing knowledge; and we think he has exercised a very wise discretion in allowing the articles to stand as they were written, and thus permitting the gradual evolution of his later views to be traced.

The earliest known true insect is *Palæoblattina* of the lower part of the Upper Silurian of France, regarded by its describer as a cockroach, although considered by our author as probably one of the Neuropteroid Palæodictyoptera (p. 286); but with this exception the insects from the Upper Devonian of the United States claim the earliest position. It is, however, only (as the author tells us elsewhere) when we reach the coal-measures that we find insect-faunas of any considerable extent, such as those of France and Illinois. The Permian, if, with the author, we refer the coal of Saarbrück to the Carboniferous, is, however, poor in insects; and the Trias, with the exception of that of parts of Colorado, almost barren. The later Secondary beds of America are likewise very barren of insect-remains, so that we have to turn to Europe to gain any definite knowledge of the fauna of that date. In the Tertiaries abundant insect-faunas occur in several river and lake-basins of both hemispheres; two of the most celebrated being the Florissant basin of Colorado, and that of Œningen on the Rhine.

The wings of the Palæozoic insects being those parts of the body which are most commonly preserved in a satisfactory condition, Mr. Scudder, at the commencement of his studies, devoted particular attention to this subject; and the first volume commences with an inquiry into the relationship of the Neuropteroid insects of the North American Carboniferous to the existing Neuroptera, as exemplified by the structure of their wings. It would be out of place here to allude to the variations in the structure of the veins of the wings presented by different groups of insects, and their derivation from a common plan of structure; and we may accordingly proceed to notice the most interesting chapter in the whole volume. This is the essay on Palæodictyoptera, commencing on p. 283. Here we have a detailed account of the reasons which induced the author to separate the whole of the Palæozoic insects from the existing orders under the name of Palæodictyoptera—a term first proposed by Goldenberg in lieu of Dohrn's preoccupied Dictyoptera, which had been suggested for an order typified by the Permian *Eugereon*. This order is defined more by the generalized characters of its various members, and the lack of those special characteristics which are the property of existing orders, than by any definite peculiarities of its own. One of its most important features is, however, that the two pairs of wings are always closely similar to one another, being equally membranous, and with the six principal veins always developed. With the exception of a few cockroach-like insects found in the American Trias, the Palæodictyoptera not only includes all the insects of the Palæozoic, but is restricted to that period, and is, therefore, extremely convenient to the geologist. The order is divided into various sections, which are severally regarded as the ancestors of the existing orders whose names they bear. Thus, the Palæozoic cockroaches constitute the Orthopteroid Palæodictyoptera; while we have a Neuropteroid section represented by *Platephemera*, *Miamia*, &c.; and an Hemipteroid one by the above-mentioned *Eugereon*. The presence in wood of Carboniferous age of borings similar to those made by modern Coleoptera, further suggests the existence of a Coleopteroid section of the order. The author (p. 320) considers that such Coleopteroids "at first showed no greater distinction between the front and hind wings than existed in other Palæodictyoptera; but afterwards those races were preserved in which the thickening of the membrane of the upper wings the better protected the insects in their burrows for the marriage flight in open air."

The author gives a still fuller account of the reasons for adopting the order Palæodictyoptera, in the essay on "Winged Insects from a Palæontological Point of View" (p. 317), from which the preceding extract is taken. Great stress is there laid on the fact that the differentiation of wing-structure characteristic of modern insects did not exist in those of Palæozoic times; all of them having a common type of neuration barely admitting of division into families. The differences in the organs of the mouth, as exemplified by the biting *Progonoblattina* (a Palæozoic cockroach) and the suctorial *Eugereon*, are considered merely as physiological adaptations of no morphological value (pp. 284, 285).

The facts and arguments detailed by the author leave, then, no doubt as to the close affinities and undifferentiated characters of all the Palæozoic insects; and also that the group Palæodictyoptera includes the ancestors of a considerable number of the existing orders of insects. Since, however, all the latter are clearly divergent branches from one or more common stocks, and are in no sense ancestral to one another, the suggestion arises whether it might not be advisable to group all the existing orders together—say, under the name of the Neodictyopterine "series"; and to rank the Palæodictyoptera as a "series" of equal value, in which the various members were not sufficiently differentiated from one another to constitute "orders." It is a very significant fact that, while the Palæozoic insects show ancestral forms of those recent orders grouped together by Packard as the Heterometabola, they include no ancestral types of the more specialized orders—Lepidoptera, Hymenoptera, and Diptera—constituting the Metabola. We have, therefore, proof that these specialized types are of later date; and it thus appears that palæontological evidence is in favour of Packard's classification.[1] Of the existing orders of insects it appears, indeed, that while the Neuroptera, Orthoptera, and Coleoptera are more or less fully represented in the Trias, it is not till the Lias that we meet with Hemiptera (Rhynchota), although *Eugereon* may be taken as sufficient evidence that a Triassic member of that order must have existed. None of the Metabola are known before the Lias, the Diptera and Hymenoptera dating from that epoch, while the Lepidoptera are unknown till the Middle Jurassic.

Though space does not permit of much further reference to the true insects of the pre-Tertiary epochs, we cannot pass over the interesting essay (p. 323) on the oldest known insect larvæ. These larvæ, which appear to be very abundant in the Trias of the Connecticut River, are known as *Mormolucoides* (*Palephemera*), and there has been much discussion as to whether they indicate Coleopteroid or Neuropteroid insects. Mr. Scudder's mode of treating this difficult question is a model of palæontological induction. After carefully reviewing all the evidence, he concludes that the fossils come nearer to the larvæ of the Neuropterous families *Perlidæ*, *Ephemeridæ*, and *Sialidæ*, and that the relationship is nearest to the latter family, which belongs to the true Neuroptera. Another exceedingly interesting article (p. 433) refers to the cockroaches of the Fairplay beds, Colorado. Several of the species from these beds belong to the Palæodictyoptera, showing the complete interdependence of two of the veins of the fore-wing characteristic of the Palæozoic types. Others, however, are true Orthopteroid cockroaches, and we thus seem to have presented to our view the very period when the Palæodictyoptera were passing into the Orthoptera. From the mingled Palæozoic and Mesozoic facies presented by their insect fauna, the author is disposed to refer the Fairplay beds to the Trias; although, as is so frequently the case, the plant-evidence does not accord with that presented by the animals.

Passing to the Palæozoic Myriopods, we notice that while all the forms described in the earlier essays are clearly referable to extinct ordinal groups, the progress of discovery has recently shown (p. 393) that side by side with these lost types there existed in the Coal-measures of Illinois Centipedes closely allied to existing forms, and belonging to the same ordinal group (Chilopoda). The essays respectively commencing on pp. 195 and 247 of the first volume give the full history of the specimens on which the author founded the orders Protosyngnatha and Archipolypoda. The former group is represented only by a single specimen from the Carboniferous of Illinois, described as *Palæocampa;* this curious creature being of small size, and in its short body, with pencils of bristles on the back, presenting a superficial resemblance to the well-known larva of the tiger-moth. Of more interest are the Archipolypoda, confined in America to the Carboniferous and Permian, although represented in the "Old Red" of Scotland. A restoration in Plate vii. A, of one of the largest of these creatures (*Acantherpestes*) gives an excellent idea of their extraordinary appearance; the animal being represented as emerging from the water and ascending the stem of a *Lepidodendron*. The figured species attained a length of about one foot; its amphibious habits being inferred from the presence of lateral apertures presumed to be branchial. The Archipolypoda agree with the Diplopoda, or Millepedes (and thereby differ from the Chilopoda), in having two ventral plates, each carrying a pair of limbs, to every dorsal plate, but differ in that each dorsal plate occupies at most only two-thirds, instead of nearly the whole of the circumference of the body. The larger species, like the figured one, were further distinguished by carrying rows of long spines on the dorsal plates. The smaller forms originally discovered by Sir J. W. Dawson in the Sigillarian stems of Nova Scotia, which were doubtless of purely terrestrial habits, and have been described as *Xylobius* and *Archiülus*, appear to indicate a distinct group of this order approximating to the modern Millepedes.

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[1] Many authorities, attaching more importance to the nature of the metamorphosis, transfer the Coleoptera to the higher group (Holometabola), in which some also include the true Neuroptera, placing the Pseudoneuroptera with the Orthoptera.

As an instance of the danger of drawing inferences in palæontology from negative evidence, we may quote a sentence from p. 196 of the first volume, where the author states that "The Diplopoda are universally considered the lower of the two in their organization, and it is therefore not surprising to find that no Chilopoda have been found in rocks older than the Tertiary series, while Myriopods with two pairs of legs corresponding to each dorsal plate range back through the entire series of rocks to the Coal-measures." This inference is, of course, completely traversed by the above-mentioned discovery of Carboniferous Chilopoda; and it may be suggested whether the presumed coalescence of two dorsal segments in the Diplopoda and Archipolypoda is not a character in advance of the Chilopoda.

The only essay devoted to Arachnids in the first volume is the one commencing on p. 419, which was originally published for the first time in September 1890. This essay treats of the Palæozoic order Anthracomarti, and of that division of the Pedipalpi known as the Phrynidea; the Scorpions being reserved for a future occasion. The Arachnids differ from both the insects and Myriopods in being represented by an existing order (Scorpions) as far back as the Silurian. Indeed, the only extinct order of the class is the Anthracomarti, which is confined to the Carboniferous, and is regarded as having some points of connection with the Adelarthrosomata, as represented by the *Phalangidæ* ("Harvest-men"), and others with the Pedipalpi, the relationship being on the whole nearer to the latter. They are characterized by their somewhat depressed bodies, in which the abdomen is distinct from the cephalothorax, and consists of a single mass composed of from four to nine distinct joints; while the palpi are short, and do not terminate in pincers or claws. With the possible exception of the Scorpions, these appear to have been the most abundant of the Carboniferous Arachnids, and were represented by a number of genera; those described in the essay before us being arranged in two families and six genera. In the Phrynidean section of the Pedipalpi, containing the Spider-Scorpions, Mr. Scudder describes a new Carboniferous genus, *Græophonus*, besides giving further characters of a previously-described species of *Geralinura*, whose nearest living ally is *Thelyphonus*, of the tropical regions of Asia, America, and Australia.

Passing to the second volume, on the Tertiary insects, of which only a very brief notice can be given, we may touch upon a few points mentioned by the author in the introduction. One of the most noteworthy circumstances to which he refers is the extraordinary profusion in which insect remains have been preserved in some of the Tertiary lake-basins of North America, this being especially the case with the Florissant basin of Colorado, belonging to the Oligocene epoch. Not less remarkable is the fact that in "hardly a single instance has the same species been found at two distinct localities"; and this not only when the localities are separated by hundreds of miles, but even when they are comparatively near. The author considers that this peculiarity may be explained by the absence of exact synchronism between any of the insectiferous beds, and he is thus led to infer that insects will probably afford very valuable aid in determining geological horizons, the modification of species having progressed much more rapidly than is the case with plants.

Another point to which attention is directed relates to the extraordinary number of forms known only by a single specimen; the author stating that, in beds whence thousands of insects have been obtained, every third or fourth specimen will prove to be a new form. The interest of these investigations is enhanced by the discovery that a considerable proportion of the Tertiary insects must be referred to extinct genera; the author considering that a large number of the species he has placed in existing genera will eventually have to be removed to new ones. We trust, however, that Mr. Scudder will not burden the science with more new terms than are absolutely essential; more especially since, if he favours us with a new edition of his "Nomenclator," he will have the additional labour of recording them a second time.

Following the introduction there is a chapter devoted to the American localities where fossil Tertiary insects are most abundantly found. In addition to the Florissant basin of Colorado, there are deposits of approximately the same age on the White River in Colorado and Utah, as well as on the Green River in Wyoming. Less productive spots include a town in Wyoming, rejoicing in the appropriate name of "Fossil," as well as various places in British Columbia, Ontario, and Pennsylvania. There are also a certain number of insects—mostly Coleoptera—from Pleistocene or recent bone-caves and other superficial deposits.

By far the greater bulk of the enormous collection with which the author has had to deal was obtained from the Florissant basin; and it is to these alone that our few remaining observations will refer. The mass of material from these deposits is, however, so vast that in the present volume (large as it is) the author has found it possible to deal only with the Arachnids, Myriopods, and the Neuroptera, Hemiptera, and Orthoptera among the true insects. Some introductory remarks are, however, given as to the relative proportions in which the Lepidoptera, Hymenoptera, Diptera, and Coleoptera, are represented in these beds.

The total number of specimens of insects obtained from Florissant during the labours of a single summer is estimated to be more than double that obtained during thirty years at the celebrated European locality, Œningen.[1] A remarkable difference occurs between the relative number of species of the different orders of insects found at the two places. Thus, while at Œningen the Diptera are less than 7 and the Hymenoptera less than 14 per cent. of the whole; at Florissant they reach respectively 30 and 40 per cent. On the other hand, while the Œningen Coleoptera form nearly half of the whole number, at Florissant they fall to 13 per cent. The great percentage of Hymenoptera is due to the prodigious number of ants; in which respect, as also in the small proportion of beetles, the fauna agrees better with that of Radaboj, in Croatia, to which it likewise approximates more closely in age. It would take too much space to enter into the details of the proportions in which the various families of the different orders are represented in these beds; but it appears that, with the exception of the Lepidoptera, nearly every prevalent family may be demonstrated to have been in existence at that epoch. Among the beetles, about three-fifths belong to the normal series, and the remaining two-fifths to the weevils; water-beetles being unexpectedly scarce. Lepidoptera are rare, only eight species of butterflies, all referable to different and extinct genera, and about the same number of moths being at present known. It is of especial interest to note that, while seven of the eight butterflies belong to the *Nymphalidæ*, no less than two of these are referable to the sub-family *Libytheinæ*, the members of which, although found in every quarter of the globe, are fewer in number than many other groups, consisting only of ten species, referable to the single genus *Libythea*. It is, therefore, a legitimate inference that the *Libytheinæ* have been on the wane since the Oligocene or some later Tertiary epoch. Some writers, it may be mentioned, regard *Libythea* as the representative of a family rather than a sub-family.

In taking leave of the author, we congratulate him on the patience and perseverance which have carried him thus far through a task of unusual magnitude and difficulty, and hope ere long to have the pleasure of welcoming its completion. With the widely-scattered literature of palæontology ever increasing, the importance and value of monographs like the present, where the whole subject is collectively treated by a master-hand, cannot be too highly estimated.

R. Lydekker.

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[1] Œningen is situated on the right bank of the Rhine, between Shaffhausen and Constance, and is in Baden, and not, as the author states on p. 26, in Bavaria.

STATISTICS OF POPULATION AND DISEASE.

Studies in Statistics. By George Blundell Longstaff. (London: Edward Stanford, 1891.)

"STUDIES" is a title appropriate to these somewhat detached investigations concerning at least three different classes of subject. The first few chapters, relating to vital statistics, are described by the author as "of an introductory and elementary character"; though the discussion which is contained in one of them, on the fluctuation of death-rates, varying according to the cause of death, does not appear to us so very rudimentary.

A great part of the book is occupied with the "growth of population": whether by "natural increase" or immigration. England and Wales alone add 1000 a day to the population of the world. "Over and above reserve men who fill up the gaps caused by death, a fresh regiment at full war strength daily marches to the front." To what quarters are they marching? The answer involves a consideration of intra-migration, as Mr. Longstaff terms the migration between the several divisions of the same kingdom. The inquiry brings into view the relatively slow increase of rural as compared with urban districts—a contrast not peculiar to the United Kingdom.

These and other facts, extracted from records accessible to all, are not absolutely new to the student of Statistics. Yet they excite gratitude, almost as much as if they were wholly due to the author; enhanced as they are by the wealth of his inferences and the luxury of his illustrations.

The statistics of the growth of America are less familiar to the English reader. By a careful analysis of the American census, Mr. Longstaff estimates that nearly one-third of the whole population (almost 28 per cent.) is "foreign"; considering as foreign not only those born of foreign parents (whether in America or elsewhere), but also *half* of those who, though native-born, have *one* foreign parent. This heterogeneity of population constitutes a grave social and political danger; particularly in the case of the rapidly-growing coloured population. In more than one sense, says the author, a black cloud may be said to hang over the future of the Republic.

Canada is not equally threatened by the dangers arising from a mixed population. Yet, even in Canada, the fact that the persons of French race form about a third part of the population, and increase more rapidly than any other known people, "cannot but be a source of anxiety and possible trouble in the future." The solidity of our Australian colonies is more perfectly satisfactory.

Surveying the British Empire, the writer exhibits the growth of the colonies relatively to the mother country during the last half-century. Whereas the ratio between the populations of the colonies and the United Kingdom was 7:100 in 1841, it had become 21:100 in 1881. Entertaining the idea of an Imperial Federation, our statistician thus estimates the balance of power in the imagined Federal Parliament. If every 100,000 of white population are entitled to one representative, then 61 per cent. of the Imperial Parliament would be English; the proportions for Scotland and Ireland would be 9 and 12 per cent. respectively.

But the political interest of these estimates must not detain us from what is perhaps the most severely scientific part of the work before us—namely, the investigation of the causes of disease. This medical portion of the volume may, as the author fears, "prove too technical for many readers"; and, perhaps we should add, critics. The student of such statistics must bring much knowledge in order to carry away much. The need of this requisite may be illustrated by one of Mr. Longstaff's examples. Certain of the curves which he traces show a remarkable correspondence between the outbursts of diphtheria and a group of other diseases, amongst which are croup and cynanche maligna. And yet between the two latter diseases and diphtheria the correspondence at some dates is not so close as the suggested theory desiderates. Diphtheria in 1859 rose enormously, while the other diseases did not rise simultaneously, or even fell. But, as we understand the matter, the theory is saved by the surmise that many cases previously ascribed to croup and cynanche maligna, were put down to diphtheria in 1859 and afterwards, when the stir created by letters in the newspapers had excited the attention of observers to the "new disease." This is one of those explanations of figures which an outsider would probably not even have thought of, and the importance of which he is little qualified to estimate.

The "ætiology" of the subject must be left to the expert. The general reader, if he cannot penetrate to the laws of causation, may at least admire the uniformity of results which the author's diagrams exhibit. The nature of some of his observations, and the labour and care which they required, are indicated in the following quotation:—

> "The object of my investigation was . . . [principally] to see whether any, and if so what, relations subsist between diseases believed to be distinct. . . . I accordingly traced eighty-nine curves representing the death-rates per million in England and Wales from as many 'alleged causes.' . . . By a simple application of the law of combinations, it will be found that to compare all these eighty-nine curves two and two together, would involve 3916 operations. Of these I have as yet actually made only 1425."

This comparison of curves representing the fluctuation of death-rates for different diseases forms some of the most beautiful pieces of statistics which we have ever seen. We may allude in particular to the comparison of erysipelas, scarlatina, rheumatism of the heart, and certain other diseases with each other and the variations in the rainfall (Plate xix.). The death-rates are shown to be parallel to each other, not only for different times, but also, in the case of three of the diseases, for different places in all the eleven registration counties of England and Wales. The splendid diagram which exhibits this manifold comparison (Plate xxi.) affords, as the author points out, a good illustration of the value of large numbers in statistical inquiries.

> "The curves for England and Wales exhibit smaller fluctuations than those for sections of the country, and the correspondences between them [between the rise and fall of death-rates for three specified diseases] are in nearly all cases much closer."

Among investigations of which the interest appeals to the mere statistician as distinguished from the medical expert, we may mention the calculation of the frequency with which coincidences between the deaths of both husband and wife from phthisis "might be expected to occur *as a pure matter of chance*, on the hypothesis that phthisis is *not* a communicable disease." By a beautiful application of the calculus of probabilities, the following conclusion is reached:—

> "It is plain, therefore, that, to show any substantial argument for the existence of infection, it would require a much larger collection of cases than has yet been published."

Another inquiry which the general reader will follow with peculiar interest relates to hydrophobia. The statistics suggest laws very different from popular beliefs. The paucity of the observations, however, necessitates caution; which Mr. Longstaff does not fail to inculcate. It is not his least merit that he instils what may be called the logic of statistics by occasional precept, as well as by repeated examples.

OUR BOOK SHELF.

The Best Books: A Contribution towards Systematic Bibliography. By William Swan Sonnenschein. Second Edition. (London: Swan Sonnenschein and Co., 1891.)

THE idea of this "contribution towards systematic bibliography" is excellent, and has been excellently carried out. When interest in a subject has been excited, the first question of the student, of course, is, Who are the best and most recent authorities on the matter? The question is by no means always easily answered, for as yet there are few good subject-indexes, and the most valuable of them are not within the reach of everyone. The present volume may almost be said, for ordinary practical purposes, to have solved the problem. Mr. Sonnenschein has not attempted anything so ambitious as a philosophic classification of the sciences. He has worked out his scheme on what he properly calls "a common-sense plan," grouping books first into large classes, then breaking them up into sections, sub-sections, and paragraphs—"with the result of obtaining all the literature of one subject in one list, and that of outlying subjects close at hand." He begins with theology, next takes mythology and folk-lore, then philosophy, society (including many different branches), geography, history, archæology, and so on, until all important departments of knowledge have been included. No one who has occasion to use the book will have the slightest difficulty in understanding the principle, or in finding the particular subdivision presenting the facts of which he is in search. The new edition contains the titles of twice as many books as the first edition (50,000 as against 25,000); and, so far as we have been able to examine them, they seem to have been admirably selected. Here we have to do only with the scientific part of the work; and, considering how vast is the material from which Mr. Sonnenschein had to choose his lists of scientific treatises, he may be congratulated on the manner in which his task has been accomplished. For the most part, he refers only to books that are in print, and easily obtainable. The very best books he has "asterisked," and in every case he gives the dates of the first and last editions, with the price, size, and publisher's name. Two separate indexes—one, a list of authors, with the titles of their works; the other, a list of subjects—add greatly to the value of the compilation.

The Fairyland Tales of Science. By the Rev. J. G. McPherson. Second Edition. (London: Simpkin, Marshall, and Co., 1891.)

THIS volume consists of a number of papers which appeared originally in various periodicals. The author does not profess to embody in them the results of independent research. His object is to give to readers who may not have access to recent scientific authorities "an accurate and at the same time interesting account of the remarkable discoveries in science during the last decade." This object he attains. His style is clear and straightforward, and, without being "sensational," he knows how to present facts and principles in a way that is likely to arrest attention and awaken curiosity. Among the subjects dealt with are the formation of dew, the colour of water, dust and fogs, lightning, sun-spots, after-glows, the enumeration of organisms in air, micro-organisms in water, and characteristics of deep-sea fishes. The first edition was issued about two years ago. In the present edition the author has added a few notes to bring the facts up to date.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

County Councils and Technical Education.

YOUR article of April 30 (vol. xliii. p. 602) is scarcely fair to the London County Council.

When you allege that the Council "*have 'grabbed' a fund, ear-marked for educational purposes,*" you assume the question at issue. The only way in which the fund in question is "ear-marked" for educational purposes is by a clause in the Act which gives each Council a discretionary power to apply the fund either to those purposes or to other purposes, as they choose.

London, which, as proved by Mr. Goschen, is exceptionally rated, has come badly off in the general scramble for Imperial doles which are devoted to the alleviation of rates; and if the representatives of London ratepayers treat this additional dole out of the beer and spirit duties as a make-up for their comparatively small share of other doles, they are doing not only what the law allows, but what equity justifies.

I believe, however, that amongst those who voted against the plan proposed by the Committee of the Council there are many who would not be unwilling to see the money devoted to education, if any well-considered and reasonable plan were proposed for this purpose.

But there are several questions which have to be answered before this can be done properly.

What do the promoters of "technical education" mean by that term? It is not to be the teaching of the elementary school; it is not to be the training of the workshop; but between these two extremes all is uncertain. The counties say, "instruction in the elements of farming"; the London County Council Committee says, "Polytechnics"; the statute says, "whatever the authorities at South Kensington define it to be." Educational reformers generally, so far as I can judge, mean by it all or any forms of secondary education, *i.e.* of the education which carries forward the work of the elementary school, and brings the pupil nearer to the business of life. But we need to be a good deal more precise before we establish a precedent and a practice.

Then, again, is it wise for the London County Council, which has work enough on its hands in looking after the physical condition of this great City, to take upon itself a task for which it is in no way fitted, and which was not contemplated when it was elected? Is it wise to muddle administration by first intrusting one part of education to one elective body—viz. the School Board—and then intrusting another part of it to a different elective body chosen for a different purpose?

Whilst such questions as these remain unanswered, the London Council exercises a wise discretion in not committing itself to any scheme for appropriating this fund, the offspring of a legislative fluke, to any special and permanent object.

You speak, as persons in general speak, of the London County Council as one amongst other County Councils. The name County Council is a misnomer which leads to constant errors. The London County Council has little or nothing in common with the bodies which have taken the place of the old magistracy in most districts. It is really the chief Town Council of the largest city or aggregation of cities in the world, and the rules and reasoning which, under the ill-drawn and ill-digested Local Government Act, are applied to both, are often singularly inappropriate. Calling London a county is the parent of endless mistakes; and to abuse the London Council because it is not acting in the same way as the Councils of counties seem disposed to act is no less confused than unfair. T. H. FARRER.

May 5.

The Alpine Flora.

I HAD not intended to continue the discussion on this subject, but Prof. Henslow's last letter calls for a few remarks. My argument, summed up, is as follows:—

(1) Alpine plants as a class show certain characters, *e.g.* dwarfing and compact growth.

(2) These characters are advantageous to them, or are correlated with such as are advantageous.

(3) Although dwarfing, &c., may be produced as the direct result of environment (*e.g.* poor soil), there is normal variability in respect to size, time of maturing, &c.

(4) When in cultivation those plants are selected which show a natural tendency to dwarfing, &c., it is found that the character is inherited; and in this way, dwarfed, early-maturing, and other peculiar races can be produced.

(5) On the other hand, when plants have been dwarfed from growing in poor soil, or otherwise as the result of environment acting directly upon them, there *appears* to be no evidence to show that the peculiarity is inherited.

(6) Supposing natural selection to be the only factor, it is fully competent, working on the normal variability, to produce the results observed, *so far as they are hereditary*. At least, so it seems to me.

To illustrate the point, take *Mertensia* again. In Colorado, *M. sibirica* grows in ravines, &c., by creeks; it could not possibly grow in the same way above timber-line, with its tall stems and abundant foliage. Yet it gains much advantage in the creek-bottoms from its height and rank growth; if it were a dwarf, it would be almost or altogether smothered. Above timber-line, on the Sangre de Cristo Range, I found the dwarf species, *M. lanceolata*. Thus we have two species frequenting different situations in the same district: each is fitted for its station; either, removed to the station of the other, could not exist. In Arctic regions, *M. sibirica* has produced a dwarf variety called *drummondii*, which is, I suppose, a first step towards the establishment of a dwarf Arctic species.

Prof. Henslow asks why, if natural selection eliminates tall plants on Alpine summits, it does not also do so lower down? I am not at all clear that it does not, in some cases. For example, why is it that plants growing on exposed sea-shores have a tendency to lie upon the ground or otherwise to evade the violence of the winds? But when a plant is growing among others, it has to compete with them in raising itself into conspicuousness, and any slight disadvantage from exposure to the winds would be more than compensated by the advantage of being able to spread its flowers and foliage in the sunlight and attract insects.

The only plant of any size I found above timber-line on the Sangre de Cristo Range was *Cnicus eriocephalus*, a wonderful great thistle, with bright chrome-yellow flowers, which are visited by humble-bees. But this plant is very prickly and woolly, and its heads are nodding; it is, though it seems paradoxical to say so, a gigantic dwarf.

The splendid *Primula parryi* shows its crimson flowers by creeks at very high altitudes in Colorado; an allied but *very small* species lives above timber-line in the same districts, called *P. angustifolia*. These are true species; *angustifolia* is not starved or frozen *parryi*. Now *P. parryi* is coming into cultivation, it would be interesting to see whether it could be modified by environment in the direction of *angustifolia*, and how far such modification would be inherited.

There are other matters one might discuss, but I think I have already written enough. I merely ask, will Prof. Henslow give a case in which the direct effect of environment *has* produced inherited dwarfing? Will he also show that natural selection cannot produce a dwarfed variety, or that artificial selection has not? T. D. A. COCKERELL.

3 Fairfax Road, Bedford Park, Chiswick, W., April 27.

MR. THISELTON-DYER, in his interesting letter in NATURE (p. 581), does not mention one of the striking characteristics of the Alpine flora—the remarkable brilliancy of the flowers, as compared with those borne by the same or similar species in

England. A comparison of this kind made by the memory is no doubt not severely scientific, but those tourists in Switzerland who are in the habit of observing flowers will probably confirm the statement. Plants grown at high levels in the Alps are, as Mr. Dyer says, above a great screen of aqueous vapour, and I have in my own mind always put down the greater brilliance of Alpine flowers to their getting more sun than in our cloudier climate. It is not, however, solely any alteration in the actual effects of the solar rays, caused by this absence of aqueous vapour, that makes the colours of Swiss flowers so bright. The same, or, I should assert from memory, even greater, brilliancy, will be found in Arctic and sub-Arctic Norway by anyone who visits the Throndhjem district and the coast to Hammerfest in June. Western Norway notoriously is one of the moistest parts of Europe; but, on the other hand, it has, broadly speaking, no night at midsummer. It is thus apparently the quantity, and not the quality, of the sunlight that causes the peculiarly vivid colours of Swiss flowers, including those of the pastures from 2000 feet upwards. I have never been in Switzerland in spring, and I cannot therefore judge whether the colours of the flora in the lower districts are also more brilliant than ours; but it will be seen below that Swiss observers find that the high Alpine flora is much more brilliant than the same plants in the lowlands.

Our great national garden at Kew is peculiarly badly situated for the growth of Alpines. The situation is low and foggy, and mild muggy weather alternates with night frosts. Above all, the smoke pall of London is peculiarly destructive in connection with the other disadvantages of the site. Alpine plants, as Mr. Dyer shows, are, in their natural state, at rest under a cloak of snow during the winter. The least warmth, however, starts them into growth, and the marvellously rapid flowering of many kinds in the ooze on the melting of a snow-bed, is one of the most curious sights of the Alps. The Kew climate (and the general English one too, though to a lesser degree) keeps the plants in growth in winter. Then fogs, smoke, and damp collect on the young growth. These enemies are peculiarly liable to attach themselves to the numerous sorts with hairy or woolly leaves. Then follow night frosts, and the young growth perishes.

The application of these remarks is, that it does not follow that, because cold frames are necessities in the culture of Alpines at Kew, they should be used elsewhere in England. There has been a long discussion recently on this very point in the gardening papers, and the general belief appears to be, that given a fairly dry climate cold frames are *injurious*, because they excite and keep plants in growth when they should be at rest. A sheet of glass suspended over a plant in the open air, so as to shoot off our superfluous rain and to keep off some of our fog, appears to be much better, for premature growth is not stimulated. Alpines should so far as practicable be kept as dry as we can in winter, by drainage, light soil, &c. Then when growth commences, say in March, they should be well watered each day (unless it is raining), early in the morning. The plentiful moisture thus supplied to some degree takes the place of the melting snow, and it has dried off before the evening frosts seize upon the leaves. The plants thus can grow freely in the day because they are surrounded with a moist atmosphere, and they are kept "stocky" (in gardeners' phrase) by the cold at night, just as they are in fact on the Alps. This is the plan recommended by that great authority M. H. Correvon, of the Jardin Alpin d'Acclimatation, Geneva. In the drier climate of that city, M. Correvon replaces the snow blanket of the Alps by pine boughs fastened closely over his Alpines. In England this would, I fear, only make the plants rot. It does not follow that, because many plants in frames at Kew grow long and straggling and lose their natural habit, they do so in England generally in the open air. The changes in the habits of Alpines are largely due to changes in soil. For instance, the Edelweiss (*Gnaphalium leontopodium*) grows perfectly freely from seed anywhere about London, but the flowers lose their compactness. I am told, however, that if plenty of lime is added to the soil they become as compact and close as in Switzerland.

In "Les Plantes des Alpes" (Geneva, Jules-Carey) M. Correvon very fully explains his views, formed, after great practical experience, on the conditions of the Alpine flora. Your space will not allow me to make many quotations from a work of the utmost interest both theoretical and practical, but the following bears on my point as to the brilliant colours of Alpine flowers:—

"Ces végétaux sont 'reine Kinder des Lichtes,' comme les a appelés un poète allemand; on ne trouve pas de champignons dans les Alpes, ni aucune plante qui n'appartienne franchement au domaine de la lumière. Aussi les espèces de nos plaines qui se trouvent transportés là-haut sont-elles parées de couleur bien plus vives, bien plus pures qu'elles ne sont chez nous."

M. Correvon gives a number of instances in support of this, which I will not quote here. In conclusion, is Mr. Dyer correct in thinking that the soil in the high Alps is permanently frozen with the exception of a slight film on the top? I am aware that when you get to considerable elevations the subsoil is frozen. For instance, I was told that the reason for the well-known mortuary on the Great St. Bernard was that bodies could not be buried there. But a great many of the flowers generally called Alpines grow below the tree limit of 6000 or 6500 feet, and few are to be found above 8000 feet. If the subsoil on the higher Alps is frozen, it would not apparently be so where trees grow, and it would be interesting to know the line of subterranean frost, and at what depths below the surface it is permanent at various elevations. J. Innes Rogers.

Chislehurst, April 27.

Co-adaptation.

I do not propose to extend the discussion on this subject beyond the present communication, but I cannot refrain from calling attention to the remarkable discrepancy in the position taken by Dr. Romanes in his last letter (April 23, p. 582), and that in his former communication (March 26, p. 489), in which he says:—"I do not . . . hold myself responsible for enunciating Mr. Herbert Spencer's argument, which the quotation sets forth. I merely reproduced it from him as an argument which appeared to me valid on the side of 'use inheritance.' For not only did Darwin himself invoke the aid of such inheritance in regard to this identical case . . . &c." If words have any meaning, this implies that Dr. Romanes agrees with Darwin in regarding this case as one in which "use inheritance" played a part. Now, after I have endeavoured to show that this supposed case of co-adaptation can be explained without the aid of "use inheritance" at all, Dr. Romanes says that there is no difference of opinion on this point between us. I can only say that I am very glad to learn this admission on his part, but why did he quote the argument from Herbert Spencer as "valid on the side of 'use inheritance,'" if he did not believe it to be a case of true co-adaptation? R. Meldola.

High and Low Level Meteorological Observatories.

I have read with much interest your article of the 11th inst. on the results obtained by simultaneous observations in the meteorological observatories at the base and at the summit of Ben Nevis. Ben Nevis rises to a height of only 4370 feet above the sea, and yet we find that the comparison of these observations gives results of a kind that could not be obtained from any number of stations all on the same level. Might we not hope for still more valuable results from similar observatories placed at the base and the summit of Etna and Teneriffe? Etna is 10,870 feet high, and Teneriffe 12,200. These would be better than any Alpine stations, because of their perfect isolation.

Belfast, April 25. Joseph John Murphy.

An "International Society."

An institution with the grandiloquent title of "The International Society of Literature, Science, and Art," which appears now to be largely touting for subscriptions, publishes in its prospectus a list of the "Honorary Council," among whom appears "Professor Flower." As I am the only person in this country to whom such a description could be applicable, and as many of my friends have inquired of me whether I have really given my support to the institution, I wrote to the secretary to inquire by what authority the name appeared, and received the following reply, which needs no comment:—

"Sir,—We beg to acknowledge the receipt of your favour of Saturday. The gentleman to whom you refer is the well-known Professor Ogilby Flower, of New York. I am sorry the coincidence should have caused you any annoyance. In future printings of our prospectus the Christian name shall be inserted, so that no misunderstanding may exist."

Although this letter was dated March 9 last, I find that the prospectus continues to be issued unchanged, otherwise I should not have cared to trouble you with what may appear a small personal matter. I may mention that there are other names upon the list which present as great or even greater difficulties of identification. W. H. Flower.

British Museum (Natural History), May 2, 1891.

ON SOME POINTS IN THE EARLY HISTORY OF ASTRONOMY.[1]

II.

WE have next to deal with the astronomical relations of the horizon of any place, in connection with the worship of the sun and stars at the times of rising or setting, when of course they are on or near the horizon; and in order to bring this matter nearer to the ancient monuments, we will study this question for Thebes, where they exist in greatest number and have been most accurately described.

The French and Prussian Governments have vied with each other in the honourable rivalry of mapping and describing the monuments. The French went to Egypt at the end of the last century, while the Scientific Commission which accompanied the army, a Commission appointed by the Institute of France, published a series of volumes containing plans of all the chief temples in the valley of the Nile, as far as Philæ.

In the year 1844, after Champollion had led the way in deciphering the hieroglyphics, we became almost equally indebted to the Prussian Government, who also sent out a Commission to Egypt, under Lepsius, which equalled the French one in the importance of the results of the exploration; in the care with which the observations were made, and in the perfection with which they were recorded. Hence it is that in attempting to get information from ancient temples it is wise to study the region round Thebes, where the information is so abundant and is ready to our hand.

We have then to consider an observer on the Nile at Thebes, and to adjust things properly we must rectify the globe to the latitude of 25° 40′, or, in other words, incline the axis of the globe at that angle to the wooden horizon.

It will be at once seen that the inclination of the axis to the horizon is very much less than in the case of London. Since all the stars which pass between the North Pole and the horizon cannot set, all their apparent movement will take place above the horizon. All the stars between the horizon and the South Pole will never rise. Hence, stars within the distance of 25° from the North Pole will never set at Thebes, and those stars within 25° of the South Pole will never be visible there. At any place the latitude and the elevation of the pole are the same. It so happens that all these places with which archæologists have to do in studying the history of early peoples, Chaldæa, Egypt, Babylonia, China, Greece, &c., are all in middle latitudes, therefore we have to deal with bodies in the skies which do set and bodies which do not, and the elevation of the pole is neither very great nor very small. In each different latitude the inclination of the equator to the horizon as well as the elevation of the pole will vary, but there will be a strict relationship between the inclination of the equator at each point and the elevation of the pole. Except at the poles themselves the equator will cut the horizon due east and due west. Therefore everything to the north of the equator which rises or sets will cut the horizon between the east or west point and the north point; those bodies which do not set will of course not cut the horizon at all.

The sun and stars near the equator, in such a latitude as that of Thebes, will appear to rise or set at no very considerable angle from the vertical; but when we deal with stars rising or setting near to the north or south points of the horizon they will seem to skim along the horizon instead of rising directly.

Now it will at once be obvious that there must be a strict law connecting the position of the sun or a star with its place of rising or setting. Stars at the same distance from the celestial pole or equator will rise or set at the same point of the horizon, and if a star does not change its place in the heavens it will always rise or set in the same place. Here it will be convenient to introduce one or two technical terms: we generally define a star's place by giving, as one ordinate, its distance in degrees from the equator: this distance is called its declination. Further, we generally define points on the horizon by dividing its whole circumference into 360°, so that we can have *azimuths* of 90° from each pole to the east and west points. We also have *amplitudes* from the east and west points towards each pole. We can say then that a star of a certain declination will rise or set at such an azimuth; or at such an amplitude. This will apply to both north and south declinations.

The following table gives the amplitudes of rising or setting (north or south) of celestial bodies having declinations from 0° to 64°; bodies with higher declinations than 64° never set at Thebes if they are north, or never rise if they are south, as the latitude (and therefore the elevation of the pole) there is nearly 26°.

Amplitudes at Thebes.

Declination.	Amplitude at Thebes.		Declination.	Amplitude at Thebes.	
°	°	′	°	°	′
0	0	0	33	37	11
1	1	7	34	38	21
2	2	13	35	39	31
3	3	20	36	40	42
4	4	26	37	41	53
5	5	33	38	43	5
6	6	40	39	44	17
7	7	47	40	45	30
8	8	53	41	46	43
9	9	59	42	47	56
10	11	6	43	49	10
11	12	13	44	50	25
12	13	20	45	51	41
13	14	27	46	52	57
14	15	34	47	54	14
15	16	41	48	55	32
16	17	49	49	56	51
17	18	56	50	58	12
18	20	3	51	59	34
19	21	10	52	60	58
20	22	17	53	62	23
21	23	25	54	63	51
22	24	33	55	65	21
23	25	41	56	66	54
24	26	49	57	68	31
25	27	58	58	70	12
26	29	6	59	71	59
27	30	15	60	73	55
28	31	23	61	76	1
29	32	32	62	78	25
30	33	41	63	81	19
31	34	51	64	85	42
32	36	1			

This being premised, we now pass to the yearly path of the sun, with a view of studying the relation of the various points of the horizon occupied by the sun at different times in the year. In the very early observations that were made in Egypt, Chaldæa, and elsewhere, when the sun was considered to be a god who every morning got into his boat and floated across space, there was no particular reason for considering the amplitude at which the boat left, or came to, shore. But a few centuries showed that this rising or setting of the sun in widely varying amplitudes at different parts of the year depended upon a very definite law. We now, of course, more fortunate than the early Egyptians, know exactly what this law is. We saw in the last lecture that not many years ago Foucault gave us a means of demonstrating the fact that the earth rotates on its axis. We have also a perfect method of demonstrating that the earth not only rotates on its axis once a day, but that it moves round the sun once a year, an idea which was undreamt of by the ancients. As a pendulum shows us the rotation, so the determination of the aberration of light demonstrates for us the revolution of the earth round the sun.

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[1] From shorthand notes of a course of lectures to working men delivered at the Museum of Practical Geology, Jermyn Street, in November 1890. The notes were revised by me at Aswan during the month of January. I have found, since my return from Egypt in March, that part of the subject-matter of the lectures has been previously discussed by Herr Nissen, who has employed the same materials as myself. To him, therefore, so far as I at present know, belongs the credit of having first made the suggestion that ancient temples were oriented on an astronomical basis. His article is to be found in the *Rheinisches Museum für Philologie*, 1885. Continued from vol. xliii. p. 563.

We have, then, the earth endowed with these two movements—a rotation on its axis in a day, and a revolution round the sun in a year. To see the full bearing of this on our present inquiry, we must for a time return to the globe or model of the earth.

To determine the position of any place on the earth's surface we say that it is so many degrees distant from the equator, and also so many degrees distant from the longitude of Greenwich: we have two rectangular coordinates, latitude and longitude. When we conceive the earth's equator extended to the heavens, we have a means of determining the positions of stars in the heavens exactly similar to the means we have of determining the position of any place on the earth. We have already defined distance from the equator as north or south declination in the case of a star, as we have north latitude or south latitude in case of a place on the earth. With regard to the other co-ordinate, we can also say it is at a certain distance from our first point of measurement, whatever that may be, along the celestial equator; speaking of the stars we call this distance right ascension, as speaking of matters earthy we measure from the meridian of Greenwich and call this distance longitude.

The movement of the earth round the sun is in a plane which is called the plane of the ecliptic, and the axis of rotation of the earth is inclined to that plane at an angle of something like $23\frac{1}{2}^{\circ}$. We can if we choose use the plane of the ecliptic to define the positions of the stars as we use the plane of the earth's equator. In that case we talk of distance above the ecliptic as celestial latitude, and along the ecliptic as celestial longitude. The equator, then, cuts the ecliptic at two points: one of these is chosen for the start-point of measurement along either the equator or the ecliptic. It is called the first point of Aries.

We have, then, two systems of co-ordinates, by each of which we can define the position of a star in the heavens: equatorial co-ordinates dealing with the earth's equator, ecliptic co-ordinates dealing with the earth's orbit. Knowing that the earth moves round the sun once a year, the year to us moderns is defined with the most absolute accuracy. In fact, we have three years: we have a sidereal year—that is, the time taken by the earth to go through exactly 360° of longitude; we have what is called the tropical year, which indicates the time taken by the earth to go through not quite 360°, to go from the first point of Aries till she meets it again; and since the equinoctial point advances to meet the earth, we talk about the precession of the equinoxes; this year is the sidereal year minus twenty minutes; then there is also another year called the anomalistic year, which depends upon the movement of the point in the earth's orbit where the earth is nearest to the sun; this is running away, so to speak, from the first point of Aries, instead of advancing to meet it, so that in this case we get the sidereal year plus nearly five minutes.

The angle of the inclination of the earth's plane of rotation to the plane of its revolution round the sun, which, as I have said, is something like $23\frac{1}{2}^{\circ}$, is called *the obliquity of the ecliptic*. This obliquity is subject to a slight change; 6000 years ago it was over 24°.

In order to give a concrete idea of the most important points in the yearly path of the sun round the earth, I have here four globes representing the earth, with another globe in the middle representing the sun, showing the four practically opposite points of the earth's orbit, in which the north pole of the axis is most inclined to the sun; the north pole of the axis is most inclined away from the sun; and the two opposite and intermediate points where the axis is not inclined to or from the sun, but is at right angles to the line joining the earth in these two positions.

A diagram (Fig. 6) shows what will happen under these conditions. If we take the two points at which the axis, instead of being inclined towards the sun, is inclined at right angles to it, it is perfectly obvious that we shall get a condition of things in which the movement of the earth on its axis will cause the dark side of the earth and also the light side represented by the side nearest to the sun both being of equal areas, to extend from pole to pole; so that any place on the earth rotating under those conditions will be brought for half a period of rotation into the sunlight, and be carried for half a period of the rotation out of the sunlight; the day, therefore, will be of the same length as the night, and the days and nights will therefore be equal all over the world.

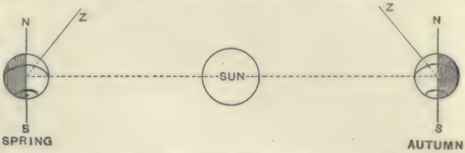

FIG. 6.—Diagram showing the equality of the sun's zenith distance at the two equinoxes. N, north pole of the earth; S, south pole; Z, zenith of Greenwich.

We call that the period of the equinoxes; the nights are of the same length as the day in both these positions of the earth with regard to the sun.

But in Fig. 7 we have a very different condition. Here the north pole is inclined at the greatest angle of $23\frac{1}{2}^{\circ}$ towards, and away from, the sun. If I take a point very near the north pole, that point will not, in summer, be carried by the earth's rotation out of the light, and a part equally near the south pole will not be able to get into it. These are the conditions at and near two other points called the solstices.

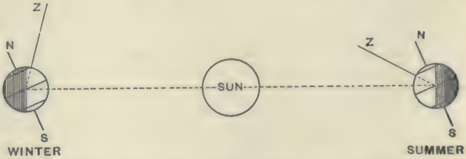

FIG. 7.—Diagram showing the variation of the sun's zenith distance from solstice to solstice. N, north pole of the earth; S, south pole; Z, zenith of Greenwich.

In each of these globes I have placed a wire to represent the overhead direction from Jermyn Street, London, and if I observe the angle between this direction of the zenith to the sun in winter I get a considerable one; but if I take the opposite six-monthly condition and take the same zenith point, I get a very small angle. In other words, under the first condition the sun will be far from the zenith of Jermyn Street, we shall have winter; and in the other condition the sun will be as near as it can be to the zenith of Jermyn Street, we shall have summer. These two points represent the two points in the earth's orbit at which the sun has the highest declination north or south. With the greatest north declination the sun will come up high, appear stationary for a day or two, as it does at our summer solstice, and then go down again; at the other point, when it has the greatest southern declination, it will go down to the lowest point, as it does in our winter, stop, and come up again—that is, the sun will stand still, and the Latin word solstice exactly expresses that idea. We have then two points in the annual revolution of the earth round the sun at which we have equal altitudes of the sun at noon, two others when the altitude is greatest and least. We get the equal altitudes at the equinoxes and the greatest and the least at the solstices. These altitudes depend upon the change of the sun's declination. The change of declination will affect the azimuth and amplitude of the sun's rising and setting, this is why the sun sets most to the north in summer and most to the south in winter. At the equinoxes the sun has always 0° Decl., so it rises and sets due east and west all over the world. But at the solstices it has its greatest declination of 23½° N. or S.; it will rise and set therefore far from the east and west points; how far, will depend upon the latitude of the place we consider. The following are approximate values:

Latitude of place. °	Amplitude of sun at solstice. °	′
25	26	5
30	27	24
35	29	8
40	31	21
45	34	40
50	38	20
55	44	0

At Thebes, representing Egypt, we find that the sun's azimuth at the summer solstice will be 26° N. of E. at rising, and it will be 26° N. of W. at setting.

These solstices and their accompaniments are among the striking things in the natural world. In the winter solstice we have the depth of winter, in the summer solstice we have the height of summer, while at the equinoxes we have but transitional changes; in other words, while the solstices point out for us the conditions of greatest heat and greatest cold, the equinoxes point out for us those two times of the year at which the temperature conditions are very nearly equal, although of course in the one case we are saying good-bye to summer and in the other to winter. To people who live in tropical or sub-tropical countries a summer solstice is a very much more definite thing than it is to us. In Egypt the summer solstice was paramount, for it heralded the rise of the Nile. Next came the autumnal equinox, for it marked the height of the inundation.

Did the ancients know anything about these solstices and these equinoxes? That is one of the questions which we have to discuss. Dealing with the monumental evidence in Egypt alone, the answer is absolutely overwhelming. The evidence I propose to bring before you consists of that afforded by some of the very oldest temples that we know of in Egypt. Among the most ancient and sacred fanes in Egypt was one at Abydos, which, the tradition runs, was built by the Shosou-Hor or servants of Horus (therefore sun-worshippers) before the time of Menes; Menes, as we have seen, having reigned at a date certainly not less than 4000, and possibly 5000 years B.C.

First a word as to the general plan of a temple such as we find it in Egypt. They may be arranged architecturally into two main groups. Edfou is the most perfect example of one of the first group, characterized by having a pylon consisting of two massive structures right and left of the entrance, which are somewhat like the two towers that one sometimes sees on the west front of some of our English cathedrals. The Temple of Ramses II. in the Memnonia at Thebes is another example (Fig. 8).

From the entrance-pylon the temple goes stretching along through various halls of different sizes and details until at last at the extreme end of the temple what is called the Sanctuary, Naos, or Holy of Holies, is reached. The end of the temple at which the pylons are situated is open, the other is closed. These lofty towers, and indeed the walls, are sometimes covered with the most wonderful drawings and hieroglyphic figures and records. Stretching in front of the pylons, extending sometimes very far in front, are rows of sphinxes. This principle is carried to such an extent that in some cases separate isolated gates have been built right in front and exactly in the alignment of the temple. At Karnak there really are two such temples back to back, and the distance which separates the outside entrances of both is greater than the distance from Pall Mall to Piccadilly; the great temple covers about twice the area covered by

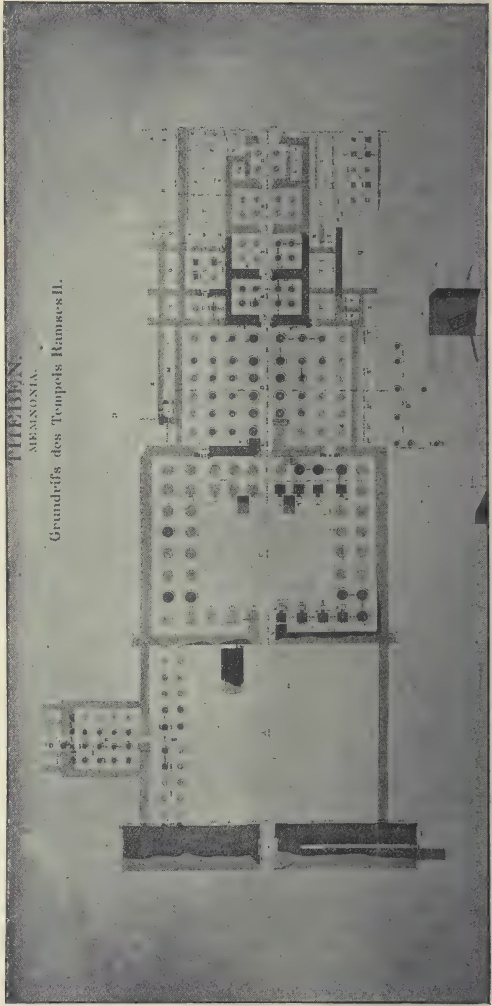

FIG. 8.—Plan of the Temple of Ramses II. in the Memnonia at Thebes (from Lepsius), showing the pylon at the open end, and the sanctuary at the closed one.

St. Peter's at Rome, so that these were temples of a vastness absolutely unapproached in the modern world.

In Denderah we have an example of the second group, in which the massive pylon is omitted. In these the front is entirely changed; instead of the pylon we have now an open front to the temple with columns—the Greek form of temple is approached (Fig. 9).

I shall not have time to get to the astronomical side of the Greek temples in this course of lectures, but I am anxious to take this opportunity to refer to the transition from the Egyptian form of temple to the Greek one. The east front of the Parthenon at Athens very much more resembles the temple of Denderah than it does the early Egyptian temple—that is to say, the eastern front is open; it is not closed by pylons.

In many Egyptian temples, in the progress from one end to the other, one goes through various halls of different styles of architecture and different stages of magnificence. But in the Greek temple this is entirely changed; the approach to the temple was outside, the temple representing, so to speak, the core, almost the Holy of Holies, of the Egyptian temple, and any magnificent approach to it which could be given, was given from the outside. But although they were quite different in their aspects, they were quite similar in their objects. Some Egyptian temples took hundreds of years to build; the obelisks were all in single blocks like that on the Embankment, and all were brought for hundreds of miles down the Nile. A temple meant to the Egyptians a very serious thing indeed.

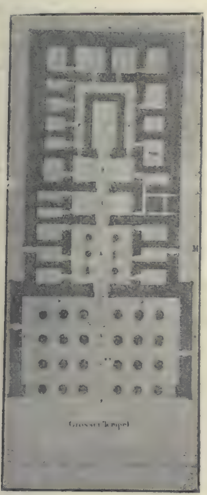

FIG. 9.—Plan of the Temple of Denderah (from Lepsius), showing the absence of a pylon.

So much, then, for a general idea of an ancient temple.

Another point is very striking in these temples, notably in the chief one at Karnak.

From one end of the temple to the other we find the axis marked out by narrow apertures in the various pylons, and many walls with doors crossing the axis. There are 17 or 18 of these limiting apertures, and in the other temple which is back to back to this one we have pylons in exactly the same way limiting the light which falls into the Holy of Holies or the Sanctuary. This construction gives one a very definite impression that every part of the temple was built to subserve a special object, viz. to limit the sunlight which fell on its front into a narrow beam, and to carry it to the other extremity of the temple—into the sanctuary—which extremity was always blocked. There is no case in which the beam of light can pass absolutely through the temple.

The idea is strengthened by considering the construction of the astronomical telescope. Although the Egyptians knew nothing about telescopes, it would seem that they had the same problem before them which we solve by a special arrangement in the modern telescope—they wanted to keep the light pure, and to lead it into their sanctuary, as we lead it to the eyepiece. To keep the light that passes into the eyepiece of a modern telescope pure, we have between the object-glass and the eyepiece a series of what are called diaphragms; that is a series of rings right along the tube, the inner diameters of the rings being greatest close to the object-glass, and smallest close to the eyepiece; these diaphragms must so be made, that all the light from the object-glass shall fall upon the eyepiece, without loss, or reflection by the tube.

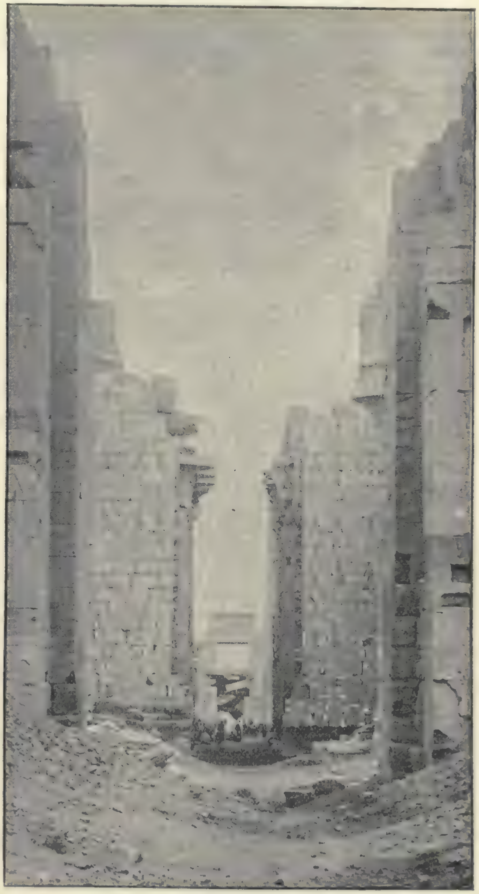

FIG. 10.—The axis of the Temple of Karnak, looking south-east, from outside the north-west pylon (from a photograph by the author).

These apertures in the pylons and separating walls of Egyptian temples exactly represent the diaphragms in the modern telescope. J. NORMAN LOCKYER.

(*To be continued.*)

HERTZ'S EXPERIMENTS.[1]

II.

IN the last article, a general method of measuring the velocity at which a disturbance is propagated was described. It depended on being able to produce a regular succession of disturbances at equal intervals of time. These were made to measure their own velocity by reflecting them at an obstacle. Then, by the interference of the incident and reflected waves, a succession of loops and nodes are produced at intervals of half the distance a disturbance is propagated during the time between two disturbances. It is a general method applicable to any sort of disturbance that takes time to get from one place to another. It has been applied over and over again to measure the rate at which various kinds of disturbance are propagated in solids, liquids, and gases. It was applied in a modified form years ago, to measure the length of a wave of light; and, within the last year, some of the most beautiful experiments on photography ever described are applications of this principle by Herr Wiener and M. Lippmann.

There are three things essential to this experiment: (1) some method of originating waves; (2) some method of reflecting them; (3) some method of telling where there are loops and where there are nodes. We will take them in this order:—

(1) How can we expect to originate electric waves? If, when a body is electrified positively, the electric force due to it exists simultaneously everywhere, of course we cannot expect to produce anything like a wave of electric force travelling out from the body; but if, when a body is suddenly electrified, the electric force takes time to reach a place, we must suppose that it is propagated in some way as a wave of electric force from the body to the distant place. This, of course, assumes that there is a medium which is in some peculiar state when electric force exists in it, and that it is this peculiar state of the medium, which we call electric force, existing in it, that is propagated from one place to another. It must be carefully borne in mind what sort of a thing this is that we call the electric force at any place. It is not a good name—electric intensity would be a better one; but electric force has come so much into use, it is hardly to be expected that it can be eradicated now. Electric force at any place is measured by the mechanical force that would be exerted at the place if a unit quantity of electricity were there. It is not a force itself at all; it is only a description of the condition of the medium at the place which makes electricity there tend to move. The air near the earth is in such a condition that everything immersed in it tends to move away from the earth with a force of about 1·26 dynes for each cubic centimetre of the body, *i.e.* each cubic centimetre tends to move with a force of 1·26 dynes. Now the condition of the air that causes this is never described as volume force existing at the place, though we do describe the corresponding condition of the ether as electric force existing there; and as volume force existing would be a very objectionable description of the condition of the air when, being at different pressures at various levels, it tends to make bodies move with a force proportional to their volume, so electric force existing is a very objectionable description of the condition of the ether, whatever it is, that tends to make bodies move with a force in proportion to their electric charges. We know more about the structure of the air than we do about the ether. We know that the structure of the air that causes it to act in this way is that there are more molecules jumping about in each cubic centimetre near the earth than there are at a distance, and we do not know yet what the structure of the ether is that causes it to act in this remarkable way; but even though we do not know the nature of the structure, we know some of its effects, by means of which we can measure it, and we can give it a name. Although we know very little indeed about the structure of a piece of stressed india-rubber, yet we can measure the amount of its stress at any place, and can call the india-rubber in this peculiar condition "stressed india-rubber." As a matter of fact, we know a great deal more about the peculiar condition of the ether that we describe as "electric force" existing than we do about the "stressed india-rubber"; and there is every reason to suppose that the structure of the ether is, out of all comparison, more simple than that of india-rubber.

When sound-waves travel through the air, they consist of compressions followed by rarefactions, and between them the pressure varies from point to point, so that here we have travelling forward a structure the same as that of the air near the earth, and waves of sound might be described as consisting of a succession of positive and negative "volume forces" travelling forward in the air: this form of expression would no doubt be objectionable, but still if all we knew about the properties of the air near the earth was that it tended to make bodies move away from the earth with a force proportional to their volume, it is quite likely that this condition of affairs near the earth might have been described as the existence of a "volume force" near the earth, and when it was discovered that this action was due to a medium, the air, it would have been quite natural to describe this state of the air as "volume force" existing in it: and then when waves of sound were observed it would be quite natural that they should be described as waves of "volume force," especially if the only way in which we could detect the presence of these waves was by observing the force exerted on bodies immersed in it, which was proportional to their volumes, and which we happen to know is really due to differences of pressure at neighbouring points in the air. We do not know what is the structure of the ether that causes it to exert force on electrified bodies, but we know of the existence of this property, and when it is in this state we say that "electric force" exists in it, and we have certain ways by which we can detect the existence of "electric force," one of which is the production of an electric current in a conductor, and the consequent electrification of the conductor, and if this is strong enough we can produce an electric spark between it and a neighbouring conductor. When a conductor is suddenly electrified, the structure of the ether which is described as electric force existing in it travels from its neighbourhood through the surrounding ether, and this is described as a wave of electric force travelling through the surrounding ether. It is desirable to be quite clear as to what is meant by the term a wave of electric force, and what we know about it. We know that it is a region of ether where its structure is the same as in the neighbourhood of electrified and some other bodies, and owing to which force is exerted on electrified bodies, and electric currents are produced in conductors.

We may, then, reasonably expect that, if it is possible to electrify a body alternately positively and negatively in rapid succession, there will be produced all round it waves of electric force—that is, if the electric force is propagated by, and is due to, a medium surrounding the electrified body, if electrification is a special state of the medium that fills the space between bodies.

(2) The next question is: How can we reflect these waves? In order to reflect a wave, we must interpose in its way some body that stops it. What sort of bodies stop electric force? Conductors are known to act as complete screens of electric force, so that a large conducting sheet would naturally be suggested as the best way to reflect waves of electric force. Reflection always occurs when there is a change in the nature of the medium, even though the change is not so great as to

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[1] Continued from vol. xliii. p. 538.

stop the wave, and it has long been known that, besides the action of conductors as screens of electric force, different non-conductors act differently in reference to electric force by differing in specific inductive capacity. Hence we might expect non-conductors to reflect these waves, although the reflection would probably not be so intense from them as from conductors. Hence this question of how to reflect the waves is pretty easily solved. We are acting still on the supposition that there are waves at all. If electric force exist everywhere simultaneously, of course there will be no waves to reflect, and, consequently, no loops and nodes produced by the interference of the incident and reflected waves.

(3) The third problem is: How can we expect to detect where there are loops and where there are nodes? Recall the effects of electric force. It tends to move electrified bodies. If, then, an electrified body were placed in a loop, it would tend to vibrate up and down. This method may possibly be employed at some future time, and it may be part of the cause of photographic actions, for these have recently been conclusively proved to be due to electric force; but the alternations of electric force from positive to negative that have to be employed are so rapid that no body large enough to be easily visible and electrified to a reasonable extent could be expected to move sufficiently to be visibly disturbed. It is possible that we may find some way of detecting the vibrations hereby given to the electrified ions in an electrolyte; and it has recently been stated that waves originated electrically shake the elements in sensitive photographic films sufficiently to cause changes that can be developed. The other action of electric force is to produce an electric current in a conductor and a resultant electrification of the conductor. Two effects due to this action have actually been used to detect the existence of the wave of electric force sent out by a body alternately electrified positively and negatively. One of these is the heating of the conductor by the current. Several experimenters have directly or indirectly used this way of detecting the electric force. The other way, which has proved so far the most sensitive of all, has been to use the electrification of the conductor to cause a spark across an air-space. This is the method Hertz originally employed. *A priori*, one would not have expected it to be a delicate method at all. It takes very considerable electric forces to produce visible sparks. On the other hand, the time the force need last in order to produce a spark is something very small indeed, and hitherto it has not been possible to keep up the alternate electrifications for more than a minute fraction of a second, and this is the reason why other apparently more promising methods have failed to be as sensitive as the method of producing sparks. If two conductors be placed very close to one another in such a direction that the electric force is in the line joining them, their near surfaces will be oppositely electrified when the electric force acts on them, and we may expect that, if the force be great enough, and the surfaces near enough, an electric spark will pass from one to the other. This is roughly the arrangement used by Hertz to detect whether there are loops and nodes between the originator of the waves and the reflector.

Now arises the problem of how to electrify the body alternately positively and negatively with sufficient rapidity. How rapid is "with sufficient rapidity"? To answer this we must form some estimate of how rapidly we may expect the waves to be propagated. According to Maxwell's theory, they should go at the same rate as light, some 300 million of metres per second, and it is evident that if we are going to test Maxwell's theory we must make provision for sufficiently rapid electric vibrations to give some result if the waves are propagated at this enormous rate. The distance from a node to a node is half the distance a wave travels during a vibration. If we can produce vibrations at the rate of 300 million per second, a wave would go 1 metre during a vibration, so that, with this enormous rate of alternation, the distance from node to node would be 50 cm. We might expect to be able to work on this scale very well, or even on ten times this scale, *i.e.* with alternations at the rate of 30 million per second, and 5 metres from node to node, but hardly on a much larger scale than this. It almost takes one's breath away to contemplate the production of vibrations of this enormous rapidity. Of course they are very much slower than those of light: these latter are more than a million times as rapid; but 300 million per second is enormously more rapid than any audible sound, about a thousand times as fast as the highest audible note. A short bar of metal vibrates longitudinally very fast, but it would have to be about the thousandth of a centimetre long, in order to vibrate at the required rate. It would be almost hopeless by mechanical means to produce electric alternations of this frequency. Fortunately there is an electric method of producing very rapid alternate electrifications. When a Leyden jar is discharged through a wire of small resistance, the self-induction of the current in this wire keeps the current running after the jar is discharged, and recharges it in the opposite direction, to immediately discharge back again, and so on through a series of alternations. This action is quite intelligible on the hypothesis that electrification consists in a strained condition of the ether, which relieves itself by means of the conductor. Just as a bent spring or other strained body, when allowed suddenly to relieve itself, relieves itself in a series of vibrations that gradually subside, similarly the strain of the ether relieves itself in a series of gradually subsiding vibrations. If the spring while relieving itself has to overcome frictional resistance, its vibrations will rapidly subside; and if the friction be sufficiently great, it will not vibrate at all, but will gradually subside into its position of equilibrium. In the same manner, if the resistance to the relief of the strain of the medium, which is offered by the conducting wire, be great, the vibrations will subside rapidly, and if the resistance of the wire be too great, there will not be any vibrations at all. Of course, quite independently of all frictional and viscous resistances, a vibrating spring, such as a tuning-fork that is producing sound-waves in the air which carry the energy of the fork away from it into the surrounding medium, will gradually vibrate less and less. In the same way, quite independently of the resistance of the conducting wire, we must expect that, if a discharging conductor produces electric waves, its vibrations must gradually subside owing to its energy being gradually transferred to the surrounding medium. As a consequence of this the time that a Leyden jar takes to discharge itself in this way may be very short indeed. It may perform a good many oscillations in this very short time, but then each oscillation takes a very very short time. To get some idea of what quantities we are dealing with, consider the rates of oscillation which would give wave-lengths that were short enough to be conveniently dealt with in laboratories. 300 million per second would give us waves one metre long; consider what is meant by 100 million per second. We may get some conception of it by calculating the time corresponding to 100 million seconds. It is more than 3 years and 2 months. The pendulum of a clock would have to oscillate 3 years and 2 months before it would have performed as many oscillations as we require to be performed in one second. The pendulum of a clock left to itself without weights or springs to drive it, and only given a single impulse, would practically cease to vibrate after it had performed 40 or 50 vibrations, unless it were very heavy, *i.e.* had a great store of energy or were very delicately suspended, and exposed only a small resistance to the air. A light pendulum would be stopped by communicating motion to the air after a very few vibrations. The case of a Leyden jar discharge is more like the case of a mass on a spring than the case of a pendulum, because in the cases of the Leyden jar there is nothing quite analogous to the way in which the earth pulls the pendulum: it is the elasticity of the ether that causes the electric currents in the Leyden jar discharge, just as it is the elasticity of the spring that causes the motion of the matter attached to it in the case of a mass vibrating on a spring. It is possible to push this analogy still further. Under what conditions would the spring vibrate most rapidly? When the spring was stiff and the mass small. What is meant by a spring being stiff? When a considerable force only bends it a little. This corresponds to a considerable electric force only electrifying the Leyden jar coatings a little, *i.e.* to the Leyden jar having a small capacity. We would consequently expect that the discharge of a Leyden jar with a small capacity would vibrate more rapidly than that of one with a large capacity, and this is the case. In order to make a Leyden jar of very small capacity we must have small conducting surfaces as far apart as possible, and two separate plates or knobs do very well. The second condition for rapid vibration was that the mass moved should be small. In the case of electric currents what keeps the current running after the plates have become discharged and recharges them again is the so-called self-induction of the current. It would be well to look upon it as magnetic energy stored up in the ether around the current, but whatever view is taken of it, it evidently corresponds to the mass moved, whose energy keeps it moving after the spring is unbent and rebends the spring again. Hence we may conclude that a small self-induction will favour rapidity of oscillation, and this is the case. To attain this we must make the distance the current has to run from plate to plate as short as possible. The smaller the plates and the shorter the connecting wire the more rapid the vibrations; in fact, the rapidity of vibration is directly proportional to the linear dimensions of the system, and for the most rapid vibrations two spherical knobs, one charged positively and the other negatively, and discharging directly from one to the other, have been used. Hertz in his original investigations used two plates about 40 cm. square, forming parts of the same plane, and separated by an interval of about 60 cm. Each plate was connected at the centre of the edge next the other plate with a wire about 30 cm. long, and terminating in a small brass knob. These knobs were within 2 or 3 mm. of one another, so that when one plate was charged positively and the other negatively they discharged to one another in a spark across this gap. An apparatus about this size would produce waves 10 or 12 metres long, and its rate of oscillation would be about 30 million per second. As the vibration actually produced by these oscillators seems to be very complex, the rate of oscillation can only be described as "about" so and so. In a subsequent investigation Hertz employed two elongated cylinders about 15 cm. long and about 3 cm. in diameter, terminated by knobs about 4 cm. in diameter, and discharging directly into one another. Such an oscillator produces waves from 60 to 70 cm. long, and, consequently, vibrations at the rate of between 400 and 500 million per second. Most other experimenters have used oscillators about the same dimensions as Hertz's larger apparatus, as the effects produced are more energetic; but many experiments, especially on refraction, require a smaller wave to be dealt with, unless all the apparatus used be on an enormous scale, such as could not be accommodated in any ordinary laboratory. When we are thus aiming at rapid rates of vibration, it must be recollected that we cannot at the same time expect many vibrations after each impulse. If we have a stiff spring with a small weight arranged so as to give a lot of its energy to the surrounding medium, we cannot expect to have very much energy to deal with, nor many vibrations, and, as a matter of fact, we find that this is the case. The total duration of a spark of even a large Leyden jar is very small. Lord Rayleigh has recently illustrated this very beautifully by his photographs of falling drops and breaking bubbles. We cannot reasonably expect each spark to have more than from 10 to 20 effective oscillations, so that, even in the case of the slower oscillator, the total duration of the spark is not above a millionth of a second. It is very remarkable that the incandescent air, heated to incandescence by the spark, should cool as rapidly as it does, but there is conclusive evidence that it remains incandescent after the spark proper has ceased, and consequently lasts incandescent longer than the millionth of a second. What is seen as the white core of the spark may not last longer than the electric discharge itself, and certainly does not do so in the case of the comparatively very slowly oscillating sparks that have been analyzed into their component vibrations by photographing them on a moving plate. The incandescent air remaining in the path of such discharge is probably the conducting path through which the oscillating current rushes backwards and forwards. Once the air gap has been broken through, the character of the air gap as an opponent of the passage of electricity is completely changed. Before the air gap breaks down, it requires a considerable initial difference of electric pressure to start a current. Once it has been broken down, the electric current oscillates backwards and forwards across the incandescent air gap until the whole difference of electric pressure has subsided, showing that the broken air gap has become a conductor in which even the feeblest electric pressure is able to produce an electric current. If this were not so, Leyden jars would not be discharged by a single spark. All this is quite in accordance with what we know of air that is, or even has lately been, incandescent: such air conducts under the feeblest electric force. All this is most essential to the success of our oscillator. Only for this valuable property of air, that it gives way suddenly, and thenceforward offers but a feeble opposition to the rapidly alternating discharge, it would have been almost impossible to start these rapid oscillations. If we wish to start a tuning-fork vibrating, we must give it a sharp blow: it will not do to press its prongs together and then let them go slowly: we must apply a force which is short-lived in comparison with the period of vibration of the fork. It is necessary, then, that the air gap must break down in a time short compared with the rate of oscillation of the discharge; and when this is required to be at the rate of 400 million per second, it is evident how very remarkably suddenly the air gap breaks down. From the experiments themselves it seems as if any even minute roughnesses, dust, &c., on the discharging surface, interfered with this rapidity of breakdown: it seems as if the points spluttered out electricity and gradually broke down the air gap, for the vibrations originated are very feeble unless the discharging surfaces are kept highly polished: gilt brass knobs act admirably if kept polished up every ten minutes or so. One of the greatest desiderata in these experiments is some method of making sure that all the sparks should have the same character, and be all good ones.

(*To be continued.*)

THE ROYAL SOCIETY SELECTED CANDIDATES.

The following fifteen candidates were selected on Thursday last (April 30), by the Council of the Royal Society, to be recommended for election into the Society. The ballot will take place on June 4, at 4 p.m. We print with the name of each candidate the statement of his qualifications.

William Anderson,

V.-P. Inst. M.E. M.I.C.E. Consulting Engineer, Royal Agricultural Society of England. Pupil of the late Sir William Fairbairn, F.R.S. Member of the firm of Messrs. Courtney and Stephens, Engineers, of Dublin, from 1855 to 1864. President, in 1863, of the Inst. of Civil Engineers of Ireland, to which Society he communicated important papers:—"On the Theory of Braced Girders;" "The Strength of Railway Bridges of Small Span, and the Cross-beams of Large Bridges;" and other subjects. Between 1872 and 1885, communicated many important papers to the Inst. of Civil Engineers, *e.g.*, "Experiments on Sugar Manufacture, in Upper Egypt, by the Sulphurous Acid Process;" "Experiments and Observations on the Emission of Heat by Hot-water Pipes;" and "Purification of Water on the Large Scale by Agitation with Iron" (being a process successfully elaborated by him, and applied at the Antwerp Waterworks, &c). Received the Telford Medal and the James Watt Gold Medal of the Inst. C.E. Author of a Lecture on "The Generation of Steam," being one of the "Heat Series" of Special Lectures delivered at the Inst. C.E.; of a Text-book on "The Conversion of Heat into Useful Work," being the substance of a course of Lectures delivered at the Society of Arts under the "Howard Trust"; of a paper on "New Applications of the Mechanical Properties of Cork," communicated as a Lecture to the Royal Institution; and of various papers communicated to the Inst. of Mechanical Engineers, the Royal Agricultural Society, &c. Distinguished for the ability with which he has applied his intimate knowledge of the science of heat, and other cognate sciences, to the practical requirements of the engineer.

Frederick Orpen Bower, D.Sc. (Camb.),

F.L.S., F.R.S.E. Regius Professor of Botany in the University of Glasgow. Distinguished for his researches in histological and morphological botany. Author (in conjunction with Prof. S. H. Vines, F.R.S.) of "A Course of Practical Instruction in Botany," and of the following papers, amongst others:—On the Development of the Conceptacle in Fucaceæ" (*Quart. Journ. Micros. Sci.*, 1879); "On the Germination of *Welwitschia*" (*ibid.*, 1880); "On the Further Development of *Welwitschia*" (*ibid.*, 1881); "On the Germination and Embryogeny of *Gnetum Gnemon*" (*Quart. Journ. Micros. Sci.*, 1882); "On the Structure of the Stem of *Rhynchopetalum montanum*" (Journ. Linn. Soc., 1883); "On the Comparative Morphology of the Leaf in Vascular Cryptogams and Gymnosperms" (Phil. Trans., 1884); "On the Apex of the Root of *Osmunda* and *Toaia*" (*Quart. Journ. Micros. Sci.*, 1884); "On Apospory in Ferns" (Journ. Linn. Soc., 1884); "On the Development and Morphology of *Phylloglossum Drummondii*" (Phil. Trans., 1885); "On Apospory and Allied Phenomena" (Trans. Linn. Soc., 1887); "On the Limits of the Use of the Terms Phyllome and Caulome" (*Annals of Bot.*, 1887); "On the Modes of Climbing in the Genus *Calamus*" (*ibid.*); "On some Normal and Abnormal Developments of the Oophyte in Trichomanes" (*ibid.*); "*Humboldtia laurifolia* as a Myrmekophilous Plant" (Trans. Phil. Soc. Glasg.); "The Comparative Examination of the Meristems of Ferns as a Phylogenetic Study" (*Annals of Bot.*, 1889); "On the Morphology of the Leaf of Nepenthes" (*ibid.*); "On Antithetic as distinct from Homologous Alternation of Generations in Plants" (*ibid.*, 1890). Translator (in conjunction with Dr. D. H. Scott) of "Comparative Anatomy of the Phanerogams and Ferns," by Anton de Bary (Clarendon Press, 1884).

Sir John Conroy, Bart., M.A.,

F.C.S. Lecturer on Physics and Chemistry, Keble College, Oxford. An assiduous Student of Experimental Science, and author of the following papers:—"On the Dioxides of Calcium and Strontium" (Journ. Chem. Soc., 1873); "On the Polarization of Light by Crystals of Iodine" (Proc. Roy. Soc., 1876); "Absorption-Spectra of Iodine" (Proc. Roy. Soc., 1876); "On the Light reflected by Potassium Permanganate" (*Phil. Mag.*, 1878); "The Distribution of Heat in the Visible Spectrum" (*Phil. Mag.*, 1879); "Experiments on Metallic Reflexion" (Proc. Roy. Soc., 1871, 1879, 1883).

Daniel John Cunningham, M.D. (Edin.),

M.D. (Dublin), F.R.C.S.I., F.R.S.E., F.Z.S., Professor of Anatomy, University of Dublin. Distinguished both as a teacher and original inquirer. Examiner in Anatomy in the Universities of London, Edinburgh, and Dublin. Member of Council, Royal Irish Academy. Vice-Pres. Zoological Society, Ireland. Vice-Pres. Anatomical Society of Great Britain and Ireland. Author of numerous anatomical memoirs in journals and publications of scientific societies. More especially may be mentioned—"Report on the Anatomy of the Marsupialia" (*Challenger* Report, Part 16); "The Lumbar Curve in Man and Apes," forming Cunningham Memoir, No. 2, published by the Royal Irish Academy, 1886; "The Spinal Nervous System of the Porpoise and Dolphin" (*Journ. Anat. Physiol.*, 1876). Author of a Text-book of Practical Anatomy.

George Mercer Dawson, D.Sc.,

F.G.S., A.R.S.M., F.R.S.C. Assistant Director, Geological Survey of Canada. Much important and valuable work, more especially in geology and ethnology, as in the following summary statement. During his thirteen years of service on the Geol. Survey (Canada) has been chiefly engaged in working out the Geology of the North-West Territory and British Columbia. Placed in charge of the Yukon Expedition, 1887. Author of numerous papers, chiefly geological, but including geographical, ethnological, and other observations, published in the Quart. Journ. Geol. Soc., Trans. Roy. Soc. Canada, *Canadian Naturalist*, &c. These deal more especially with the superficial geology of the regions explored, but some describe Foraminifera and other microscopic organisms. Author of fifteen reports published by the Geological Survey of Canada, and joint author (with Dr. Selwyn) of a Descriptive Sketch of the Physical Geography and Geology of Canada, and (with Dr. W. F. Tolmie) of Comparative Vocabularies of the Indian Tribes of British Columbia.

Edwin Bailey Elliott, M.A.,

Fellow of Queen's College, Oxford. Vice-President of the London Mathematical Society. Mathematical Lecturer of Queen's and Corpus Christi Colleges. Distinguished as a Mathematician and original investigator in various branches of mathematical research. Author of the following papers:—"Generalization of Prevost and Lhuilier's Theorem in Chances" (*Ed. Times*, vol. xxxv.); "On Normals to Envelopes" (*Mess. of Math.*, vol. ix. p. 85); "On Multiple Definite Integrals" (Lond. Math. Soc. Proc., vol. viii., pp. 35, 146); "Kinematics on a Sphere" (*ibid.*, vol. xii., p. 47); "Multiple Frullanian Integrals" (*ibid.*, vol. xv., p. 12; Small Motions of Systems with One Degree of Freedom" (*Mess. of Math.*, vol. xv., p. 38); "The Linear Partial Differential Equations satisfied by Pure Ternary Reciprocants" (Lond. Math. Soc. Proc., vol. xviii., p. 142); "On the Interchange of the Variables in certain Linear Differential Operators" (Abstract, Roy. Soc. Proc., vol. xlvi., p. 358 [ordered to be printed in the Phil. Trans.]); and eighteen other papers printed in the London Mathematical Society's Proceedings and elsewhere between the years 1875 and 1890.

Percy Faraday Frankland, B.Sc.,

A.R.S.M., Ph.D. Professor of Chemistry. Formerly Senior Demonstrator in the Chemical Laboratory of the Normal Schools of Science, South Kensington. Author of upwards of twenty original papers in the Phil. Trans. and Proc. Roy. Soc., in the Journals of the Chem. Soc., the Soc. of Chem. Industry, &c. Known for his researches on Bacteriology and on the Chemical Aspects of Fermentation.

Percy C. Gilchrist,

A.R.S.M. Metallurgist. Distinguished as a Metallurgist, especially in connection with the manufacture of iron and steel. In association with the late Mr. S. G. Thomas he greatly advanced metallurgical practice by the introduction of a process which enables iron to be dephosphorized on a large scale. The process, which is known as the "Basic" process, possesses more than national importance, and its value has been universally recognized. It has further been shown that the slag, which is a product of the Basic process, contains phosphorus in a form which can be readily assimilated by vegetation. One result of his metallurgical work has thus been to substantially benefit agriculture, as more than half a million tons of basic slag are now used annually as a fertilizer. He is the author of numerous papers published in the Journal of the Iron and Steel Institute and elsewhere.

William Dobinson Halliburton, M.D., B.Sc.,

Assistant Professor of Physiology in University College, London. Has during the past four years devoted his entire time to research work in, and teaching of Physiology, especially the chemical side of that science. Has published the following, among other communications:—"On the Proteids of Serum" (Proc. Roy. Soc., and *Journ. of Physiol.*, 1884); "On the Chemical Composition of Invertebrate Cartilage" (Proc. Roy. Soc., 1885, and *Quart. Journ. Micros. Sci.*); "On the Blood of Crustacea" (*Journ. of Physiol.*, 1885, and in a Report to the Scottish Fisheries Board); "On Hæmoglobin and Methæmoglobin Crystals" (*Brit. Med. Journ.*, 1886, and Proc. Physiol. Soc.); "On the Blood-proteids of Lower Vertebrates" (*Journ. of Physiol.*, 1886); "On the Coagulation of Myosin" (Prelim. Communication to Physiol. Soc., 1887).

Oliver Heaviside,

Learned in the science of electro-magnetism, having applied higher mathematics with power and success to the developments of Maxwell's theory of electro-magnetic wave propagation, and having extended our knowledge of facts and principles in several directions and into great detail. He is the author of the following papers among many others:—"On Electro-magnetic Induction and its Propagation" (48 parts, 1885–87, in the *Electrician*); "The Induction of Currents in Cores" (15 parts, 1884–85); "Some Electrostatic and Magnetic Problems" (5 parts, 1883); "Current Energy" (19 parts, 1883–84); "On the Electro-magnetic Effects due to the Motion of Electrification through a Dielectric" (*Phil. Mag.*, 1889); "The General Solution of Maxwell's Equations" (*Phil. Mag.*); "On Electro-magnetic Waves" (6 parts, *Phil. Mag.*, 1888); "On Resistance and Conductance Operators" (*Phil. Mag.*, 1887); "On the Self-induction of Wires" (7 parts, *Phil. Mag.*, 1886–87); "On the Electro-magnetic Wave Surface" (*Phil. Mag.*, 1885); "On the Electro-magnetic Effect of a Moving Charge"; "The Deflection of an Electro-magnetic Wave by Motion of the Medium"; "On the Working of Cells with Condensers" (*Phil. Mag.*, 1874); "On the Extra Current" (1876); "On the Speed of Signalling through Heterogeneous Telegraph Circuits" (*ibid.*, 1877); "On the Effect of Faults on the Speed of Working Cables"; "On Electromagnets" (Journ. Soc. Tel. Eng.); "On Induction between Parallel Wires" (*ibid*).

John Edward Marr, M.A. (Cantab.),

F.G.S. Fellow and Lecturer of St. John's College, Cambridge, and University-Lecturer in Geology. First Class Nat. Sci. Tripos, 1878; Sedgwick Prizeman, 1883; Examiner for the Nat. Sci. Tripos, 1886–87. Secretary of the Geological Society, 1888. Author of the following:—"Fossiliferous Cambrian Slates near Carnarvon" (Quart. Journ. Geol. Soc., 1876); "On Phosphatized Carbonate of Lime at Cave Ha" (*Geol. Mag.*, 1876); "On some well-defined Life-zones in the lower part of the Silurian of the Lake District" (Quart. Journ. Geol. Soc., 1878); "On the Cambrian and Silurian Rocks of the Dee Valley" (*ibid.*, 1880); "On the Pre-Devonian Rocks of Bohemia" (*ibid.*, 1880); "On some Sections of the Lower Palæozoic Rocks of the Craven District" (Proc. Yorks. Geol. Soc., 1882, and Brit. Assoc., 1881); "The Classification of the Cambrian and Silurian Rocks" (*Geol. Mag.*, 1881); "On the Cambrian and Silurian Rocks of Scandinavia" (Quart. Journ. Geol. Soc., 1882); "Origin of the Archæan Rocks" (*Geol. Mag.*, 1883); "The Classification of the Cambrian and Silurian Rocks" (Sedgwick Prize Essay, 8vo, Cambridge, 1883); "The Earth History of the Remote Past compared with that of Recent Times" (8vo, Cambridge, 1886); "The Lower Palæozoic Rocks near Settle" (*Geol. Mag.*, 1887); "The Work of Ice Sheets" (*ibid.*); "Glacial Deposits of Sudbury" (*ibid.*); "On some Effects of Pressure on the Devonian Sedimentary Rocks of North Devon" (*ibid.*, 1888); "The Lower Palæozoic Rocks of the Fichtelgebirge" (*ibid.* 1889); "The Metamorphism of the Skiddaw Slates" (Brit. Assoc., 1889). Joint-papers:—"The Lower Palæozoic Rocks of the Neighbourhood of Haverfordwest" (Quart. Journ. Geol. Soc., 1885); "The Stockdale Shales" (*ibid.*, 1888).

Ludwig Mond,

F.I.C. President of the Society of Chemical Industry, V.-P. Chem. Soc. Distinguished technical chemist and inventor. Has made important additions to chemical industrial processes and products, especially with reference to the alkali industry, having improved the mode of manufacture of carbonate of soda, caustic soda, hydrochloric acid, chlorine, ammoniacal products, and gas generating furnaces, &c. In 1863 he developed what is known as the "Mond Process of Sulphur Recovery from Alkali Waste," and has since that date devoted himself to the introduction and development of the ammonia soda process of alkali manufacture into England. Author of various papers in Rept. Brit. Assoc., Journ. Soc. Chem. Ind.

William Napier Shaw, M.A.,

Fellow of Emmanual College, Cambridge. Was nominated by Lord Rayleigh as one of the Demonstrators of Physics in the Cavendish Laboratory at Cambridge. He held that position from 1880 to 1887, and he has since continued his connection with the Laboratory as University Lecturer in Physics. His knowledge of the manner in which the teaching of Physics is conducted in the great German Universities (acquired at Berlin under Helmholtz) enabled him to bear an important part in the organization of the laboratory. A considerable part of the success of the Cambridge School of Physics is due to his exertions, backed by his knowledge of Physics. Author of numerous books and papers, of which the following are especially worthy of notice:—"Practical Physics" (jointly with Mr. Glazebrook), Longmans, 1885; "Practical Work in the Cavendish Laboratory," University Press, 1886; "Faraday's Law of Electrolysis with reference to Silver and Copper," Rept. Brit. Assoc., 1886; "Electrolysis" and "Pyrometer," "Encyc. Brit."; "On Vaporimeters," &c., Rept. to the Meteorol. Council, 1884; "On Hygrometric Methods, Part I.," Rept. to the Meteorol. Council, printed in Phil. Trans.

Silvanus Phillips Thompson, D.Sc. (Lond.),

Principal and Professor of Physics in the City and Guilds of London Technical College, Finsbury; formerly Professor of Experimental Physics in University College, Bristol. Author of many papers published in the Proceedings, &c., Royal Society, Physical Society, Institution of Electrical Engineers, Society of Arts, and British Association, including the following:—"The Theory of the Magnetic Balance" (Proc. Roy. Soc., 1884); "Electro-deposition of Alloys" (*ibid.*, 1887); "Subjective Interference of Sound" (*Phil. Mag.*, 1887); "Opacity of Tourmaline Crystals" (*ibid.*, 1881); "The Meaning of the Constant in Bernouilli's Law of the Lifting Power of Magnets" (*Phil. Mag.*, 1888); "Development of the Mercurial Air Pump" (Journ. Soc. Arts, 1887); "The Influence Machine from 1788 to 1888" (Journ. Soc. Telegr. Engin., 1888). Author of a treatise on "Dynamo-Electric Machinery" (3rd edit., 1888), and of an elementary text-book of Electricity and Magnetism (43rd thousand, 1889), which has gone through many English and several foreign editions. Originator of improvements in polarizing prisms, in the method of adjusting resistance coils, and in sundry electrical apparatus. Member of Council of the Physical Society, and of the Institution of Electrical Engineers. Distinguished for his acquaintance with the science of electricity, more particularly in its experimental and technical aspects.

Thomas Henry Tizard, Staff-Commander, R.N., H.M.S. *Triton*,

F.R.G.S. Distinguished as a Hydrographical Surveyor and Marine Meteorologist. Has been employed for 25 years in the Naval Surveying Service. In China, Mediterranean, and Red Seas, 1862–72. Senior Assistant-Surveyor in the *Challenger* Expedition, 1872–76. Prepared the reports on the sea temperatures, and on the meteorological observations obtained under his own superintendence during the voyage (*Challenger* Report, vol. ii.); Joint Author of vol. i. *Challenger* Report, contributing the hydrographical portion of the Narrative of the Voyage. Has since served for nine years in charge of surveys on the coasts of the United Kingdom; now employed in command of H.M.S. *Triton*. Has contributed a paper to the Royal Society on the exploration of the Faeroe Channel (Proc. Roy. Soc., vol. xxxv. pp. 202–26; and on the meteorology of Japan, to the Meteorological Council (Official Publication, No. 28).

THE ENDOWMENT OF RESEARCH IN FRANCE.

AT the meeting of the Paris Academy of Sciences on April 27, the Secretary read the following extract from the will of the late M. Cahours:—

"I have frequently had the opportunity of observing, in the course of my scientific career, that many young men, distinguished and endowed with real talent for science, saw themselves obliged to abandon it because at the beginning they found no efficacious help which provided them with the first necessities of life and allowed them to devote themselves exclusively to scientific studies.

"With the object of encouraging such young workers, who for the want of sufficient resources find themselves powerless to finish works in course of execution, and in remembrance of my beloved children, who also would walk in a scientific path at the moment when death takes me from them, I bequeath to the Academy of Sciences, which has done me the honour to admit me into its fraternity, a sum of one hundred thousand francs.

"I desire that the interest of this sum may be distributed every year by way of encouragement to any young men who have made themselves known by some interesting works, and more particularly by chemical researches.

"In order to assure this preference, independently of the express recommendation that I make here to my successors, I wish that, during at least twenty-five years after the commencement of the interest payable to the Academy, three members at least of the Chemistry Section may take part each year in a Commission of five members charged by the Academy to distribute the prizes. I express further the formal desire that this choice should fall, as far as possible, on young men without fortune not having salaried offices, and who, from the want of a sufficient situation, would find themselves without the possibility of following up their researches.

"These pecuniary encouragements ought to be given during several years to the same young men, if the Commission thinks that their productions have a value which permits such a favour.

"Nevertheless, in order that the largest number of young workers may participate in the legacy I institute, I desire that the encouragements may cease at the time when the young *savants* who have enjoyed them obtain sufficiently remunerative positions."

M. Janssen then made the following remarks:—

"The legacy which has been made to the Academy, by our very eminent and very regretted *confrère*, appears to me to have considerable import not only by its importance, but especially by the way that it opens, and the example that it affords, to all those who hereafter may desire to encourage the sciences by their liberality.

"M. Cahours, whose sure judgment and long experience enabled him to know the most urgent necessities of science, had, like most of us, become convinced of the necessity of introducing a new form in the institution of scientific recompenses.

"Our prizes will always continue to meet a great and noble necessity; their value, the difficulty of obtaining them, and the *éclat* they take from the illustriousness of the body which awards them, will make them always the highest and most envied of recompenses.

"But the value, also, of the works it is necessary to produce in order to lay claim to them prohibits the research to beginners. It is a field that is only accessible to matured talents.

"But, besides those *savants* who have already an assured career, there are many young men endowed with precious aptitudes, and directed by their inclination to pure science, but turned very often from this envied career by the difficulties of existence, and taking with regret a direction giving more immediate results. And yet, how many among them possess talents which, if well cultivated, might do honour and good to science!

"We must say, however, that it is in leaving their studies that those who wish to devote themselves to pure science experience the most difficult trials, and these difficulties are increased every day by the very rapid advance of the exigencies of life.

"We must find a prompt remedy for this state of things if we do not wish to see the end of the recruitment of science.

"This truth, however, is beginning to be generally felt. The Government has already created institutions, scholarships, and encouragements, which partly meet the necessity. Some generous donors are also working in this manner. I will mention especially the noble foundation of Mdlle. Dosne, in accordance with whose intentions a hall is at this moment being built, where young men, having shown distinguished aptitudes for high administration, the bar, or history, will receive for three years all the means of carrying on high and peaceful studies.

"Let us say, then, plainly, and in speaking thus we only feebly echo the expressions of the most illustrious members of the Academy, that it is by following the way so nobly opened by Cahours that the interests and prospects of science will be most efficaciously served."

NOTES.

A SPECIAL meeting of the Physical Society of London will be held at Cambridge on Saturday, May 9. The members will leave Liverpool Street at 11 a.m., and on arrival at Cambridge will become the guests of the Cambridge members. The meeting will be held in the Cavendish Laboratory at 2.30. The following communications will be read: some experiments on the electric discharge in vacuum-tubes, by Prof. J. J. Thomson, F.R.S.; some experiments on ionic velocities, by Mr. W. C. D. Whetham; on the resistance of some mercury standards, by Mr. R. T. Glazebrook, F.R.S.; on an apparatus for measuring the compressibility of liquids, by Mr. S. Skinner; some measurements with the pneumatic bridge, by Mr. W. N. Shaw. After the meeting members will have an opportunity of seeing the Cavendish Laboratory and other University Laboratories.

THE annual meeting of the Iron and Steel Institute began yesterday, and will continue to-day and to-morrow. It is being held as usual at the Institution of Civil Engineers in Great George Street.

A VALUABLE bequest has been made to the Department of Science and Art by the late Miss Marshall, of 92 Warwick Gardens, Kensington. In addition to a large number of scientific books and instruments which are left for the use of students, a sum of £1000 is bequeathed for the founding of scholarships, or for application in any other way that may be considered best for the advancement of biological science.

THE Queen has approved the appointment of Lord Derby to be Chancellor of the University of London, in the room of the late Lord Granville.

THE death of Prof. Joseph Leidy, in his sixty-eighth year, is announced. He was Professor of Anatomy in the University of Pennsylvania and of Natural History in Swarthmore College; President of the Academy of Natural Sciences of Philadelphia; and Director of the Department of Biology in the University. In a future number we shall give some account of his services to science.

A REUTER'S telegram from New York, dated May 1, announces the death, at Berkeley, California, of Prof. John Le Conte, brother of Mr. Joseph Le Conte, formerly professor of geology and natural history in the University of California.

WE regret to have to announce the death of Captain Cecilio Pujazon, the Director of the Marine Observatory of San Fernando, near Cadiz. He died on April 15, in his fifty-seventh year. Captain Pujazon was well known to the members of the Eclipse Expedition of 1870, who formed the Cadiz party. He came to London to the Conference on Marine Meteorology in 1874.

IN answer to a question put by Mr. H. Fowler in the House of Commons on Monday, Sir W. Hart Dyke said that from the returns already received, in answer to a circular issued by the

Science and Art Department at the end of March last, it appeared that of the fifty county councils and sixty county boroughs in England, sixteen of the former and twenty-five of the latter had already decided to apply the whole of their share of the residue under the Local Taxation (Customs and Excise) Act of 1890 to science and art and technical education. Nine county councils and two county boroughs had made grants varying from "nearly the whole" to a smaller proportion of their share to the same purpose. Twelve county councils and seven county boroughs had the matter under consideration; that is to say, they had appointed committees, and in many cases the committees had recommended the allocation of the whole or the greater part of the residue fund to technical instruction, but their reports had not yet been confirmed by the county or borough councils. With regard to Wales, the question was complicated by the fact that the Welsh Intermediate Education Act included technical instruction, but it appeared that four county councils and one county borough had applied the whole of their share of the residue under the Intermediate Education Act; while two county councils and one county borough had divided their quota between that Act and the Technical Instruction Act. The remaining six county councils had either made no return, or else had the matter under consideration.

THE Council of University College, Bangor, having resolved to make provision in the physical department (Prof. A. Gray) for the study of applied electricity, an 8 horse-power (nominal) compound engine, working up to 24 horse-power, has just been installed by Messrs. Robey and Co., Lincoln. On Saturday last a satisfactory trial of the engine and boiler was made. The equipment includes a special educational Victoria dynamo (capable of being converted at will into a shunt, compound-wound, or series dynamo, without impairing its usefulness for general work), by the Brush Electrical Engineering Co., an alternating dynamo, and a large secondary battery. The electrical measuring instruments are of the latest design, and include a fine composite balance, and electrostatic voltmeter of Sir William Thomson's invention. The equipment forms a valuable addition to the resources of the College for the teaching of pure and applied physical science, and will render it possible to give a very complete course of instruction in electrical engineering, as well as in the general theory of electricity.

THE Philosophical Society of Berlin offers a prize of 1000 marks for the best essay on the relation of philosophy to the empirical science of nature. The essays may be written in German, French, English, or Latin, and must be sent in before April 1, 1893.

THE Italian Meteorological Society has celebrated its twenty-fifth anniversary by erecting a memorial tablet in the mediæval castle of Turin. The founder of the Society, Father Denza, and various notabilities and ladies were present. Father Denza gave a *résumé* of the history of the Society, which now possesses no less than 250 observatories and stations. The ceremony was terminated by the transmission of a telegram to the King, as Honorary President of the Society.

THE Chief Signal Officer of the United States has published Part III. of "Bibliography of Meteorology," comprising titles relating to the general motions of the atmosphere, or "winds," while the important division of "storms" is being prepared for issue as Part IV. The present volume, like its predecessors, is a lithographic reproduction of a copy prepared by means of a type-writer, as funds were not forthcoming for printing the work, and it contains a total of 2000 titles of books and papers dating from the origin of printing to the close of 1881, with a supplement to the close of 1889, and an author index. The work is quite unique, and will be an invaluable aid to the study of the subject treated of.

AN account of the Birmingham School of Medicine, written originally for the information of those members of the medical profession who attended the Birmingham meeting of the British Association in 1890, has now been published separately. The authors are Dr. B. C. A. Windle and Mr. W. Hillhouse. Their intention is to show—and this they do most effectually—that the centre of the Midland district possesses one of the best equipped schools of medicine in the provinces. The interest of the descriptions is greatly increased by reproductions of some photographs.

A FISH and Game Commission, taking evidence on behalf of the Ontario Government, has received many complaints as to the destruction of deer and other depredations by wolves; and all the witnesses agree that the present bounty of £1 paid for each wolf killed should be raised to £2 10*s*. or £3. It has also been shown that, if the game laws are not more strictly enforced, many birds and fur-bearing animals will probably be exterminated.

THE preliminary returns of the recent census operations in India show that the population in British territory is 220,400,000, as against 198,655,600 in the former census, an increase of nearly 22,000,000. The Feudatory States, omitting incomplete returns, which may be taken at about 90,000, have a population of 61,410,000, making a total of 281,900,000, as against 250,700,000 for the same areas at the last census. The returns give Bombay 806,000, Madras 449,000, Calcutta municipal area and port 674,000, and including the suburbs Howrah and Bally, 969,000. At the last census the total for the same area was 847,000. Calcutta municipal area shows an increase of 92,000, and Howrah and Bally an increase of 24,000. The returns from Burmah show that the population of the whole country, excluding the Shan States, is 7,507,063, or 48·8 persons to the square mile. The population of Lower Burmah alone is 4,526,432, or an increase of about 790,000 since 1881.

THE Boston Society of Natural History has issued a pamphlet announcing the completion of the general plans for the formation of zoological gardens and aquaria in Boston, and appealing to the American public for support. The pamphlet is prettily printed and illustrated, and sets forth very effectively the arguments which may be advanced in favour of the scheme.

THE new number of the Journal of the Royal Horticultural Society contains a full report of the Dahlia Conference, held at the Chiswick Gardens on September 23 last; and of the Grape Conference, held in the same Gardens on September 24. The number also contains valuable papers on various other subjects interesting to horticulturists.

THE Trustees of the Indian Museum, Calcutta, have issued an interesting and instructive Report, by Mr. E. C. Cotes, on the locust of North-Western India (*Acridium peregrinum*). The Report sums up the results of an investigation conducted in the entomological section of the Museum. It seems to be established that most of the flights of this locust issue from the region of sand-hills in Western Rajputana. Others, however, invade India from breeding-grounds which probably lie along the Suliman Range, or even, perhaps, in some cases, beyond India's western frontier, in the sandy deserts of Baluchistan, Southern Afghanistan, and Persia, though the reports received from these regions, Mr. Cotes says, are so fragmentary that no very definite conclusions can be formed from them.

THE *New Zealand Journal of Science*, the publication of which was suspended in 1885, has been revived. The first two numbers of the new issue have been sent to us, and if the same general level of excellence can be maintained in future numbers, there ought to be no doubt as to the success of the enterprise. The following are among the papers: on the history of the

Kiwi, by Prof. T. J. Parker; on the breeding habits of the European sparrow in New Zealand, by T. W. Kirk; the humble-bee in New Zealand, by G. M. Thomson; some notes on the occurrence of the trap-door spider at Lyttelton, by R. M. Laing; on the discovery of the nickel-iron alloy Awaruite, by Prof. G. H. F. Ulrich.

In the paper on the humble-bee in New Zealand, Mr. Thomson says that, wishing to find how far these insects are adapting themselves to new flowers in the colony, he has for a considerable time kept a record of the flowers they visit and of those they leave alone. He has noticed them on many species of introduced plants which they never appear to visit in Europe. They seldom approach white flowers; and, with two exceptions, he has never heard of their visiting the flowers of indigenous plants. The exceptions are *Fuchsia excorticata* and the Ngaio (*Myoporum lætum*).

Messrs. R. Etheridge, Jun., and Mr. A. Sidney Olliff have produced in common a paper which forms a valuable addition to the Memoirs of the Geological Survey of New South Wales. The title is "The Mesozoic and Tertiary Insects of New South Wales."

Messrs. Baillière, Tindall, and Cox publish a second edition of Dr. Thomas Dutton's practical treatise on "Sea-Sickness." Sensible readers will at once be favourably impressed by the author's statement that there is "no absolute specific" for this distressing malady.

Messrs. Cassell and Co. have issued Part 31 of their "New Popular Educator," which will be completed in 48 parts. Besides illustrations in the text, there is a carefully prepared page representing coloured reactions characteristic of certain metals, &c.

Mr. T. H. Cornish, of Penzance, has a note in the current number of the *Zoologist* on some remarkably large catches of fish on the Cornish coast. On March 18 last, 12,000 grey mullet, *Mugil capito*, were captured, by means of a draw seine, by the fishermen of Sennen Cove, at Whitsand Bay, Land's End. The fish were of fine quality, one being brought to Mr. Cornish which measured 2 feet in length, 1 foot 3 inches in girth, and weighed 6 pounds 10 ounces. On the 31st of the same month a Lowestoft mackerel driver, fishing some leagues south-west of the Lizard, took 48,000 mackerel. No such catch of mackerel, for one night's fishing, had ever been heard of before at Penzance, and what makes it more extraordinary, says Mr. Cornish, is that it should have taken place in March, when the catches usually average a few hundreds only. Later on in the season, in the fishing west of Scilly, 20,000 to 25,000 is regarded as a heavy catch. The catch sold for £360.

With reference to our note (vol. xliii. p. 521) on an award made by the Japanese Government to Dr. Shohei Tanaka for "the invention of a new musical instrument," Mr. J. W. Goundry, of Gosforth, Newcastle, writes to us that over twenty years ago he patented an arrangement for giving enharmonic intervals in all keys on the ordinary unaltered keyboard, and that he has had both an organ of 31, and a harmonium of 36, sounds per octave, playing Bach's fugues and Handel's choruses, &c., on the system. He claims that, although his patents were very crude and imperfect, they contain at least the germ of a complete solution of the problem of reconciling just intonation with the ordinary manual. "They embody a system of sounds," he says, "which I believe to be theoretically the truest and practically the simplest possible, and which has nowhere else been described."

Ammonium sulphovanadate, $(NH_4)_3VS_4$, has been isolated in large crystals by Drs. Krüss and Ohnmais, and an account of their work, which also includes the preparation of several other sulpho-salts of vanadium, will be found in the latest number of *Liebig's Annalen*. It is well known that when ammoniacal solutions of vanadates are treated with sulphuretted hydrogen a magnificent purple colouration is produced, presumably due to the formation of sulpho-salts. It has not been found possible, however, to obtain such salts by crystallization *in vacuo*. The method of obtaining the ammonium salt now described is as follows:—A stream of sulphuretted hydrogen is led into an ice-cold saturated solution of ammonium metavanadate, NH_4VO_3, in the strongest ammonia. The immediate effect is to produce the violet-red colour, but the colouration soon disappears and a brown solid is precipitated. On continuing the passage of the gas this precipitate slowly redissolves with production again of the deep violet colour. When the re-solution of the precipitate is almost complete the liquid is filtered, and sulphuretted hydrogen again led through the solution. In a short time crystals commence to separate, when the current of gas is stopped and the liquid left to crystallize in a closed vessel. The crystals thus obtained consist of opaque rhombic prisms very much resembling in appearance those of potassium permanganate. The faces are very brilliant and reflect a steel bluish-violet colour with a greenish tint when the reflection is received at a certain angle. They may be washed with absolute alcohol and afterwards with ether, and finally dried *in vacuo*. The mother liquors from the first crystallizations deposit magnificent crystals on being allowed to stand some weeks. The substance may be much more quickly obtained and in larger quantity by substituting either potassium or sodium vanadates for the ammonium vanadate used in the above mode of preparation, as these salts are much more soluble in ammonia than ammonium vanadate. It is somewhat remarkable that in this case pure ammonium sulphovanadate should be obtained, no potassium or sodium sulpho-salts being ever found in the product. The crystals of ammonium sulphovanadate are permanent in dry air, but are slowly decomposed with evolution of sulphuretted hydrogen in moist air. They are readily soluble in water, forming a solution which is coloured intensely violet even when very dilute. A solution containing only one part of the salt in 100,000 parts of water still possesses a beautiful rose-red colour. After a short time this solution decomposes, sulphuretted hydrogen being liberated and the colour changing to brown. When a freshly prepared solution is added to a solution of a salt of the alkaline earthy metals, no precipitate is produced, owing to the solubility of the sulphovanadates of these metals. But in the case of calcium a remarkable deepening of the violet colour is produced. If, for instance, a little calcium chloride is added to a dilute solution possessing a just perceptible rose tint, the colour becomes immediately deep violet, owing to the extreme tinctorial power of the calcium salt.

In our note in vol. xliii. p. 592, upon the preparation among other silicon compounds of silicon chloro-tribromide, $SiClBr_3$, by M. Besson, it was stated that this substance had not been hitherto prepared. We wish to correct this statement. Silicon chloro-tribromide was prepared by Prof. Emerson Reynolds in 1887, and a descriptive note of the work was given in Nature at the time (vol. xxxvi. p. 137).

The additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Mrs. Emily Palmer; two Brazilian Caracaras (*Polyborus brasiliensis*) from Terra del Fuego, a Turkey Buzzard (*Cathartes aura*) from the Falkland Islands, presented by Mr. F. E. Cobb, C.M.Z.S.; two Herring Gulls (*Larus argentatus*), British, presented by Mrs. Attenborough; a Pine Grosbeak (*Pinicola enucleator*), British, presented by Mr. W. H. St. Quintin; a Bennett's Wallaby (*Halmaturus bennetti* ♂) from Tasmania, two Diamond Snakes (*Morelia spilotes*) from Australia, deposited; two Tasmanian Wolves (*Thylacinus* *cynocephalus* ♂ ♀), three Ursine Dasyures (*Dasyurus ursinus* ♂ ♀ ♀) from Tasmania, two Brush Turkeys (*Talegalla lathami* ♂ ♀), four Australian Wild Ducks (*Anas superciliosa*) from Australia, received in exchange; a Black Lemur (*Lemur macaco*), two Persian Gazelles (*Gazella subgutturosa*), born in the Gardens.

THE INSTITUTION OF MECHANICAL ENGINEERS.

ON the evenings of Thursday and Friday of last week, the 30th ult. and the 1st inst., the Institution of Mechanical Engineers held an ordinary general meeting; the President, Mr. Joseph Tomlinson, occupying the chair. There was an attendance of members somewhat in excess of that which is usual at the ordinary meetings of this Institution. There were two items on the programme—namely, a paper on Lancashire boilers, and a further report from the Research Committee on Marine Engine Trials. The discussion of the latter occupied so much time that the boiler paper had to be shelved until next meeting.

The latest steamer upon which the Marine Engine Research Committee has been experimenting is a cargo vessel named the *Iona*. She was built and engined by W. Gray and Co., of West Hartlepool, and is a good modern example of what can be done in fuel economy with triple expansion engines when high speed is not aimed at. This latest report of the Committee should re-establish in some minds the belief in the economy of the marine surface-condensing engine, which had been, so it was thought, rather shaken by the previous labours of the Committee. When on the first trials of the Committee the fuel consumption of the *Meteor*, *Fusi Yama*, and the *Colchester* came out at not less than 2 pounds of coal per indicated horse-power per hour—the *Colchester's* consumption being nearly 3 pounds per hour—it was said, by those who had never believed in the claims of marine engineers, that the bubble was pricked by a trial made by competent and unbiassed persons. It is true the *Tartar's* trial improved on these figures, the coal consumption coming out 1·77 pound per indicated horse-power per hour; still this is some way behind the 1½ pound of which marine engineers had been boasting. The experiments with the *Iona*, now under consideration, have rehabilitated the marine engine as an economical form of steam motor, for there can be no doubt that the engines of that vessel have given off on trial one unit of power per hour for less than the pound and a half of coal, and we have no reason to think that the 1·38 pound shown on the chief engineer's independent trial is not a fair average for sea running when the disturbing element of measuring tanks is omitted.

The *Iona* is a well decked vessel, built in 1889. She has triple expansion engines on three cranks, working a single screw. The vessel is 275·1 feet long, 37·3 feet wide, and 19 feet deep in the hold. Her moulded depth is 21 feet 10 inches, and her coefficient of fineness is 0·765. She has a cellular bottom. Her mean draught in dock before trial was 20 feet 8 inches, but she rose half an inch in salt water, the displacement being 4430 tons. The engines had been freshly overhauled. The trial took place off the east coast, between Robin Hood Bay and Great Yarmouth. The weather was fine throughout. The engines are triple compound surface condensing. The cylinders are placed in the order—intermediate, high, low, going from forward to aft. The cranks rotate in the sequence—high, intermediate, low. The diameters of the cylinders are 21·88 inches, 34·02 inches, and 56·95 inches; the stroke is 39 inches. The high pressure cylinder only is jacketted, steam being taken from the boiler direct. Outside this jacket are the receivers for the intermediate and low pressure cylinders. The jacket steam therefore parts with heat to the high pressure cylinder, and also heats up the steam passing to the two other cylinders. The arrangement is unusual but not new. A feature worth noting in the present day is that the steam distributing valves are all slide valves. Mr. Mudd, the designer of the engines, does not follow the modern fashion of using piston valves, it being his belief that the advantages they offer are not equal to those lost. The surface condenser has 1360 square feet of tube surface. There are two ordinary steel boilers having 42 square feet of grate surface, the total heating surface being 3160 square feet; which is equal to 75·2 times the grate area. It is not to be wondered at that, with this liberal allowance of heating surface, the fuel economy came out very satisfactorily. The total cross-sectional area through the tubes is 18·3 square feet, and the area across the funnel 30·7 square feet. A notable feature about this vessel is that the boilers are worked on forced draught; or rather there is a fan for supplying air to the fires, for a pressure equal to only 0·17 inch of water in the ash pits hardly fulfils the popular notion of forced draught. The steam for driving the fan-engine was supplied from the donkey boiler, and therefore the measurements of quantities in the performance table were not affected by the amount of steam used by the fan. The matter is not one of great importance—the power to drive the fan not being, perhaps, more than the three-hundredth part of the power of the propelling engines; but we question whether it is strictly fair—as comparing the *Iona's* machinery with that of other vessels—not to take the fan-engine steam from the main supply. The boilers in this ship have an extraordinarily large proportion of tube surface as compared to the grate surface, and this would be likely to lead to an insufficiency of draught were the lighter specific gravity of the chimney gases alone depended upon. If, therefore, the aid of the fan has to be brought in, its cost as well as its services should be taken into account. This is looking at the matter from the point of view of taking the total efficiency of the machinery, and Prof. Kennedy might very justly urge that the steam used by the fan would be a disturbing element, and prevent him from properly determining the efficiency of the engines. The fan undoubtedly belongs to the boiler, but not more so than the feed-pumps; all boilers, however, must have feed-pumps, while comparatively few have fan-engines. If ever it comes to be that fan-engines are almost as much matters of course as feed-pumps, it will be convenient to class the former with the engine, but until then it is as well to estimate the steam required for forced draught purposes by itself; still it should be taken into account.

The air from the fans is taken to the furnace through gridiron valves, which close automatically when the furnace-door is opened, so as to prevent a rush of flame into the stokehold. A small jet of air is also admitted through the wet end of the boiler back by a passage made for the purpose. In this way there are two streams of air which meet in the combustion-chamber. There is also a hanging bridge attached to the back tube plate, and depending into the flame box at the back of the bridge. By these arrangements a very thorough mixing of the air and furnace gases is secured; and to this, no doubt, is due the unusually perfect combustion which was obtained on the trial. The small grates give additional space for the mixing and burning of the gases before they enter the tubes, a most desirable feature in boiler design, and one which should do much to put the cylindrical flues of modern high-pressure boilers on an equality, in the matter of combustion, with the rectangular furnaces of the comfortable low-pressure days of the past generation of marine engineers. At the same time we must not forget that a large amount of fuel burnt on a small grate requires a large combustion chamber. It is the volume of gases evolved which has to be considered. It should be stated that the arrangement for forced draught was designed by Mr. J. R. Fothergill, of Hartlepool, engineer superintendent to the firm owning the ship.

It is so difficult to get accurate data upon the weight of marine engines, that we add the figures given in the report:—

	Tons.
Engines alone	94·92
Shafting, tunnel-bearings, and propeller ...	26·59
Engine-room auxiliaries, including donkeys, pipes, platforms, ladders, and gratings ...	12·16
Boilers alone	58·60
Boiler-room auxiliaries, including forced draught gear, smoke-box, uptake, funnel, furnace gear, mountings, stokehold floor, boiler-chocks, and ties	28·49
Water in boilers	35·75
Total	228·22

The coal used was of good quality. The following analysis (as used) will be of interest:—

	Per cent.
Carbon	82·34
Hydrogen	5·47
Moisture	1·94
Ash	2·90
Nitrogen, sulphur, oxygen, &c., by difference ...	7·35
	100·00

The calculated calorific value is 14,830 thermal units per pound, which corresponds to the evaporation of 15·35 pounds of water from and at 212° F. A portion of the coal used was also tested by a Thomson calorimeter, and gave a value of 14,980 thermal units per pound. Thirteen samples of furnace gases were taken over mercury and were analyzed. The following are the means:—

	By volume per cent.	By weight per cent.
Carbonic acid	8·20	12·12
Carbonic oxide	0·00	0·00
Oxygen	11·17	12·01
Nitrogen	80·63	75·87
	100·00	100·00

Chimney temperatures were read every half-hour by a mercury thermometer and by two Murrie pyrometers at 30 feet above the furnace bars. The readings of the three instruments agreed, the average temperature being 452° F. It was a pity that the readings were taken so far from the fires, it being desirable to know the heat of the products of combustion immediately after leaving the heating surface of the boiler. The arrangement, however, was unavoidable, owing to the exigencies of running the ship on her voyage. The measurement of the feed was carried out by means of two tanks in the usual way. An effort was made to determine the quantity of water brought over unevaporated, by the draught of steam. This was done by taking samples of condensed steam from the steam pipe and samples of boiler water, and analyzing them to ascertain the percentage of salt. Unfortunately the apparatus broke down; but from two pairs of analyses made, it was estimated that there was 2·87 per cent. of unevaporated boiler water in the condensed steam. If this were the case with boilers so easily driven as those of the *Iona*, where there could hardly have been any semblance of "priming," as the term is understood by engineers, the quantity of water brought over in small and hardly driven boilers must be enormous. It is a point of the greatest importance in steam-engine economy, and we trust Prof. Kennedy will pursue his investigations in this direction. It also came out during the discussion that the stop valve, or throttle valve, was very much closed during the trial, a fact which should still further have reduced the chance of unevaporated water finding its way into the engines.

Indicator diagrams were taken every half-hour during the trial, and an average set is attached to the report. The power was very evenly distributed between the three cylinders, showing good design of the engines. The total indicated horse-power was 645·4. Diagrams were also taken from the air and circulating pumps. For these interesting and valuable details we must refer our readers to the paper itself, as we are unable to reproduce the diagrams.

The following are some of the chief elements of the trial:—

Date	July 13 and 14, 1890.
Duration of trial	16 hours.
Heating surface, total	3160 square feet.
,, ,, tubes	2590 ,, ,,
Grate area	42 ,, ,,
Total heating surface to grate surface...	75·2 ratio.
Grate area to flue area through tubes...	2·3 ,,
Mean boiler pressure above atmosphere	165·0 lbs. per sq. in.
Mean admission pressure, high-pressure cylinder	142·5 ,, ,,
Mean vacuum in condenser below atmosphere	13·88 ,, ,,
Mean revolutions per minute	61·1
I.H.P. of high-pressure cylinder ...	205·6
,, intermediate ,, ...	221·2
,, low-pressure ,, ...	218·6
Coal burnt per hour	942 pounds.
,, ,, square foot of grate per hour	22·4 ,,
Coal burnt per square foot of total heating-surface per hour	0·298 ,,
Coal burnt per I.H.P. per hour ...	1·46 ,,
Carbon equivalent of coal	1·02 ,,
Feed-water per hour	8616·0 ,,
,, ,, lb. of coal	9·15 ,,
,, ,, ,, from and at 212° F.	10·63 ,,
Efficiency of boiler	69·2 per cent.
,, engine	17·1 ,,
,, engine and boiler ...	11·8 ,,
Mean speed of vessel during trial ...	8·6 knots per hour.

A long discussion, occupying both evenings of the meeting, followed the reading of the paper, but our account has already extended to such a length that we cannot give a report of it. Perhaps the most interesting point raised was in connection with the closing of the chimney damper, which it appeared was only one-sixth open during the trial. The reason given for this was that in this way heat was prevented from escaping up the chimney. It is difficult to account for such an effect, excepting perhaps to some trifling extent due to minor causes, but several engineers whose opinion is worthy of respect testified that such was the effect in practice. One would think that the escape of heat by the chimney would be governed by the volume of escaping products of combustion with a chimney of any reasonable cross area.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The first Clerk Maxwell Scholarship, for research in Experimental Physics, has been awarded to W. Cassie, M.A., Trinity College.

Mr. H. J. Mackinder, the Reader in Geography at Oxford, is to lecture for the Teachers' Training Syndicate on "The Teaching of Geography," on May 30.

The annual dinner of the Philosophical Society was held in the Combination Room of St. John's College on May 2, Prof. G. Darwin in the chair.

DUBLIN.—Sir Robert Ball begins on Wednesday, the 13th inst., a course of lectures on "The Theory of Screws," in Trinity College, Dublin.

SCIENTIFIC SERIALS.

THE *Quarterly Journal of Microscopical Science* for March contains:—On a new species of Phymosoma, with a synopsis of the genus, and some account of its geographical distribution, by Arthur E. Shipley (Plate xi.). The new species, *P. weldoni*, was found by Prof. Weldon at Bimini Island, the Bahamas; it has no trace of hooks on the introvert; there are two retractors. A synopsis of the twenty-seven species now known is given, but seventeen species are described in Selenka's monograph on the Sipunculidæ. As to the geographical distribution, seventeen species are found in the Malay Archipelago, of which thirteen are endemic, five are found in the Red Sea, four in the Mauritius, and three are found in the West Indies, but *P. lovenii* is found only in the Bergen Fiord.—On the British species of Crisia, by Sidney F. Harmer (Plate xii.). The author thinks that the ovicells furnish satisfactory specific characters; the aperture in the ovicell is also an important character. Specific diagnoses of *C. denticulata*, Lmk., *C. eburnea*, Linn., *C. aculeata*, Hass., and *C. ramosa*, n. sp., are given. Notes are given of the habit of the Zoarium at different seasons, on the mode of branching, and on the breeding-times.—The later larval development of Amphioxus, by Arthur Willey (Plates xiii.–xv.). The author again visited Messina, in the summer of 1890, to complete his studies on the development of the atrial chamber of Amphioxus. As a possible explanation of the asymmetry of the larva, Willey thinks that it can be traced ultimately to the adaptive forward extension of the notochord, being thus a purely ontogenetic phenomenon; the club-shaped gland is shown to be a modified gill-slit.—On the structure of two new genera of earthworms belonging to the Eudrilidæ, and some remarks on Nemertodrilus, by Frank E. Beddard (Plates xvi.–xx.). *Hyperiodrilus africanus*, n. gen. and sp., and *Heliodrilus lagosensis*, n. gen. and sp., found in a Ward case from Lagos, at Kew Gardens.

THE only article of general interest in the *Nuovo Giornale Botanico Italiano* for April is a note on the stigmatic disk of *Vinca major*, by Sig. M. Pitzorno. In the reports of the Italian Botanical Society are short papers by Sig. Baccarini on the secretory system of the Papilionaceæ; on the arrangement of herbaria, by Sig. L. Micheletti; and others of special interest to Italian botanists.

SOCIETIES AND ACADEMIES.

London.

Royal Society, April 16.—"An Attempt to determine the Adiabatic Relations of Ethyl Oxide. Part I. Gaseous Ether." By Prof. W. Ramsay, F.R.S., and E. P. Perman, B.Sc.

The object of the research described in the memoir is the determination of the behaviour of ether in the state of gas approaching towards the state of liquid, when heat is communicated to it, so as to alter its condition adiabatically.

Previous researches by one of the authors in conjunction with Dr. Sydney Young have yielded data regarding the relations of pressure, temperature, and volume of gaseous and of liquid ether from which the values of the isobaric and of the isochoric differentials are obtainable. Such results lead directly to a knowledge of the differences between the specific heats at constant pressure and those at constant volume; and these differences are not constant, but vary with varying volume, pressure, and temperature.

The memoir contains an account of experiments made to determine the ratio between the specific heats at constant pressure and those at constant volume. The velocity of sound in gaseous ether was determined at various temperatures, pressures, and volumes; and by means of the isothermal differentials, and the experimental results for the velocity of sound, the ratios between the two specific heats were calculated. From the differences and the ratios of the specific heats, the values of the specific heats were deduced.

The general conclusion is that, for any constant volume, the specific heat, whether at constant volume or at constant pressure, decreases to a limiting value with rise of temperature, and subsequently increases; and that the change with temperature is more rapid, the smaller the volume.

At large volumes, the specific heats tend towards independence of temperature and volume, while at small volumes the influence of change of temperature and volume is very great.

The authors are at present investigating similar relations for liquid ether.

Zoological Society, April 21.—Prof. W. H. Flower, C.B., F.R.S., President, in the chair.—A communication was read from Lieut.-Colonel Sir Oliver B. C. St. John, R.E., containing notes on a case of a Mungoose (*Herpestes mungo*) breeding during domestication.—Mr. R. E. Holding exhibited and made some remarks on some remarkable horns of Rams of the domestic Sheep of Highland and other breeds.—Messrs. Beddard and Murie exhibited and made remarks on a cancerous nodule taken from the stomach of an African Rhinoceros (*Rhinoceros bicornis*), which had recently died, after living 22 years in the Society's Gardens.—Mr. E. T. Newton read a paper on the structure and affinities of *Trogontherium cuvieri*, basing his remarks principally on a fine skull of this extinct Rodent lately obtained by Mr. A. Savin from the forest-beds of East Runton, near Cromer.—Mr. H. J. Elwes read the first part of a memoir on the Butterflies collected by Mr. W. Doherty in the Naga Hills, Assam, the Karen Hills in Lower Burmah, and in the State of Perak.—Mr. J. J. Lister gave an account of the birds of the Phœnix Islands, Pacific Ocean, as collected and observed during a visit to this group made in H.M.S. *Egeria* in 1889.

May 1.—Sixty-second Anniversary Meeting.—Prof. Flower, F.R.S., President, in the chair.—After the auditors' report had been read, and other preliminary business had been transacted, the report of the Council on the proceedings of the Society during the year 1890 was read by Mr. Sclater, F.R.S., the Secretary. It stated that the number of Fellows on January 1, 1891, was 3046, and that the number of Fellows elected or readmitted in 1890 was 121, being 4 less than the corresponding number in 1889. Since the last anniversary 2 Foreign Members and 11 Corresponding Members had been elected to fill vacancies in those lists. In recognition of the effective protection accorded for sixty years to the Great Skua (*Stercorarius catarrhactes*) at two of its three British breeding stations—namely, in the Island of Unst by the late Dr. Laurence Edmondston and other members of the same family, and in the Island of Foula by the late Dr. Scott, of Melby, and his son, Mr. Robert T. C. Scott—the silver medal of the Society had been awarded to Mrs. Edmondston, of Buness House, as representative of that family, and to Mr. Robert T. C. Scott, of Melby. The total receipts of the Society for 1890 had amounted to £25,059, which, although not quite equal to those of 1889, had exceeded those of 1888 by upwards of £1000, and might be deemed to be satisfactory. The ordinary expenditure for 1890 had been £23,342 6*s.* 11*d.*, which was £659 2*s.* 8*d.* more than the corresponding amount for 1889. Besides this an extraordinary expenditure of £230 4*s.* 6*d.* had been devoted to the material improvement of the Monkey House, which brought up the total expenditure of the year to £23,572 11*s.* 5*d.* The balance brought forward from 1889 was £1242 13*s.* 11*d.*, and this, added to the income received in 1890, gave a total sum of £26,302 11*s.* 9*d.* available for the expenditure of the year 1890. By these means the Council had been enabled, after payment of the ordinary and extraordinary expenditures of the year, not only to devote the usual sum of £1600 to the reduction of the mortgage-debt on the Society's freehold premises (which at present amounted to £5000 only), but also to purchase a sum of £1000 in Consols, in order to form the nucleus of a new reserve fund. The usual scientific meetings had been held during the session of 1890, and a large number of valuable communications had been received upon every branch of zoology. These had been published in the annual volume of Proceedings for 1890, which contained 730 pages, illustrated by 57 plates. Besides this, part x., being the concluding part of the twelfth volume, of the Society's quarto Transactions had been issued. The twenty-sixth volume of the *Zoological Record*, containing a summary of the work done by British and foreign zoologists during the year 1889, had been issued to the subscribers in December last, and had thus been published before the close of the year after that to which it relates. The library had been kept in good working order during the past year, and had been much frequented by working zoologists. A large number of accessions, both by gift and purchase, had been received and incorporated. In the Gardens the only new work carried out in 1890 had been the completion of the improvements of the Monkey House, but a large number of repairs and renewals of the different buildings in the Gardens had been made, and other minor improvements had been carried out. The number of visitors to the Gardens during the year 1890 had been 640,987, the corresponding number in 1889 having been 644,579. The number of school children admitted free in 1890 was 35,935. The number of animals in the Society's collection on December 31 last was 2256, of which 693 were mammals, 1273 birds, and 290 reptiles. Amongst the additions made during the past year, twelve were specially commented upon as of remarkable interest, and in most cases representing species new to the Society's collection. About 28 species of mammals and 20 of birds had bred in the Society's Gardens during the summer of 1890. The report concluded with a long list of the donors and their various donations to the Menagerie during the past year.—A vote of thanks to the Council for their report was moved by Mr. W. H. Hudleston, F.R.S., seconded by Mr. A. J. Scott, and carried unanimously.—The report having been adopted, the meeting proceeded to elect the new Members of Council and the officers for the ensuing year. The usual ballot having been taken, it was announced that Mr. William T. Blanford, F.R.S., Dr. Albert Günther, F.R.S., Mr. E. W. N. Holdsworth, Sir Albert K. Rollit, M.P., and Mr. Howard Saunders, had been elected into the Council in the place of the retiring members, and that Prof. Flower, C.B., F.R.S., had been re-elected President, Mr. Charles Drummond, Treasurer, and Dr. Philip Lutley Sclater, F.R.S., Secretary to the Society for the ensuing year.—The remaining business having been concluded, the President handed the silver medal of the Society to Mr. Thomas Edmondston, who attended on the part of Mrs. Ursula Edmondston, of Buness, Unst, Shetland, and to Mr. A. P. Purves, who attended on behalf of Mr. Robert T. C. Scott, of Melby, Shetland, in recognition of the effective protection accorded by them and their families respectively to the Great Skua at its breeding places in the Shetland Islands.—The proceedings terminated with the usual vote of thanks to the President.

Geological Society, April 22.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read:—Results of an examination of the crystalline rocks of the Lizard district, by Prof. T. G. Bonney, F.R.S., and Major-General C. A. McMahon. The authors, in company with the Rev. E. Hill, spent a considerable part of last August in examining anew those sections in the Lizard district which had any bearing upon the questions raised since the publication of Prof. Bonney's second paper in 1883. They had also the advantage of occasional conference with Mr. Teall and Mr. Fox, whose valuable contributions to the knowledge of the crystalline rocks of this district are well known. That the Lizard serpentines are altered peridotites may be regarded as settled, but doubts have been expressed as to their relation to other associated rocks, and as to the meaning of a streaky or banded structure exhibited by certain varieties. The authors, after re-examination of a large number of sections, feel no doubt of the accuracy of their original view that the peridotite was intruded into the hornblende schists and banded "granulitic" rocks, after these had assumed their present condition. In it they find no signs of any marked pressure-metamorphism, either prior or posterior to serpentinization. They have failed to connect the streaky or banded structure with any foliation or possible pressure-structure in the schists, and they can only explain it as a kind of fluxion-structure, viz. as due to an imperfect blending of two magmas of slightly different chemical composition, anterior to the crystallization of the mass. The Porthalla sections have been examined with especial care, not only because the serpentine is nowhere so conspicuously banded, but also because its intrusive character has been denied, both it and the hornblende schists being ascribed to the alteration of a series of sedimentary rocks of suitable composition. For this view the authors have failed to discover any evidence, and consider it contrary to stratigraphical and petrographical facts. In regard to the genesis of the crystalline schists, which for purposes of reference were divided by Prof. Bonney into a "granulitic," a "hornblendic," and a "micaceous" group, the authors show that in parts of the first the more acid rock breaks through the more basic, as if intrusive, in others they appear to be perfectly interstratified, the one passing backwards and forwards, though rapidly, into the other. But between these extremes, intervals can be found where the two rocks seem as if partially drawn out together. The authors are agreed that certainly one, probably both, of these rocks are igneous, that when the basic rock was solid enough to be ruptured, the acid magma broke into it, and sometimes softened it sufficiently to allow of the two flowing for some little distance together, after which crystallization took place. In regard to the hornblende schists, the authors are not yet satisfied that either fluxion or mechanical crushing will account for every structure which they have examined, and prefer to leave the question, in certain cases, an open one. The most distinctive features of the micaceous group appear due to subsequent earth-movements, so that, though it exhibits some special characteristics, the authors are doubtful whether it is any longer worth while separating it from the hornblende schists. Of the igneous rocks newer than the serpentine, the gabbro has received the closest attention. It exhibits in places (especially in the great dyke-like mass at Carrick Luz) a very remarkable foliation or even mineral banding, which has been claimed as a result of dynamo-metamorphism. The authors bring forward a number of instances to establish the following conclusions:—(*a*) That this foliation occurs most markedly where the adjacent serpentine does not show the slightest sign of mechanical disturbance; (*b*) that it must be a structure anterior to the consolidation of the rock; (*c*) that it sets in and out in a very irregular manner; (*d*) that when it was produced the rock was probably not a perfect fluid. Hence they explain it also as a kind of fluxion structure, produced by differential movements in a mass which consisted of crystals of felspar and pyroxene, floating thickly in a more or less viscous magma. The authors' investigations tend to prove that (*a*) structures curiously simulative of stratification may be produced in fairly coarsely crystalline rocks by fluxioned movements anterior to crystallization; and that (*b*) structures which of late years have been claimed as the result of dynamo-metamorphism subsequent to consolidation must have, in many cases, a like explanation. This is probably the true explanation of a large number of banded gneisses which show no signs of crushing and holocrystalline, but in their more minute structures differ from normal igneous rocks. The authors have seen nothing which has been favourable to the idea that pressure has raised the temperature of solid rocks sufficiently to soften them. A discussion followed, in which Mr. Teall, the Rev. E. Hill, Prof. Hull, the President, General McMahon, and Prof. Bonney took part.—On a spherulitic and perlitic obsidian from Pilas, Jalisco, Mexico, by Frank Rutley.

Royal Microscopical Society, April 15.—Dr. R. Braithwaite, President, in the chair.—Mr. T. Charters White presented three slides of sections of teeth permeated with collodion.—A letter from Mr. J. Aitkin, of Falkirk, was read, on a spot-mirror method of illumination.—An abstract was read of a paper, by Surgeon V. Gunson Thorpe, R.N., on some new and foreign Rotifera found on the West Coast of Africa, and belonging to the genera *Trochosphæra* and *Floscularia*.—Mr. E. M. Nelson exhibited two forms of bull's-eye condenser—one made like Herschel's asplanatic, the other a new and simpler form, being made of two plano-convex lenses. This condenser seemed to answer its purpose admirably, the amount of spherical aberration being only about one-fifth of that which existed in the old form.—Mr. Nelson also read some further notes on Diatom structures as test-objects, which he illustrated by photographs.—Mr. C. Haughton Gill's additional note on the treatment of Diatoms was read, the subject being illustrated by photo-micrographs. Mr. Mayall said the problem Mr. Gill had endeavoured to solve was as to the existence or not of cellular structure in Diatoms extending through their substance, and he sought to demonstrate this by making chemical depositions which would probably fill up the cavities sufficiently to be distinguished by the microscope. Mr. Gill's observations were of great interest, because he had experimented with the definite purpose of testing a special point, thus applying to microscopy what Herschel would have termed an "experiment of inquiry"—a direct questioning of Nature on a point that had hitherto been regarded as almost beyond the sphere of experiment.

PARIS.

Academy of Sciences, April 27.—M. Duchartre in the chair.—The Secretary read an extract from the will of the late M. Cahours, and M. Janssen made some remarks upon the legacies left for the foundation of scholarships (see p. 17).—On the expressions of the pressures in an elastic homogeneous body, by M. H. Resal.—On the theory of elasticity, by M. H. Poincaré.—Researches upon humic substances, by MM. Berthelot and G. André. According to the observations of the authors, the humic substance formed by the action of hydrochloric acid upon cane sugar possesses etheric and anhydric properties, and is comparable in certain respects to the lactones.—On the origin of pus cells and on the *rôle* of these elements in inflamed tissues, by M. L. Ranvier.—On the performance of marine engines and that of screws, and on a geometrical method for calculating the first of these values without a dynamometer, by M. A. Ledieu.—Mica as an invariable dielectric, by M. E. Bouty. The author has previously shown that the capacities of mica condensers vary slightly with the duration of charging. He now finds that mica behaves as an invariable dielectric in a direction normal to the planes of cleavage—that is, the capacity (c) of a lamina of useful surface (ρ) and thickness (e) is represented by the formula $c = \frac{k\rho}{4\pi e}$, where k is a constant. It is remarked that the origin of the large variations of such condensers with duration of charging is the electrolysis of foreign substances contained in the superficial layers.—On an alternate current motor, by MM. Maurice Hutin and Maurice Leblanc.—Quantitative studies on the chemical action of light: first part—measure of physical absorption, by M. G. Lemoine. The action of light upon a mixture of oxalic acid and ferric chloride of various thicknesses and strengths is theoretically and experimentally determined.—Effect of the presence of halides of potassium upon the solubility of the neutral sulphate of potassium, by M. Ch. Blarez. Between 0° and 30° the solubility of K_2SO_4 in water is given in parts per 100 by

$$Q\theta = 8{\cdot}5 + 0{\cdot}12\theta.$$

On adding KCl, or other halide of potassium, at any definite temperature, the K_2SO_4 remaining in solution is given by the expression—

K_2SO_4 dissolved = a constant − the amount of K in added salt;

for any temperature this becomes

$$K_2SO_4 \text{ dissolved at } \theta^\circ = 7{\cdot}5 + 0{\cdot}1417\theta - \text{K of added salt.}$$

The precipitating action of the halides of potassium upon the saturated solution of the neutral sulphate of potassium is *proportional to the equivalent of the added salt*.—On iso-cinchonine, by MM. E. Jungfleisch and E. Léger.—On a hydrocarbon of the terpene series contained in the oils of compressed gas, by MM. A. Étard and P. Lambert. This is a pyropentylene not identical with valylene or pirylene; it polymerizes readily to $C_{10}H_{12}$. Its properties and relations with the terpenes will be given in a subsequent paper.—Researches upon trehalose, by M. Maquenne. Anhydrous trehalose is an oct-atomic alcohol isomeric with the saccharoses, and very near to maltose in chemical constitution; it yields glucose on inversion, and does not fulfil aldehydic functions.—On the constitution of aqueous solutions of tartaric acid, by M. Aignan. The author arrives at the conclusion that tartaric acid exists in aqueous solution in the state expressed by the formula $(C_4H_6O_6)_2$, partially dissociated according to a definite law.—Researches upon the artificial production of hyalite at the ordinary temperature, by M. Stanislaus Meunier.—On the stomachic digestion of the frog, by M. Ch. Contejean. Experimental evidence is given (1) that the pepsin secreted by the œsophagus is more abundant or more active than that of the stomach; (2) that the œsophagean and stomachic pepsins transform coagulated albumin into syntonin, and afterwards into peptone, without passing through the pro-pepsin stage; (3) that the predominance of the action of œsophagean pepsin on stomachic pepsin is especially manifest by the larger quantity of syntonin that it produces.—On the sexual evolution of the trouts of the Pyrenees, by M. A. Cannieu. The metamere of the endodermous layer and of the primitive circulatory system in the post-branchial region of Vertebrata, by M. F. Houssay.—Contribution to the study of the mechanism of urinary secretion, by M. O. van der Stricht. —Reappearance during winter of the starch in ligneous plants, by M. Émile Mer. The researches indicate that in ligneous plants starch is reabsorbed at the end of the autumn, and generated at the beginning of spring. It results from this that the winter, instead of being the season during which the amylaceous reserve is most considerable, is the season during which it is least.—On some points in the anatomy of the vegetative organs of Ophioglossa, by M. G. Poirault. The observations show that the Ophioglossum fungus is never reproduced by spores, but is propagated exclusively by buds on the roots.—On the existence of Diatoms in the lower lands of North France and Belgium, by M. L. Cayeux.—On the proportion of water in corn from different localities, by M. Balland.—On the treatment of phylloxerous vines by carbon bisulphide mixed with vaselines, by M. P. Cazeneuve.

BRUSSELS.

Academy of Sciences, February 7.—M. F. Plateau in the chair.—Micrographical researches on the nature and origin of phosphate rocks, by M. A. F. Renard. The author gives the preliminary results of some researches on the formation of phosphate rocks. The investigation has been especially directed towards the problem of the origin of these rocks, and some important conclusions are arrived at with regard to this point. A lithographic plate, containing magnified representations of nineteen phosphate chalk specimens, accompanies the paper.—The winter of 1890-91, by M. F. Folie. It is remarked that observations at Brussels show that the winter of 1890-91 is one of the severest passed during the last sixty years. Since 1833 seven winters have been of a severity comparable with the last. They are 1837-38, 1840-41, 1844-45, 1846-47, 1854-55, 1870-71, 1879-80. A table is given showing the mean minimum temperature and the mean temperatures experienced during these years. This comparison and a consideration of summer temperatures do not point to any particularly definite facts. The idea that a hot summer succeeds a rigorous winter does not appear to be supported. On the contrary, it appears that the coming summer should be more cold than hot, with the exception of the months of May and August.—On variations in the latitude of a single place, by M. F. Folie. The reality of the variations in latitude deduced from observations made at Berlin, Potsdam, and Prague, are contested on the ground of systematic errors in the formulæ of reduction, due to the assumption that the earth has been considered to move as a solid body, whereas M. Folie believes it to be composed of a fluid nucleus with a solid crust. —Researches on the development of Arachnætis: contribution to the morphology of Cerianthidæ, by M. E. van Beneden.—Researches on the velocity of evaporation of liquids at temperatures below their boiling-points, by M. P. de Heen. The first part of this paper was read at the January meeting. The results are now given of experiments on the variations of the velocity of evaporation with the hygrometric condition of the current employed. The whole of the observations show that the velocity of evaporation, v, of a liquid surface acted on by wind may be expressed by the formula—

$$v = \mathrm{AF}\,(100 - 0{\cdot}88f)\sqrt{\mathrm{V}}$$

where A is a constant, F the tension of the saturated vapour at the temperature of the liquid, and V the velocity of the current. —Determination of the radius of curvature in parallel coordinates, by M. Maurice d'Ocagne.

March 7.—M. Plateau in the chair.—On a curious peculiarity of currents of water, and on one of the causes of sudden floods, by M. G. van der Mensbrugghe. An explanation is given of the fact that in a river the maximum velocity of the current does not occur at the surface, but about three-tenths of the depth below the surface.—Reduction of nitrates by sunlight (second note), by M. Émile Laurent. The author has caused a beam of sunlight to fall upon solutions of nitrates placed in a vacuum, and has found that after a certain time the space contained liberated oxygen, whilst the liquids possessed the characteristic reactions of nitrites. M. Laurent has analyzed the oxygen and nitrites, and finds that the quantity of gas is sensibly proportional to the nitrite formed. As might have been expected, the blue end of the spectrum possesses the most powerful reducing action.—Note on the coagulation of the albumins of the serum of cow's blood, by MM. J. Corin and G. Ansiaux. The authors support the assertion made by Halliburton in 1883, that the albumin of serum ought not to be considered as a single substance, but as a mixture of two or three albuminoids, α, β, and γ, coagulating respectively at temperatures—$\alpha = 73^\circ$ C., $\beta = 77^\circ$ C., and $\gamma = 82^\circ$ C. The blood of man, the dog, pig, rabbit, &c., were known to contain these three substances, and it is now shown that the serum of the cow also contains the paraglobulin α, and the albumins β and γ. Further, it is shown that opalescence and coagulation are not distinct things, but two forms of one and the same phenomenon occurring at the same temperature.—On the curvature of polars with respect to a point on a curve of the nth order, by Prof. C. Servais.—Discovery of a variable star, by M. L. de Ball. An account is given of observations of a variable red star situated in R.A. 20h. 41m. 19s., Decl. $+ 2^\circ\ 2'{\cdot}3$ (1891). The observations extend from September 15, 1890, to January 9, 1891. In this time the magnitude of the star increased from 8·7 to 8. The star is not included in Bermingham's Red Star Catalogue. M. de Ball's observations are only eye-estimations, and have not been made by the aid of a photometer. Further evidence of variability is therefore required.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Lessons in Astronomy: C. A. Young (Arnold).—Practical Perspective: J. Spencer (Percival).—Revision or Examination Sheets; Subject II., Machine Construction and Drawing, Elementary Stage: A. G. Day (Percival).—General Physiology: Dr. C. Calleja (Kegan Paul).—Differential and Integral Calculus: A. G. Greenhill, 2nd edition (Macmillan and Co.).—Natural Selection and Tropical Nature: A. R. Wallace; new edition (Macmillan and Co.).—Fifth Report of the U.S. Entomological Commission: A. S. Packard (Washington).—Principles of Political Economy and Taxation: D. Ricardo; edited by E. C. K. Gonner (Bell).—L'Évolution des Formes Animales: F. Priem (Paris, Baillière).—Géologie, Principes—Explication de l'Époque Quaternaire sans Hypothésis: H. Hermite (Neuchâtel, Attinger).

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THURSDAY, MAY 14, 1891.

PRACTICAL GEOLOGY.

Aids in Practical Geology. By Grenville A. J. Cole, F.G.S., Professor of Geology in the Royal College of Science for Ireland. (London: C. Griffin and Co., 1891.)

An Introduction to the Study of Petrology: the Igneous Rocks. By Frederick H. Hatch, Ph.D., F.G.S. (London: Sonnenschein and Co., 1891.)

HOWEVER prophetic may have been the far-seeing premonitions of men in advance of their age in the dim past, and however invaluable may have been the additions made to the superstructure since, it can scarcely be doubted that the foundation-stones of geology were laid by Scotchmen and Englishmen towards the end of the last, and during the earlier part of the present century. And what a charm is there about the story of these sturdy pioneers, not perhaps quite the men whom one would have picked out as most fitted or most likely to become the fathers of a new science. It has about it the elements of a genuine romance. For the early training of few of these men was such as to give a scientific bent to their mind; they did not have what we are pleased to call "the advantage of a scientific education"; it is probable that they never spoke, perhaps never dreamed of, such a phrase as "the scientific method," which we are so fond of formularizing, and on which we plume ourselves somewhat. But in spite of these seeming drawbacks, rather perhaps because with these men genius was allowed to run its spontaneous untrammelled course, they opened out to mankind a domain of knowledge the very outskirts of which had been barely touched upon before. Of shrewd mother-wit were they; too keen of eye to be wrong about their facts; not a few were ardent sportsmen, and the same instinct which led them to ride straight to hounds or patiently and warily to stalk the deer, led them also, as they brushed away minor details, to go direct to main issues, and carried them on, without rest but without haste, through the toils of many a year's steady field-work. With what awe and reverence do we look up to these giants when we pass their achievements in review!

Nor does it one whit impair this feeling of respectful admiration to turn to the other side, and cast a glance at what were their unavoidable shortcomings. They were too hard-headed to be illogical in the matter of straightforward inferences, but it was hardly to be expected that they would escape going astray sometimes when they ventured on recondite speculation. Rough is not the word for their method; incomplete would be nearer the mark, but even that can scarcely be applied when the means at their disposal are taken into account. No one had yet taught the value of the microscope and balance to the geologist; and, when these and other instruments of precision were introduced, there was just a tendency to gird at appliances that had a finnicking look about them to Titans who had so long and so successfully relied on their hammers and their wits.

But by degrees it became clear in Germany, and later on in England, that, though the great main roads of the newly-discovered territory had been tracked out with such brilliant success, methods more refined than had sufficed for pioneering work must be introduced if all the intricacies of its lanes and by-ways were to be explored. Then the swing of the pendulum rather tended to bring about a disposition to exalt the new means of investigation, and there was just a risk that the sound basis of field-work might come to be undervalued if not neglected; and that Mineralogy and Petrology, instead of being the handmaids of Geology, might be thought to constitute the whole of that science. But the mischief never went far. The mantle which had fallen from the shoulders of the great fathers was not to be lightly cast aside; and, while every new aid was cordially welcomed, the conviction grew stronger and stronger that honest work in the field must for ever be the starting-point of geological inquiry.

How thoroughly this truth has become engrained in the minds of geologists is seen directly we open Prof. Cole's "Aids in Practical Geology." A large part of the book is taken up with minute and precise directions for carrying out the various kinds of microscopical, optical, and chemical examination of minerals and rocks. But on the first page we read—

> "Such aids in determinative geology as are given in the following pages may be applied in any halting-place, or in cities after the return from an expedition; but, in any case, observations made on specimens are of slight importance if uncoupled with knowledge of their true position in the field."

And again—

> "After a study of a number of type specimens, the student is recommended to go out to some well-described district, and to endeavour to recognize the varieties of igneous and sedimentary rocks by careful observation in the field. In this way alone can he appreciate the various modes of weathering, the massive or minuter structures due to jointing, the smooth or rugged outlines that characterize the masses of which his hand-specimens form a part. . . . Nothing short of striking the rock-mass *in situ* with the hammer, and taking in with the eye its position and surroundings, even to the broader features of the landscape, should content the geologist who would follow worthily the founders and masters of the science."

Again and again the author reiterates the lesson—

> "Just as no mountain mass can be described by a stranger from a number of hand-specimens, however beautiful, so no rock can be adequately described from isolated microscopic sections. Again and again the observer will pass from his section to the solid specimen, and from this, in memory at any rate, to the great mass of which it formed a part."

And in dealing with the nomenclature of igneous rocks, the chaotic state of which is so largely due to the ignoring of their field-relations, it is insisted that—

> "The following out of an igneous rock in the field is a most important lesson, and will soon determine what is valuable and what is valueless in any proposed scheme of classification."

That the author, in these and similar passages, is not speaking from hearsay, not merely re-echoing what is now a truism, is shown by the admirable practical directions which he gives in the first chapter for the outfit and procedure of the field-geologist. Here, and indeed throughout the book, the instructions are detailed and precise

The author has not forgotten the time when he was a beginner, his early failures, and the disappointments of his student-days, when, from the neglect of some slight precaution, he failed to obtain the results he had been led to expect; and he has used every means in his power, by minute and specific instruction, to shield those who use his book from similar mishaps. As an instance, take what he says about the effect of acids on minerals. How often has the self-taught man turned wearily to one book after another on mineralogy, in the hope of getting some definite information on this point, and all he arrived at was the curt statement, "Soluble in acids," which each apparently had copied from its predecessor, or all had borrowed from some common source. What acid? Concentrated or dilute? Cold or hot? Quickly, or perhaps only after a fortnight's boiling? All these points he was left to make out for himself as best he could. The happier pupil of Prof. Cole is treated far more liberally, and will not have to weary himself by feeling about in the dark if he attend to the cautions and instructions of the book now before us. The directions for blowpipe-work are equally precise. Only one who has been himself an actual worker would have told the observer to wait "till the first red glow has gone off" before noting the colour of a borax-bead. Of course, anyone would, sooner or later, find this out for himself; but, till he had found it out, he would probably blunder not a little; and anything that economizes time nowadays is not to be despised. There is no need to multiply instances; everyone who uses the book will find that it eminently deserves the epithet of "practical," which the author has assigned to it.

But are there no weak points on which the critic may exercise his function? Attention may perhaps be called to the following:—On p. 6, a graphical method, due to Mr. Dalton, is given for determining the full dip of a bed from the dips on two oblique sections. The writer may perhaps be pardoned for preferring a method of his own, given first in the *Geological Magazine* for 1876, p. 377. But, independently of any personal predilection, it may be said that the diagram in the case of this method is simpler than in that of Mr. Dalton. This makes it easier to recollect, and, besides, the fewer lines there are in a graphical construction the less is the chance of error. In dealing with "streak," it would be well to notice that the true streak of some hard minerals, Iron-glance for instance, is not obtained till they have been rubbed down in an agate mortar.

Doubt is thrown on the value of Turner's test for the detection of boron (p. 41): there is an article by Dr. C. Le Neve Foster in the *Mineralogical Magazine* (vol. i. p. 77) which should be consulted in this connection.

It is hardly worth while criticizing the nomenclature and classification of the crystalline rocks. No two petrographers are in agreement here, and probably the existing schemes of arrangement are all of about equal value. There is fortunately no multiplication of species or introduction of new names. It might be possible to take objection to the description of Quartz-felsite as a compact form of Granite, for the part played by the quartz in the two rocks is totally different, and must be correlated with a difference in their mode of consolidating. Quartz-felsites are specially common as dykes, and there may have been facilities for the escape of water in their case, up the fissures which they fill, that were not present in the case of the more thoroughly buried magma of Granite. It was doubtless the presence of water in the granite-magma which kept the quartz fluid or plastic after the other minerals had crystallized; its escape in the case of Quartz-felsite may have led to the early crystallization of the quartz. In dealing with the foliated rocks, the author touches on the debated point of the "true schists." We are pretty well used to this phrase, and have waited long in the hopes of being told what constitutes a "true schist," but our patience has not yet met with the reward it merits. The author is of opinion that "the alleged distinction between schist-like rocks and schists of pre-Cambrian age requires great delicacy of definition." This is delicately put, and will command the assent of most geologists.

The palæontological section will perhaps be looked upon somewhat derisively by those well versed in biology. But it will serve its end, which is to enable those who cannot pretend to any large amount of biological knowledge to know the commoner fossils when they see them, and determine the genus to which they belong. The method may have a large element of "rule-of-thumb" about it, it may be called empirical, but in a large number of cases it is not practicable to attain to anything better. And it has a certain educational value, for it makes a student use his eyes even if it but slightly disciplines his reason.

That the work deserves its title, that it is full of "aids" and in the highest degree "practical" will be the verdict of all who use it.

Nor will Dr. Hatch's handy volume be any less welcome. Those who wish to have in a compact form the prominent characters of the rock-forming minerals and the igneous rocks, will find all the information needed by a student concisely and lucidly put forth. Some slight acquaintance with crystallography and the optical properties of minerals is assumed. A short section on these subjects would have made the book more self-contained, and need not have increased its size very materially.

The igneous rocks are defined to be "those that have been formed by the consolidation of molten material." There is a spice of danger in the word "molten," for it may lead to the belief that the fluidity of the material was the result of "dry heat." In the case of a Laccolite the view so generally held is taken, that the overlying beds have been bent up by the intrusion of a molten mass. It is, to say the least, quite as likely that earth-movement caused a differential amount of bending in two adjoining beds, and that, as an empty space was thus gradually formed between the two, the molten matter was driven into it.

On the subject of the classification of the igneous rocks we find the following healthy expression of opinion: "The various types are so intimately related, that any attempt at rigid and systematic classification is not likely to meet with any great measure of success." Certainly not till some sounder basis of classification than any yet suggested is hit upon. In the meantime Dr. Hatch's grouping is one that from its clearness and simplicity will be a real boon to the student.

A most useful feature in the book is the list of localities where each rock occurs. The illustrations are very well executed. Though the book has appeared only recently, one teacher at least can already bear testimony, founded on actual experience, as to its value to students.

A. H. GREEN.

BACTERIOLOGY.

Les Virus. Par Dr. S. Arloing. (Paris: Ancienne Librairie, Germer, Baillière, et Cie., 1891.)

THE name of Dr. S. Arloing as the author of a work on bacteriology is a sufficient guarantee that the book is worth reading, nor are we disappointed. "Les Virus" is one of the best volumes on this science yet produced. It is not a mere compilation of other men's work, giving a categorical account of the numerous pathogenic and non-pathogenic bacteria now recognized, but is a thorough scientific investigation into the principles of one of the most important branches of medical science, and might perhaps be better called a manual of "microbiology."

The work is divided into six parts, under the following heads:—

(1) General considerations as to the nature of the bacterial poison.

(2) Form and mode of life of the microbes (biology).

(3) The part taken by the microbes in the propagation and spread of infectious diseases.

(4) Struggle of the host against the poison. Natural extinction and artificial destruction of its effects.

(5) Immunity enjoyed by the body against certain microbes.

(6) Attenuation and reproduction of the bacterial poison.

It will be seen by the above list that this work covers a large field, and one not exactly dealt with by any previous author.

In the first part, which is subdivided into six chapters, Dr. Arloing commences with an historical survey of the science of bacteriology, pointing out the gradual extension of ideas from the time of Rhazes, who, in the ninth century, attributed small-pox to a process of fermentation "comparable to that which takes place in the juice of the grape when made into wine"; touching then on the works of Rayer, Davaine, Chaveau, and others, the author traces the development of the science until present times and the discoveries of Koch and Pasteur. An interesting comparison of the "virulent" parasites with simple parasites, such as *Trichina spiralis*, then follows; and, next, the formulation of two statements which form the basis of the modern science: (1) the active agents of the virulent process are organisms; (2) these organisms are living, and possess specific properties.

The second part of the work deals with the biology of bacteria. The methods of cultivating them are fully described, and, what we do not remember to have seen in any other work on bacteriology, there is a full account of the effect on micro-organisms of nourishment, temperature, light, atmospheric conditions, and electricity. In this part, also, are two most important chapters—namely, the effects on the microbes of the nature of the cultivating medium. This is only just beginning to be properly understood, and its investigation has already been productive of valuable results.

The chapter on the products of the growth of micro-organisms is hardly up to the general excellence of the work. It has not been sufficiently brought up to date, so that the researches of Dr. Hankin, and the more complete investigations of Dr. Sidney Martin in reference to the albuminoses and alkaloids, do not appear in it. The diastases and ptomaines are, however, fully discussed, and much may be learnt from a perusal of this chapter.

The third division of the book is devoted to the *rôle* which the microbes play in the propagation and causation of disease. The chapter on contagion is one of the best in the book, and would alone form a most valuable *brochure*. After a consideration of the general modes in which contagion is carried, a most exhaustive account is given of air, water, soil, food, and artificial inoculation (vaccination) as carriers of disease. As a natural sequence, the modes of entry of the germs into the body are then described, auto-infection being included; and next we have a consideration of what may become of the organisms after their entry, and the changes which take place in the host. The descriptions here given are exceedingly precise, and, although rather condensed, convey all that can be desired.

Passing now to the fourth part, we find four chapters devoted to the strife between the host and the microbes, and the natural extinction and artificial destruction of the poison. In the third chapter the subject of disinfection is noticed, both by heat and antiseptics, special attention being drawn to the necessity of the careful disinfection of sputum, linen, bedding, &c.,—points which cannot be too strongly insisted upon in all hospitals, and not merely in those devoted to fevers or diseases of the chest.

The fifth part deals with the very difficult, and, at present, vague subject of "immunity." Dr. Arloing divides immunity into two classes—"acquired" and "natural." On this subject no one is more qualified to speak than the author of this work, for he has made it almost a special study for years, and it is treated of in his usual masterly way.

The sixth and last part contains some of the more recent researches (especially those of Pasteur) on the attenuation of the virus.

Taking the work as a whole, we cannot speak too highly of it. We heartily congratulate the author on the success of his labours. The book is well illustrated, and we cordially recommend it to all those who wish to study a subject so replete with interest and of such vital importance to mankind.

F. J. W.

OUR BOOK SHELF.

Anleitung zur Bearbeitung meteorologischer Beobachtungen für die Klimatologie. Von Dr. Hugo Meyer. (Berlin: Julius Springer, 1891.)

WERE this little book less severely technical in form, it might be commended to the notice of that large class of observers whose sole aim and object in meteorological registration is to ascertain the characteristics of the local climate and to compare them in detail with those shown by the similar records of other places. It teaches how the results of observation may be tabulated or graphically represented in the forms most approved by climatologists, and discusses with much precision the meaning of different kinds of mean values; though, indeed, it omits all mention of the geometric mean, the application of which in climatology was lately under discussion in the Royal Meteorological Society. But it is, we fear, hardly elementary enough to meet the requirements of beginners and amateurs, especially such as regard a formula of any complexity with something of that distant respect that they accord to holy mysteries; and on the other hand it aims at nothing beyond the formal and statistical presentment of facts, and never deviates into the seductive, if sometimes illusive, field of physical causation. It is what its title proclaims it to be, a guide to the working out of meteorological observations for the purposes of climatology—the climatology, that is to say, of the temperate zone. For those who work in a more extended field, some of the author's methods and dictates may be found to need modification. His schedule of the usual hours of observation makes no mention of those most frequently observed in the tropics, and his uncompromising condemnation of the use of Lambert's formulæ in reducing wind-registers, however justifiable in the case of the variable winds of these latitudes, ignores that of countries where trade-winds or monsoons blow steadily for weeks or months together with but little deviation from the normal quarter, and where the direction undergoes a regular oscillation daily. In working out this daily oscillation at such places, the use of Lambert's formula is not only justified but almost indispensable.

Within the somewhat narrow limits that Dr. Meyer has prescribed to himself, he has executed his task carefully and conscientiously, but in this country, at least, his merits are likely to be appreciated by only a small class; chiefly, indeed, by that estimable few who find in plodding labour its own sufficient reward. The student who is endowed with some share of scientific imagination, who loves to trace the inner workings of Nature, and sees in diagrams and tabulated statistics only means to this end, will find Dr. Meyer's work a somewhat dry study; and when he shall have mastered its contents, should he ever be challenged by Arthur Clough's "Questioning Spirit," and asked,

> "What will avail the knowledge thou hast sought?"

he must answer as he best may from his own mental resources. His author, at least, will not help him to a reply.

Intensity Coils: how made and how used. By "Dyer." Sixteenth Edition. (London: Perken, Son, and Rayment.)

In this book a simple and interesting account is given of galvanic batteries, induced electricity, and the methods of making and using intensity coils, which include numerous experiments that may be described briefly as "popular." In the present edition many other branches of the subject have been touched upon, including electric lighting, electric bells, electric telegraph, electric motors; and a few words are said on the telephone, microphone, and phonograph. Although the book is not presented as a scientific treatise, but simply as a guide containing the necessary instructions for making and using the above-named instruments, yet by its means many may be led to make a more advanced study of the subject, which to-day is of such high importance.

General Physiology. By Camilo Calleja, M.D. (London: Kegan Paul, Trench, Trübner and Co., Limited, 1890.)

The author of this book means by the word "physiology" "discourse of nature"; and his intention is to denote by it "the study of positive science in the abstract sense." The scheme he has set before himself is nothing less than "to comprehend under the fundamental principle of mechanism—conservation of energy—all the laws and theories concerning nature." In order to show the spirit in which he sets about the accomplishment of his task, it may perhaps be enough to say that he regards the planets as "bodies constituted of organic and inorganic matter," and that to him living organic matter seems "the proximate agent of planetary movements, for which non-living bodies are only the cosmic medium." The sun, we learn, is not "a body in combustion," but "principally a great reflecting mass, which, situated in the focus of the orbits of many planets, reflects their infra-luminous emissions, these producing light by their conglomeration." As for "natural light or daylight," it is "a photothermic radiation produced by transference, not only of the radiating motion of the planets, but also of the motion engendered by solar living beings." If anyone is attracted by writing of this kind, he will find plenty of it in Dr. Calleja's amusing volume.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

Co-adaptation.

It sometimes appears to me that the neo-Darwinians must speak a language of their own, because they are so fond of telling me, in a stereotyped phrase, that, "if words have any meaning," such and such words have expressed some meaning which no ordinary grammatical construction can extract. The present is a good case in point. Prof. Meldola says that he finds "a remarkable discrepancy" between my two previous letters on the above subject, and seeks to reveal it by quoting from the first letter, thus:—

"'I do not . . . hold myself responsible for enunciating Mr. Herbert Spencer's argument, which the quotation sets forth. I merely reproduced it from him as an argument which appeared to me valid on the side of "use-inheritance." For not only did Darwin himself invoke the aid of such inheritance in regard to this identical case, ...' &c. If words have any meaning, this implies that Dr. Romanes agrees with Darwin in regarding this case as one in which 'use-inheritance' played a part."

Does it? When a man says that in his opinion a certain argument in favour of a certain conclusion is valid, is this equivalent to his saying that he accepts the conclusion? And when he adds, twice over, that he purposely abstains from expressing any opinion of his own with regard to the conclusion, is this equivalent to his saying the precise opposite?

The state of the case is simply as follows. Prof. Meldola reproduced Mr. Wallace's argument against Mr. Spencer's defence of "use-inheritance." I wrote to show that this particular argument was invalid; but that there was another argument on the same side, which, if adduced, would be valid, *supposing that it could be sustained by facts.* Now, in his reply, Prof. Meldola abandoned the invalid argument, and adopted the one which I had stated. Accordingly I wrote a second time, in order to show that we were then agreed upon this being the only argument which could be logically brought against Mr. Spencer's position. But I again added that I would express no opinion as to whether this argument could be successful in subverting Mr. Spencer's position. In point of fact, with regard to *this* question I have no fully-formed opinion to express. But, unless the neo-Darwinians have eventually become unable to comprehend the attitude of "suspended judgment," one would suppose that they might still appreciate the difference between sifting arguments as good or bad on both sides of a question, and finally deciding with regard to the question itself.

Christ Church, Oxford, May 8. George J. Romanes.

I wrote in good faith when in my last brief communication I expressed the intention of allowing the subject to drop, because I considered that the discussion had arrived at a stage when those who were interested in the matter would be able to form their own opinion as to the value of the arguments adduced on either side of the question. I very much regret to find, however, that Dr. Romanes—whose amount of spare time appears to be most enviably inexhaustible—still finds it necessary to prolong the correspondence. I am compelled, therefore, to enter the field once more, if only for the purpose of presenting my own case in its true light. What Dr. Romanes's position may now be I must confess is becoming distinctly less clear with each of his contributions to the subject, but I am not the first who has lost his way in attempting to thread the mazes of this writer's productions. As far as I am concerned it will suffice to say that the case is not "simply" as he presents it in the foregoing communication. In the review of Mr. Pascoe's book, from which this discussion originated, I did not merely reproduce "Mr. Wallace's argument against Mr. Spencer's defence of 'use-inheritance.'" I accepted that argument as valid, but I extended it by emphasizing the importance of the factor of superimposed useful characters accumulated during successive periods of the phylogeny. I pointed out that large numbers of cases of co-adaptation might be thus accounted for, and I used Mr. Spencer's own illustration by way of example. In summing up his own conclusion, Mr. Wallace says: "The difficulty as to co-adaptation of parts by variation and natural selection appears to me, therefore, to be a wholly imaginary difficulty which has no place whatever in the operations of Nature" ("Darwinism," p. 418). Not only, therefore, has Dr. Romanes misrepresented my view, but he has gone further. The other "argument on the same side" referred to in the above communication is this very denial of co-adaptation as a fact in Nature. This, with most amazing *sangfroid*, is now claimed by my correspondent, who speaks of it as "the one which I had stated"! I must leave it to others to decide what value can be attached to the statements of a writer who adopts the principle of appropriating an argument, and putting it forward in a manner which would lead most readers to consider that he had been the first to elaborate it simply because he has expressed the same idea in abstract symbols instead of in concrete terms.

The next phase in the discussion is the admission by Dr. Romanes that Mr. Wallace's conclusion is correct, *i.e.* that co-adaptation is non-existent: "As it appears to me, from his reply, that Prof. Meldola's views on the subject of 'co-adaptation' are really the same as my own, I write once more in order to point out the identity" (NATURE, vol. xliii. p. 582). Mr. Romanes did more, therefore, than simply point out that we were agreed that this was "the only argument which could be properly brought against Mr. Spencer's position." He said that our views were "really the same," and this after I had accepted Mr. Wallace's conclusion as to the non-existence of co-adaptation. To crown all, he now tells us that he has no fully-formed opinion to express, but that he is in a condition of "suspended judgment"! I must really leave the case as it stands. If "neo-Darwinians" have a language of their own, at any rate it appears to be intelligible among themselves, if only from the circumstance that they have been enabled to stereotype a phrase which conveys their views with respect to the difficulty of following my correspondent's reasoning. I have been no more fortunate than other "neo-Darwinians" in this attempt, but in the endeavour to carry on the discussion of a biological question with a writer who stops short as soon as the subject assumes a truly biological aspect (see NATURE, vol. xliii. p. 582), I have become keenly impressed with the utter sterility of Dr. Romanes's method, which not only fails to advance our knowledge of the origin of species by any substantial contribution of fact, but which degrades the theoretical side of the subject into mere verbiage. If this is "palæo-Darwinism," I am rejoiced to think that I am grouped with those who are outside the pale.

In conclusion, to prevent further misunderstanding, let me add that, in admitting that the chances are "infinity to one" against a number of independent *useful* variations occurring when required in the same individual, I merely quoted the expression as given by Mr. Herbert Spencer and repeated by Dr. Romanes. I do not for a moment suppose that Mr. Spencer used the words in any more than a colloquial sense as indicating that there were "heavy odds" against such a combination, and in this sense only is my admission made. That the phrase has no exact mathematical significance is, I imagine, sufficiently obvious, but I have thought it desirable to make this qualification.

R. MELDOLA.

Physiological Selection and the Different Meanings given to the Term "Infertility."

IN the discussion concerning the segregation of varieties occupying the same region, and the influence of physiological selection in securing this result, it is necessary that we consider the different meanings given to some of the terms by different writers. The general fact on which Dr. Romanes insisted, in his paper on "Physiological Selection," was compatibility in the reproductive system of some, and incompatibility in that of others belonging to the same species. On p. 360 of his paper we read that "racial incompatibility," "however produced," "is the primary condition required for the development of varieties into species." Infertility and sterility are also used by him as equivalents for incompatibility in the reproductive system. Thus, on p. 400 we find the statement that "All natural varieties which have not been otherwise prevented from intercrossing, and which have been allowed to survive long enough to develop any differences worth mentioning, are now found to be protected from intercrossing by the bar of sterility—that is, by a previous change in the reproductive system of the kind which my theory requires."

Dr. Romanes did not attempt to catalogue the different forms of discriminative incompatibility that are included in the incompatibilities of the reproductive systems of different races, but reference was made to three forms: (1) to compatibility in the time of flowering in those of the same race, as contrasted with incompatibility in those of different races, as on pp. 352 and 356; (2) to greater numerical fertility when the male and female elements of the same race unite, than when those of different races unite, as in the note on p. 354; and (3) to numerical infertility through deficient production by hybrids, as on p. 369, and p. 357 in the note, and in the suggested experiments on p. 405, in which the pure and hybrid seed are both to be sown, and the comparative "degrees of fertility" to be noted. To these forms which were mentioned, we may add, as coming under the category of physiological incompatibilities, (4) lack of vigour in hybrids; (5) lack of adaptation in hybrids; (6) lack of escape from competition with kindred in hybrids; and (7) the superior energy and promptness with which the male and female elements unite in pure unions, as contrasted with cross unions. Dr. Romanes probably refers to this principle when he speaks of sterility as "failure to blend" (p. 365).

This last, when associated with the free distribution of the fertilizing elements, ensures the segregation (that is, the discriminative isolation) of two or more varieties occupying the same area and propagating during the same season, and therefore seems to me the most important of the forms of physiological segregation. This segregative principle, which I call potential or prepotential segregation, must, in almost every case, be operative between species and varieties that continue distinct while indiscriminately mingled on the same area and while fertilized by elements freely and indiscriminately distributed during the same season, for no other principle is able to secure free propagation and at the same time to prevent crossing under such conditions. Seasonal segregation is here excluded, and the other forms of physiological segregation when acting under such conditions are of little avail in preventing swamping unless carried to the extreme, and they then involve a waste of from one-half to the whole of the germs of the less numerous variety; for the most favourable case possible is when two varieties occupy the area in equal numbers, and such cases rarely exist, especially in the initial history of species.

Though numerical infertility and tardy potency are readily distinguished, complete impotence and complete numerical sterility are more likely to be confounded; for the complete incapacity of the male and female elements of different varieties for uniting involves failure to produce hybrids, as complete as when the elements unite without producing living offspring or germinating seed. The great difference is that in the case of cross impotence the germ remains unaffected by the alien fertilizing element, and therefore ready to be fertilized by any fertilizing element of its own kind that may reach it; while in the case of simple numerical cross sterility (if there be any such case) the alien elements promptly unite, and therefore leave no opportunity for subsequent fertilization by the coming of the kindred fertilizing elements. Cross impotence, with prepotence of pure unions when associated with the free distribution of the fertilizing elements, produces positive segregation; for, when characterizing varieties occupying the same area, it ensures the propagation of each with its own kind while preventing crossing; but numerical infertility of first crosses produces what I call negative segregation, for, though it is unable to secure segregate breeding, it lessens or obliterates the swamping effects of any crossing that takes place, and is therefore of great importance in the preservation of distinct varieties and species when the positive forms of segregation only partially prevent crossing. The four forms of hybrid inferiority mentioned above are also forms of negative segregation, and, though of the highest importance when co-operating with prepotential segregation or any other principle that partially prevents cross unions, are, it seems to me, incapable of preserving distinct varieties or species, when unassisted by any degree of positive segregation.

We are now prepared to see how the different meanings of infertility have occasioned more or less misunderstanding in the discussion of physiological selection and its effects. With Dr. Romanes, the seven forms of segregation above-mentioned are all forms of infertility between races, and therefore are all causes of physiological selection; while in my nomenclature, all but the first are included under impregnational segregation, and only the second and third are considered forms of cross infertility (or, in other words, of segregate fecundity). Using the term in this restricted sense, I have elsewhere maintained that it is very improbable that cross infertility is, in any case, the only isolative principle securing the continuance of distinct varieties and species indiscriminately commingled on the same area, even when the elements are freely distributed; and as this statement is liable to be taken as equally applicable to physiological selection, I wish to have it clearly understood that, in my usage, the two terms are not equivalent, and what I have said of cross infertility is not in the same sense true of physiological selection.

In Dr. Wallace's criticism of physiological selection, he seems to limit the meaning of infertility between races to numerical infertility of first crosses, and then assumes that this is the only incompatibility that is included under physiological selection. This limitation, if correct, would of course limit the effects that could properly be attributed to this principle.

Before closing I wish to raise the question whether a high degree of selective numerical fertility between races is not always associated with some degree of selective potential fertility. Or, using infertility in the more restricted meaning given in my nomenclature, is not a high degree of segregate fecundity and cross infertility always associated with some degree of segregate prepotence and cross impotence? As we know that these two forms of incompatibility are usually, if not always, associated in the segregation of species, is it not probable that they are similarly associated in the segregation of varieties? Again, as we know that segregate prepotence, when associated with the free distribution of the fertilizing elements, will produce prepotential segregation, effectually preventing crossing, without impairing powers of survival, and as there are many cases in which the continued segregation of varieties occupying the same area is due entirely to this principle, and many other cases in which it is due to weakened forms of this principle associated with other forms of incompatibility in the reproductive system, and still other numerous cases in which partial isolation (produced by a slight diversity of habits, or by the occupation of adjoining districts) would be speedily broken down except for these physiological incompatibilities, are we not fully warranted in the assertion that physiological selection is an essential factor in the evolution of many species?

The importance of this form of segregation having been recognized, the question naturally arises as to what have been the causes through which the incompatibility has ceased to be sporadic, and has become racial. As Dr. Romanes has not entered on the discussion of this point, I have given the more attention to it. I think I have succeeded in showing: (1) that any portion of a species subject to temporary isolation, through occupying a new station or district, is more or less liable to become incompatible with the rest of the species, owing to the cessation of reflex selection, by which the mutual fertility and other compatibilities of an inter-generating stock are kept in force (see NATURE, vol. xlii. pp. 28 and 369); (2) that partially segregative endowments are, through the very laws of propagation, cumulative (see "Divergent Evolution," Linn. Soc. Journ. —Zool., vol. xx. pp. 246–260); (3) that all the transformations that arise in forms thus segregated are inevitably divergent, and not parallel (see "Intensive Segregation," Linn. Soc. Journ.—Zool., vol. xxiii. pp. 312–322). JOHN T. GULICK.

26 Concession, Osaka, Japan.

Propulsion of Silk by Spiders.

THE author ("O. P. C.") of the article on "Arachnida" in the "Encyclopædia Britannica," says:—"The emission of silk matter appears to be a voluntary act on the part of the spider; but it is a disputed question among arachnologists whether spiders have the power forcibly to expel it, or whether it is merely drawn from the spinnerets by some external force or other. Mr. Blackwall, author of the 'History of Spiders in Great Britain and Ireland,' is of the latter opinion. Mr. R. H. Meade (Yorkshire) in Report of the British Association, 1858, thinks, that (from microscopic anatomical investigations which he has himself made) there is good evidence of spiders having the power to expel it; for he finds a certain muscular arrangement which would apparently suffice to give this power, and observers have actually seen the lines propelled."

Owing to the doubt herein expressed, may I ask your insertion of a chance observation lately made by me upon a spider, which has convinced me of the truth of the theory that spiders do expel their lines at will, and this, too, as secondary to one still remaining attached to the spinnerets?

She was hanging from the ceiling about 3 feet from a mullioned window, against which I was able to observe her movements most accurately. I was first led to observe her closely, by finding myself attached to her within one minute of my approach. On my breaking this line, she attempted to regain the ceiling; a breath of air from me stopped and brought her down again, when I saw her draw her legs together, pull her head up higher than the spinnerets of her abdomen by means of her ceiling-line, and, *following upon no visible effort of hers whatever*, I was the next moment conscious of the presence of another line stretching out from her spinners to a distance short of 3 feet, and at an angle of about 75° with the first. This line failing to find an attachment floated upwards and lay alongside of the other, and the spider again made for the ceiling. Nine times during the space of one hour, I got her to repeat this attempt to make a horizontal connection. Between two of the intervals of her attempts, I called in two naturalist friends who both witnessed with me, *and at the same instant of time*, the sudden appearance of the new line.

With each successive trial, I was able to substantiate and improve my observation; at first the appearance of the line seemed instantaneous, as to its whole length; next I was able to detect its elongation of itself after about 2 feet of its length was visible; then I could see it leaving the spinnerets; and finally, during the last moment of its travel, I could perceive very distinctly that it drew the spider slightly forward.

From these premises I can but infer that the viscid matter contained by the silk-glands, which, at the ordinary slow rate of emission, turns to gossamer immediately upon its exposure to the air, when expelled as now, violently, remains viscid sufficiently long to reach a certain distance.

These secondary threads, carried towards the ceiling by the spider, were never brought down again when she fell to the length of the main line, but were each time left, disconnected from her, at the spot from whence she fell when I blew her. Their loose end invariably floating upwards until alongside of the spider's main line, was, I think, noteworthy.

In conclusion, I would say that *sight* seemed to play no part in her choice of a direction for the connecting line; though I was close to her all the time, and indeed the only object apparently which was close enough, she only hit me the first time, when perhaps she had *heard* my approach; this may strengthen the remarks made by Mr. C. V. Boys in your number for November 13, 1890, where he says: ". . . . sight, as we understand the term, in spite of their numerous eyes, seems to be absent." S. J.

St. Beuno's College, St. Asaph, N.W., April 27.

The Crowing of the Jungle Cock.

I THINK there can be little doubt that Mr. H. O. Forbes has fallen into the same mistake as I had, in regard to Mr. Bartlett's statement that "none of the known wild species are ever heard to utter the fine loud crow of our domestic cock."

At first I took this to mean that the jungle cock did not crow *at all*, and was collecting notes from sporting men here, to supplement my own 26 years' experience, when yours of February 5 arrived, and by it I see that Mr. Bartlett implies that the crow is not so full, loud, and long, as that of our barn-door cock.

Mr. Forbes exactly gives the difference, as thinner, more wiry, and high pitched; it is also shorter, at least in the wild *G. ferrugina.* These I have often heard crowing, and shot in the extreme east of Asam, where for a very large area, on the Upper Diking River, and across Patkai, there are no inhabitants.

This same *G. ferrugina* is, however, to be found wild all over Asam, and the countries around; eggs found in the jungles are often hatched under domestic fowls, and hence these are frequently crossed, and the crow of the cock varies much in consequence.

But the difference between the wild *G. ferrugina* and our "barn-door" cock, in this particular, is so well marked that it could *invariably* be detected.

I may perhaps mention a curious sight I saw last year, within 100 yards of my bungalow, in the evening. A cloud of white ants were rising on the air, in the main road, and a jackal and jungle cock were busy eating the "neuters" swarming all over the ground; presently another jackal joined and the cock was between them: all were so busy feeding that they took no notice of each other, the jackals often lying on their bellies, while the cock moved about between them, at 2 or 3 yards only. By this time 15 or 20 people were looking on and laughing. Suddenly a third, younger jackal, joined the group, and after eating the ants a short time, and walking about like the others, dropped into the ditch and *stalked* the cock, crouching close to him. The latter at once flew, and made a bee line for the forest 400 yards off. The total area of the ants was about 20 feet by 8 only.

S. E. PEAL.

Sibsagar, Asam, March 27.

Antipathy [?] of Birds for Colour.

WITH regard to the destruction of the yellow crocus by the sparrow, mentioned by your correspondent "M. H. M." in NATURE, vol. xliii. p. 558, this bird appears to have a predilection for yellow. In an article on "Birds' Nests and Nest-building," in the *Animal World*, present number, an instance is given of sparrows using the flowers of the laburnum for their nest. Only lately I have been watching them picking out the yellow centres of the daisy, but in this case *it was for food*, and I am inclined to believe that some portion of the crocus is also eaten. At this time of the year they are well known to be partial to buds and flowers of different kinds—for instance, the blossoms of the gooseberry bushes.

Doubtless, the bright yellow colour attracts the attention of this now much censured bird, so omnivorous in his tastes and such a general scavenger, and therefore not wholly to be condemned.

Clevedon, April 28. T. B. J.

The Destruction of Fish by Frost.

REFERRING to Prof. Bonney's letter in NATURE, vol. xliii. p. 295, regarding the destruction of fish by frost, and in which he asks for information from more northern latitudes, I may say that during the winter of 1885–86, at Cape Prince of Wales, Hudson's Strait, when the thickness of ice in a small lake was being measured, live fish were often seen; and upon the last occasion, when the ice measured six feet and half an inch, several were thrown up with the water that, upon our cutting through, immediately overflowed. These fish were about an inch and a half in length and were extremely lively. I may add that during the summer both feeder and outlet of the lake averaged about eight inches in depth and the lake nine feet in its deepest part. The former ceased to flow on November 8, when, too, ice, fourteen inches in thickness, covered the lake.

F. F. PAYNE.

Meteorological Service of Canada,
Toronto, April 16.

The Flying to Pieces of a Whirling Ring.

WITH reference to the recent discussion in your columns on the whirling of steel bands, the following results will be of interest.

A weldless steel flask, with spherical body 12 inches in diameter and ⅜ inch thick, constructed for use in a centrifugal milk separator, to revolve about its axis of symmetry at a normal speed of 7000 revolutions per minute, was whirled at a gradually increasing speed, with a view to ascertaining the "bursting" velocity.

At 16,000 revolutions per minute the body of the flask had bulged 2 inches in diameter: this is equivalent to an extension of 17 per cent. of the circumference; the peripheral speed being 840 feet per second, and the tension 31·5 tons per square inch.

The experiment was not continued, as it was considered sufficiently satisfactory, and the bulged flask is kept as a curiosity.

CHAS. A. CARUS-WILSON.

McGill University, Montreal.

HERTZ'S EXPERIMENTS.[1]

III.

IN the last article the principles upon which a rapidly vibrating electric oscillator should be constructed were considered, and how the sudden break-down of the air gap enabled these rapid vibrations to be started. It is probable that this break-down occurs in a time smaller than the thousand millionth of a second. How very rapid interatomic motions must be!

Consider now the principles on which an apparatus is to be constructed to receive the vibrations produced by this oscillator. We may observe in the first place that as we are dealing with a succession of impulses at equal intervals of time we can utilize resonance to accumulate the effect of a single impulse. Resonance is used in an immense variety of circumstances to accumulate the effect of a series of impulses, and is avoided in another immense variety of circumstances to prevent accumulating the effect of a series of impulses. We see, we hear, we photograph by using it; we use it to make musical sounds, to keep clocks and watches going, to work telegraphs. By avoiding it carriages drive safely over rough roads, ships navigate the seas, the tides do not now overwhelm the land, the earth and planets preserve their courses round the sun, and the solar system is saved from destruction. Resonance may be thus described:—If a system is able to vibrate by itself in any way, and if we give it a series of impulses, each tending to increase the vibration, the effect will be cumulative, and the vibration will increase. To do this the impulses must be well timed, at intervals the same as the period of vibration of the system itself. Otherwise some of the impulses will tend to stop the vibration, and only some to increase it, and on the whole the effect will be small. In order to use resonance in the construction of the detector of waves of electric force, we must make our detector so as to be capable of an electric vibration of the same period as the generator of the waves. If we do this we may expect the currents produced in it to be increased by each wave, and thus the electrification at its ends to increase, and so increase the chance of our being able to produce a visible spark. Two ways of using a detector have been mentioned. One is to observe the heating of a conductor by the current in it, and the other to observe a spark due to the electrification at the end of the conductor. The latter is the most sensitive and has been most frequently employed, and is the method first employed by Hertz. Two forms of detector may be used for observing sparks. One form consists of a single conductor bent into a circle with its two extremities very close together. An electric charge can oscillate from one end of this to the other round the circle and back again. If the circle be the proper size, about 70 cm. in diameter for the large sized oscillator and about 8 cm. in diameter for the smaller sized one described in the last article, the period of oscillation of this charge will be the same as that of the charge on the generator of the waves, and its oscillation will be increased by resonance until, if the ends of the circular wire be close enough together, the opposite electrification of the ends will become great enough to cause a spark across the gap. The other form of detector depends on using two conductors, each of which has the same period of electric oscillation as the oscillations we wish to detect. These are placed in such a position that an end of one is near that end of the other which will at any time be oppositely electrified. For example, if the electric force in our waves be in vertical lines, then if we place two elongated conductors, one vertically above the other and separated by a very small air space, the electric force alternating up and down will cause currents to run up and down the conductors simultaneously, and the upper ends of both will be similarly electrified at any instant, while the lower end of the upper one will always be oppositely electrified to the upper end of the lower conductor, and if these two points, or two short wires connected with them, be close enough together, a spark will pass from one to the other whenever the electric force sets up these electric oscillations in the conductor. Thus this apparatus is a detector of the electric force. Whenever there is a spark we may be sure that there is electric force, and whenever we cannot get a spark we may be sure that there is either no electric force or anyway too little to produce sparks. The apparatus will be more sensitive for electric forces that oscillate at the same rate as the natural vibration of the electric charge on the conductor, because the effect of each impulse will then add to that of the last; resonance will help to make the electrifications great, and so there will be a better chance of our being able to produce a spark. We may weaken the strength of this air gap by reducing the pressure of the air in it. To do this the ends of the conductors, or wires connected with them, must lead into an exhausted air vessel, such as a Geissler's tube. There is no doubt that much longer sparks may thus be produced, but they are so dim and diffused that when dealing with very minute quantities of electricity those sparks in a vacuum are not more easily seen than the smaller and intenser sparks in air at atmospheric pressure. The additional complication and difficulty of manipulation from having the terminals in a vacuum are not compensated for by any advantages. This whole detecting apparatus works on somewhat the same principle as a resonator of definite size connected with one's ear when used to detect a feeble note of the same pitch as the resonator. Such a resonator might very well be used to find out where this note existed and where it did not. It would detect where there were compressions and rarefactions of the air producing currents of air into and out of your ear. In the same way the conductor sparking tells where there are alternating electric forces making currents alternately up and down the conductor, and ultimately electrifying the end enough to make it spark. In the sound resonator there is nothing exactly like this last phenomenon. We have much more delicate ways of detecting the currents of air than by making them break anything. If anybody would allow the electric currents from a Hertzian detector to be led directly into the retina of his eye, it would probably be a very delicate way of observing, though even in this direct application of the current to an organ of sense it is possible that these very rapidly alternating currents might fail to produce any sensible effect, for they are not rapid enough to produce the photochemical effects by which we see.

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[1] Continued from p. 14.

To recapitulate the arrangements proposed in order to detect whether electric force is propagated with a finite velocity, and if possible to measure it if finite. It is proposed to create electric oscillations of very great rapidity, oscillating some four or five hundred million times per second, and it is expected thereby to produce waves of electric force whose length will be less than a metre if they are propagated with the velocity of light. It is proposed to do this by causing an electric charge to oscillate backwards and forwards between two conductors, and across an air gap between them. This oscillating charge is to be started by charging the conductors, one positively and the other negatively, until they discharge by a spark across this air gap. By making the conductors small, and the distance the charge has to go from one to the other small, the rate of oscillation of the charge can be made as great as we require. If waves are produced by this arrangement, we can reflect them at the surface of a large conducting sheet, and then loops and nodes will be produced where the incident and reflected waves co-exist. The loops will be places where the alternating electric forces are great, while at the nodes there will be no electric forces at all. In order to detect where there are these alternating electric forces and where there are none, it is proposed to use either a single wire bent nearly into a circle, with a very minute air gap between its ends, or else two conductors placed end to end, with a minute air gap between their ends. In either case, if the natural period of vibration of a charge on the single conductor, or on each of the conductors in the second arrangement, is the same as the rate of alternation of the electric force we wish to detect, there may be sufficient electrification of the neighbouring ends to cause a spark across the minute air gap. We are thus in possession of a complete apparatus for determining whether electric waves are produced, and what their wave-length is.

The experiment is conducted as follows:—

The two conductors which are to generate the waves are placed, say, one above the other, so that the electric charge will run up and down in a vertical line across the spark gap between them. They might be placed horizontally or in any other line, but for definiteness of description it is well to suppose some definite position. We may call them A and B. They are terminated in polished knobs, between which the spark passes. A and B are connected with the terminals of a Ruhmkorff coil, or a Wimshurst or other apparatus by which a succession of sparks may be conveniently made to pass from A to B. Before the spark passes, A and B are being electrified, and when the spark occurs the electricity on A rushes over to B, and part of it charges B, while the electricity on B rushes across the spark, and partly charges A, this taking place alternately up and down. Each time there is less electricity, for some is neutralized during each oscillation by the opposite charge; for energy is being spent, some in overcoming the resistance of the spark gap, *i.e.* in producing the heat developed there, and some in producing electric waves in the surrounding medium. Thus the electric energy of the two oppositely charged bodies A and B is gradually dissipated, and one way of describing this is to say that the two opposite electric charges combine and neutralize one another. This whole language of talking of electric charges on bodies, and electric currents from one to the other, of electric charges neutralizing one another, and so forth, is not in accordance with the most recent developments of electro-magnetic theory. At the same time, those for whom these articles are written are familiar with this language, and with the view of the subject that it is framed to suit, while they are unfamiliar with ether electrically and magnetically strained and thereby the seat of electric and magnetic energy, and consequently it would have added very much to their difficulty in grasping the details of a complicated question if it had been described in unfamiliar terms, and from an unfamiliar point of view.

The electric force in the neighbourhood of the vertical generator will lie in vertical planes through it, and as A and B are alternately positive and negative, the electric force will alternately be from above downwards, and from below upwards. If, then, this force is propagated outwards in a series of waves, we may expect that all round our generator waves of electric force will be diverging; waves in which the force will be alternately down and up. The state of affairs might be roughly illustrated by elastic strings stretched out in every direction from our generator. If their ends at the generator be moved alternately down and up, waves will be propagated along the strings, waves of alternate motion down and up.

In order to reflect these waves, we require a metallic sheet of considerable area some two or three wave-lengths away from the generator; so far away in order that we may have room for our detector to find the loops and nodes formed every half wave-length where the outgoing waves meet those reflected from the screen. Not too far away, or our waves will be too feeble even at the loops to affect our detector. The waves are thrown off all round, but are most intense in the horizontal plane through the spark, so that our detector had better be placed as near to this plane as possible. The detector may be either a very nearly closed circle of wire, or two conductors, each somewhat longer and thinner than the combined lengths of the generating conductors, and placed vertically over one another, and separated by a minute air gap. As the theory of this latter form of detector is simpler than that of the circle, it will simplify matters to consider it alone. The two conductors should each have a period of electrical oscillation up and down it, the same as that of the charges on the generator. The generator consists of two conductors certainly, but then during the time the spark lasts they are virtually one conductor, being connected by the spark across which the electric charges are rushing alternately up and down. Hence the period of oscillation of the charges on the generator corresponds to that on a single conductor of the same size as its two parts combined. Various experiments have been made as to the best form for these conductors that form the detector. They might be made identical with the generator, only that the spark gap in the generator should be represented by a connecting wire. They may be longer and thinner. If longer, they should be thinner, or they will not have the same period of vibration. On the whole, the best results have been got with conductors somewhat longer and thinner than the generator. It is not generally convenient that the spark between the two conductors that form the detector should take place directly from one to the other. It is not easy to make arrangements by which distance apart of these conductors can be regulated sufficiently accurately. The most convenient way is to connect the lower end of the upper conductor and the upper end of the lower one each with a short thin wire leading, one to a fixed small knob, and the other to a very fine screw impinging on the knob. The screw may then be used to adjust the spark gap between it and the small knob with great accuracy. This spark gap must be very small indeed, if delicate work be desired. A thousandth of a centimetre would be a fair-sized spark gap. The minute sparks that are formed in these gaps when doing delicate work are too faint to be seen, except in a darkened room. Having placed the detector in position between the generator and the screen, the difficult part of the observation begins. It is heartrending work at first. A bright spark now and then arouses hope, and long periods of darkness crush it again. The knobs of the generator require repolishing; the spark gap of the detector gets closed up; dust destroys all working; and not without much patience can the art be attained of making sure of getting sparks whenever the conditions are favourable, though it is easy enough not to get sparks when the conditions are unfavourable. Before making any measurements, all this practice must be gone through. It is hard enough with the success of others before us to encourage us, with their advice to lead us, with a clear knowledge of what is to be expected to guide us. How much credit, then, is due to Hertz, who groped his way to these wonderful experiments from step to step, without the success of others to encourage him, without the advice of others to lead him, without any certainty as to what was to be expected to guide him. Patiently, carefully, through many by-paths, with constant watchfulness, and checking every advance by repeated and varied experiments, Hertz worked up to the grand simplicity of the fundamental experiment in electricity that is engaging our attention.

Having gained command over the apparatus, we may look about for places where sparks occur easily, and for others where they cannot be produced. Two or three places may be found where no sparks can be observed. These places will be found to be nearly equidistant. They are the nodes we are in search of. The distance between any pair is half the distance an electric wave is propagated during the period of an oscillation. Their presence proves that the electric force is not propagated instantaneously, but takes time to get from place to place. If the electric force were propagated instantaneously, there might be *one* place where the action of the currents induced in our reflecting sheet neutralized the direct action of our generator; but there could not be a series of two or more such places between the generator and the reflecting sheet. That there are more than one proves that electric force is propagated from place to place, and does not occur simultaneously everywhere. It sets the crowning stone on Maxwell's theory that electric force is due to a medium. Without a medium there can be no propagation from place to place in time. It only remains to confirm by calculation that the rate of propagation is the same as that of light. This is a complicated matter. It involves the question of how fast should, on any theory, the charge oscillate up and down a conductor. The problem has only been accurately solved in a few special cases, such as that of a sphere by itself. The conductors that have been employed are not this shape, are not by themselves, and so only rough approximations are possible as to the rate at which these oscillations occur. Knowing the wave-length will not determine the velocity of propagation unless we know the period of vibration; and consequently this direct measure of the velocity has only been roughly made; but it agrees as accurately as could be expected with Maxwell's theory that it must be the same as the velocity of light if electrical phenomena are due to the same medium as light. The conviction that more accurate determinations will confirm this agreement is founded upon safe ground. It was pointed out that the ether that transmits light and is set in vibration by the molecules of matter can hardly avoid moving them itself. This ether can hardly help having other properties than merely transmitting a comparatively small range of vibrations. It can hardly help producing other phenomena. When it has been shown that, if there is a medium concerned in conveying electric and magnetic actions, it must possess properties which would enable it to transmit waves like light; and when it has been shown that there *is* a medium concerned in conveying electric and magnetic actions, and that the rate at which they are conveyed is approximately the same as the rate at which light is propagated; the conclusion is almost unavoidable that we are dealing with the same medium in both cases, and that future experiments, capable of accurate calculation and observation, will confirm the conclusion that electric force is propagated through, and by means of, the luminiferous ether with the velocity of light. We really know very little about the nature of a wave of light. We know a great deal more about electric and magnetic forces, and much may be learnt as to the nature of a wave of light by studying it under the form of a wave of electric force. The waves produced by the Hertzian generator may be a metre long or more. The difficulty is to get them short enough. We know a good deal about how they are produced, and from this, and also by means of suitable detectors, we can study a great deal about their structure. They are truly very long waves of light. Atoms are Hertzian generators whose period of vibration is hundreds of millions of millions per second. A Hertzian generator may vibrate rapidly, but it is miserably slow compared with atoms. And yet the wonder is that atoms vibrate so slowly. If a Hertzian generator were, say, 10^{-7} cm. long, about the size of a good big atom, its period of vibration would be some hundreds of times too rapid to produce ordinary light. Atoms are probably complicated Hertzian generators. By making a complicated shape, as, for example, a Leyden jar, a small object may have a slow period of vibration. All that is required is that the capacity and self-induction may be large in comparison with the size of the conductor. We saw that these rapidly vibrating generators have but little energy in them: they rapidly give out their energy to the ether near them. This is also the case with atoms. These, when free to radiate, give up their energy with wonderful rapidity. How short a time a flash of lightning lasts! It is hardly there but it is gone: the heated air molecules have so suddenly radiated off their energy. The reason why atoms in the air, for instance, do not radiate away their energy like this is because all their neighbours are sending them waves. Each molecule is a generator, but it is a detector as well. It is kept vibrating by its neighbours: it occupies a part of the ether that is in continual vibration, and so the atom itself vibrates. As each atom can radiate so rapidly, it must be a good detector: its own vibrations must be very much controlled by the neighbourhood it finds itself in; and as the waves of light are very long compared with the distances apart of molecules, those in any neighbourhood are probably, independently of their motions to and fro, each vibrating in the same way. It is interesting to calculate how much of the energy in the air is in the form of vibrations of the ether between the molecules of air. A rough calculation shows that in air at the ordinary density and temperature only a minute fraction of the total energy in a cubic centimetre is in the ether; but when we deal with high temperatures, such as exist in lightning-flashes and near the sun, and with very small densities, there may be more energy in the ether than in the matter within each cubic centimetre. All this shows how wide-reaching are the results of Hertz's experiments. They teach us the nature of waves of light. We can learn much by considering how the waves are generated. Let us consider what goes on near the generator, consisting of two conductors, A and B, sparking into one another. Before each spark, and while A and B are being comparatively slowly what is called charged with electricity, the ether around and between them is being strained. The lines of strain are the familiar tubes of electric force. If A be positive, these tubes diverge from all points of A, and most from the knob between it and B, and converge on B. Where they are narrow, the ether is much strained; where wide, the ether is but little strained. Each tube must be looked upon as a tube of unit strain. The nature of the strain of the ether is not known; it is, most probably, some increased motion in a perfect liquid. We must not be surprised at the nature of the strain being unknown. We do not know the nature of the change in a piece of india-rubber when it is strained, nor indeed in any solid, and though the ether is much simpler in structure than india-rubber, it can hardly be wondered at that we have not yet discovered its structure, for it is only within the present century that the existence of the ether was demonstrated, while men have known solids and studied their properties and structure for thousands of years. Any way, there is no doubt that the ether is strained in these tubes of force when A and B are oppositely charged, and that the energy per cubic centimetre of unstrained ether is less than that of strained ether, and that the work done in what is called charging A and B is really done in straining the ether all round them. When the air gap breaks down, and an electric spark takes its place, there is quite a new series of phenomena produced. Suddenly, the strained ether relieves itself, and, in doing so, sets up new motions in itself. The strained state was probably a peculiar state of motion, and in changing back to ordinary ether a new and quite distinct state of motion is set up. This new state of motion all round the conductors is most intense near the spark, and is usually described as an electric current in the conductors and across the spark, or as a rushing of the electric charge from one conductor to the other. The electric current is accompanied by magnetic force in circles round it, and the tubes of magnetic force define the nature of the new movement in the ether as far as we know it. Hitherto, for the sake of simplicity, the existence of this magnetic force has been unnoticed. It is due to a peculiar motion in the ether all round what are called electric currents. The current in fact consists of little else than a line, all round which this movement is going on; like the movement surrounding an electrified body, but also unlike it. Whenever electric forces are changing, or electrified bodies moving, or electric currents running, there this other peculiar motion exists. We have every reason for thinking that this, which may be called the magnetic strain in the ether, as the movement all round electrified bodies was called the electric strain—that this magnetic strain only exists in these three cases: (1) when the electric strain is changing; (2) when electrified bodies are moving; and (3) when electric currents are running. These three may be all cases of one action: certainly the magnetic strain that accompanies each is the same, and it seems most likely that the electric change is only another aspect of the magnetic strain. There are analogies to this in the motion of matter that partly help and partly annoy, because they partly agree and partly will not agree with the etherial phenomena. Take the case described in a former article of a chain transmitting waves. Attention was drawn to the displacement of a link and to its rotation. Now for the analogy: to seem at all satisfactory the first thing that would strike one would be to pay attention to two *motions*, to the velocity of displacement of the link and to its rotation. This would lead to interminable difficulties in carrying out the analogy. We cannot liken electric strain to a velocity in this direct and simple way, because, what are we to do with a change in the strain which produces the same effects as a continuous current? A change in the strain is all very well, it would be like a change in the velocity, but what about a continuous change in the velocity: we can hardly suppose a velocity continually increasing for ever: we are evidently landed in immediate difficulties. It is better therefore to be content to liken the electric strain to a displacement of the chain link. It seems most likely that it really is a peculiar motion in the ether, but we must be content for the present with the analogy. If we want to drive it further, we must suppose stress in the chain that draws the link back to be due to a motion in the chain or of things fastened to it, and then the changed motions produced by a displacement of the chain might be analogous to the peculiar motions accompanying electric strain. It would lead us too far to work out this analogy. Returning to the simpler case of the displacement of the link representing electric strain, and the velocity of its rotation representing magnetic strain, see how the actions near a Hertzian generator may be likened to what takes place when a wave is being sent along a chain. While the conductors are being slowly charged we must suppose electric strain to be produced in all the surrounding space. This is a comparatively slow action, and as the rate of propagation is very rapid, the electric strain will rise practically simultaneously in the whole neighbourhood, and that it does so is a most important fact to be taken account of in all our deductions from these experiments. This slow charging must be represented by a slow raising of one end of the chain, which raises the rest of it to a great distance apparently simultaneously if the raising be done slowly. Suddenly the air gap breaks. This might be represented by lifting the chain with a weak thread, and by having the end of the chain fastened to a pretty strong spring. When the thread broke the spring would pull the chain back quickly, would pass its position of equilibrium, and thus commence a series of rapid vibrations on each side of this position; the vibrations would gradually die away owing to the energy of the spring being gradually spent, partly on friction in itself, and partly in sending waves along the chain. In actually performing the experiment, an india-rubber tube or limp thin rope is better than a chain when hung horizontally, as the chain is so heavy; when it can be hung vertically, a chain does very well. In the description it simplifies matters to describe a chain, because it is easier to talk of a link than of a bit of the rope: a link has an individuality that identifies it, while a bit of the rope is so indefinite that it is not so easy to keep in mind any particular bit. Consider now what these waves are, what sort of motion originates them. When the spring first starts, the near parts of the chain move first. What happens to any link? One end of it moves down before the other. What sort of motion, then, has the link? It must be rotating. Thus it is that change in the displacement is generally accompanied by rotation of the links. Thus it is that change in electric strain is accompanied by magnetic strain. The analogy goes farther than this. Each wave thrown off may be described as a wave of displaced or as a wave of rotating links, and the most displaced are at any time the most rapidly rotating links. Just in the same way, what have hitherto been called waves of electric force may also be looked upon as waves of magnetic force. Because there are two aspects in which the motion of the chain may be viewed does not diminish from the essential unity of character of the wave-motion in its waves; and similarly the fact that these Hertzian waves have an electric and a magnetic aspect does not diminish from the essential unity of character of the wave-motion in them. At the same time the two elements, the displacement of a link and the rotation of a link, are quite distinct things; either might exist without the other; it is only in wave propagation that they essentially co-exist. In the same way electric strain and magnetic strain are quite different things; though in wave-motion, and indeed whenever energy is transmitted from one place to another by means of the ether, they essentially co-exist.

FIVE YEARS' PULSE CURVES.

OVER five years ago it occurred to me that there would be considerable interest in keeping a systematic record for some time of the rate of pulsation, *i.e.* of the number of beats (per minute) of the pulse. I therefore commenced the practice by taking, every night, an observation of my own pulse; these observations, originally undertaken solely for my own personal interest, have been continued without intermission up to the present time; and, on throwing the results into a graphic form, I found so close a symmetry and concord between the curves for these five years, that I thought it might be interesting to readers of NATURE to have these results put before them.

First, then, as to the method adopted in these observations. I count the pulse beats for one minute[1] every night[2] before retiring to bed, and invariably while in a standing posture. From the records thus obtained the average for each month is deduced in the usual way, viz. by adding together all the numbers for the month, and dividing by the number of days on which observations were taken. With regard to this important consideration—*i.e.* of the number of observations, since an insufficiency thereof would, of course, greatly vitiate the value of my curves—I may state that during the first four years I omitted to take an observation on only seventeen nights altogether. During the fifth year, I find twenty-one observations missed. Nevertheless, the *net* data from which the curves are deduced are not quite so abundant as this statement would imply; for, in calculating the monthly average, I have invariably struck out altogether *all readings above* 79. My reason for this procedure was simply that I wished to obtain a curve showing the normal pulsations; now, anything much above 75 is abnormally high (especially in my own individual instance, for it will be noticed that my pulse is below the usual average of 70), and I can nearly always assign a distinct cause, such as the feverishness caused by a cold, or excitement, or recent exercise; it therefore appeared to me fairest to knock out altogether the results of such disturbing causes, and since for this purpose an arbitrary line must be drawn somewhere, I decided to draw it at 79. On the other hand, however, I have retained all the other readings, no matter how low they might be, although the "fifties" are very common, and occasionally even the "forties" have been touched. It might, perhaps, be thought that these very low readings should be neglected equally with the very high, but such a course appeared to me altogether illegimate, both because such low readings seemed, judging from their occurrence, to be, so to speak, *normally* caused, and unassignable to any distinct extraordinary cause known to myself, and also because I should hardly have known where to draw a minimum line. However, I now regret that the readings below 50, at any rate, were not rejected; but such readings are so extremely rare that they cannot have much influenced the curves. In order that the reader may judge for himself on what data these curves are founded, I have appended a table showing the *net* number of readings from which each monthly average was drawn, and have also stated (in brackets) the number of readings below 60 included in each month.

Turning now to the curves themselves, this monthly average is shown by the *thin* line. It is necessary to explain that these curves were drawn by marking the monthly average by a dot on the *extreme right* of each space representing a month. I was undecided for some time whether to adopt this plan or to mark this dot in the *middle* of each monthly space; but after trying both plans I concluded that now adopted to be the simpler. The actual curves were, of course, obtained by connecting all these dots by straight lines.

On examining this monthly curve, it is at once obvious that there is a strong similarity between the five years; clearly every year the curve falls through the spring, until about midsummer, and then rises wonderfully steadily and regularly in every case (except in 1889) through the autumn to November or December. On the whole, two maxima seem to be indicated—namely, one in November, followed by a fall, and then by a rise to another maximum in February or January. But it will be noticed that in the winter 1889-90 there is the unusual phenomenon of a fall through November, and then the two maxima are replaced by an intermediate maximum reached in December. So that here, in spite of the broad concord and regularity, there was rather too much local irregularity to be altogether satisfactory. In the lower portions of the curves, again, there is even more irregularity. Those of 1887 and 1888 (but emphatically the former) are indeed remarkably free from aberration; but in 1886 there is an extraordinarily abrupt and irregular rise through July, followed by a compensating fall through August. In 1890 there is an almost identical irregularity in the same two months; while in 1889 we have a remarkable irregularity in the spring. Now these irregularities puzzled me a good deal; still, in each case

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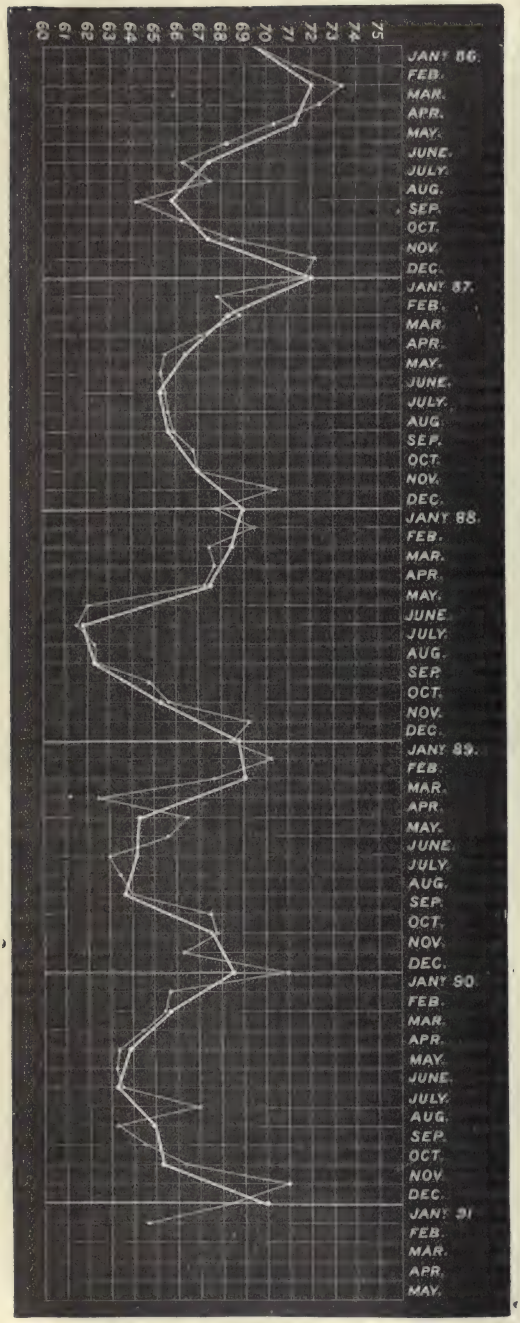
[1] Thus avoiding the considerable error that is introduced by counting for, say, fifteen seconds only, and multiplying.

[2] The time has varied from 23.0 o'clock to 1 o'clock.



(except that of the winter irregularity, 1889–90), I could assign a fairly plausible explanation. For instance, during the summer of 1886 I was under medical treatment: in July of 1890 I was touring among the Swiss mountains: while at the end of February 1887 I had removed from a low-lying northern suburb, to a rather higher southern one; this change might with some plausibility be considered as the possible disturbing cause in the 1889 spring curve.

Nevertheless, looking at the results as a whole, I was not satisfied with the curves: it appeared to me as by no means improbable that the monthly average was calculated on a rather too short period, thus allowing temporary disturbing causes to manifest themselves unduly. I therefore determined to try the effect of calculating the averages on a *two-monthly* period, throwing into one total January and February, March and April, May and June, July and August, September and October, November and December respectively. On drawing the curves corresponding to these averages (*thick*-lined curve), I was delighted to find order and symmetry completely regnant: all the aberrations have of course disappeared, and order is supreme. This two-monthly curve clearly shows a single maximum in winter, followed by a fall to the minimum at midsummer, and then by a rise to the winter maximum.

It is evident that the curves for all five years are very closely similar, though by no means identical[1] in nature; but I am especially anxious to point out the extraordinary symmetry displayed by the *curves on either side of a maximum or minimum* point. For instance, the curves for the following periods,

1886 July–October,
,, April–December,
,, November–February 1887,
1888 November–May 1889,
1889 The whole year,

are wonderfully symmetrical, in some cases even being almost geometrically exact.

What, however, may be the exact interpretation of these curves I must leave it to those better acquainted than myself with physiology to decide; but it is worth noting that these curves are exactly contrary to the statement in Michael Foster's text-book, that the pulse *is said* to rise in summer.[2]

The following is the table above referred to as showing the *net* data for each month, and also (in brackets) the number of readings below 60 included in each case:—

1886.		1888.	
January	28	January	25 (1)
February	19	February	23 (1)
March	21	March	24 (3)
April	25	April	30 (4)
May	30 (1)	May	30 (11)
June	29	June	30 (11)
July	30 (1)	July	31 (10)
August	28 (8)	August	28 (9)
September	29 (2)	September	28 (5)
October	29 (1)	October	31 (5)
November	25	November	26 (2)
December	23	December	28 (3)
1887.		1889.	
January	28 (2)	January	25
February	26 (1)	February	23 (2)
March	30 (3)	March	31 (9)
April	27 (4)	April	28 (8)
May	27 (3)	May	28 (3)
June	25 (5)	June	29 (10)
July	31 (3)	July	28 (4)
August	30 (6)	August	28 (7)
September	28 (2)	September	27 (4)
October	30 (2)	October	30 (2)
November	23	November	26 (4)
December	25 (2)	December	25 (1)

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[1] Seeing how very many causes must co-operate in producing the one resultant—of pulsation-rate—it would be very strange if the curves for different years *were* identical.

[2] I quote from the third edition.

1890.

January	27 (5))	July	24 (2)
February	24 (5)	August	30 (8)
March	28 (4)	September	29 (7)
April	28 (7)	October	24 (5)
May	27 (7)	November	22 (1)
June	26 (8)	December	30

If these numbers be compared with the curves, it will be found that in a rough way they agree with them; the diminishing number of these low readings every autumn, no less than their increase towards the summer, being obviously correlated with the rise and fall of the curves.

F. H. Perry Coste.

THE SCIENCE MUSEUM AND GALLERY OF BRITISH ART AT SOUTH KENSINGTON.

VIGOROUS protests continue to be made against the appropriation, for the new Gallery of British Art, of the site which ought to be used, as originally intended, for the Science Museum. Several letters on the subject by men of high authority have been printed in the *Times*; and on Tuesday a deputation, which could not but command attention and respect, waited upon Lord Cranbrook and Mr. Goschen to represent to them the opinions held by all who are in a position to form a trustworthy judgment on the question. The Government are still engaged in considering the matter, and it is to be hoped that they are receiving and giving heed to the counsel of their natural advisers, although, unfortunately, this is *a priori* extremely doubtful.

We print the letters addressed to the *Times* by Sir F. Bramwell, Mr. Poynter, and Sir J. Coode, and an account of the proceedings of the deputation on Tuesday.

It has for many years been recognized that the science collections at South Kensington are housed in a manner which largely diminishes their value for their principal use—viz. that in connection with the Royal Normal School of Science.

This school, as every one knows, is, as regards its main building, situated on the east side of Exhibition Road, while the collections are scattered about in the South Gallery and in the West Gallery adjacent to Queen's Gate.

In 1885 the Government appointed an inter-departmental committee to consider the subject and to report, and they nominated me, as being unconnected with any department, chairman of the committee. The committee (with one dissentient) reported in the sense that on the land lying west of Exhibition Road, and between that road and Queen's Gate, suitable buildings should be erected according to a complete design, but that they should be carried out in successive portions. Nothing was done on this report.

In 1889 another committee was appointed; this committee made very similar recommendations, and last year the Government acquired further land.

There are now on the west side of Exhibition Road, and immediately opposite the science schools, the observatories used by Mr. Norman Lockyer, and also a newly-erected physical laboratory.

Everything seemed to be, after all these years of waiting, in train for affording the needed accommodation, when, incredible as it must appear, the Chancellor of the Exchequer announced that the whole of this well-considered and satisfactory arrangement is to be given up. He stated it had been determined to sweep away the observatories and the physical laboratory, already on the west side of the road, and close to the science schools, and to devote this particular plot of ground to a picture gallery. I look upon this as a most disastrous proceeding, and one that, in the interest of the great National Department at South Kensington, should not be entertained for one moment.

Any one who will take the pains to visit the ground, or even to look at an accurate plan of it, will see that there is plenty of good space available for the picture gallery without interfering with the needs of the science collection, and that the notion of building it where proposed is so thoroughly preposterous that, as our American friends say, it must have originated in "pure cussedness."

Frederick Bramwell.

No. 5 Great George Street, Westminster, May 9.

Sir Frederick Bramwell in his letter of this morning points out the disastrous effect on the interests of the national Department of Science at South Kensington which will result from the intrusion of the new Gallery of British Art, to be planted precisely on the spot where it will cause the greatest amount of inconvenience. To an artist a still more flagrant instance of "pure cussedness" in this matter would appear to be that the building should be placed where it can have no connection with the existing galleries, when there is a piece of ground higher up the road in immediate connection with them.

The galleries on the east and west of the Horticultural Gardens, which were built for pictures at the time when there was a scheme for holding annual international exhibitions, are, whether by a happy "fluke" or by careful calculation on the part of their constructor, General Scott, without doubt the best lighted and the best proportioned picture galleries that have ever been constructed in England. Sir Frederick Leighton has, I know, expressed this opinion, and every artist who exhibited in these galleries during the three or four years that the exhibitions were held there will, I believe, agree in it: "We never saw our pictures look so well." These galleries are even now being connected by a building crossing the intervening space, the lower half of which will belong to the Imperial Institute, while the upper part is to be available for purposes of exhibition, thus making a connected group, and what would appear to be an unrivalled building for the purposes of a Gallery of British Art.

Why these buildings, acknowledged to be as good as they can be, and actually ready on the spot, should not be used for this purpose, according to what I understand was the original and nearly accepted scheme, it is somewhat difficult to understand. If the building for which £80,000 has been so liberally offered were placed higher up the road, above the Technical Institute, where there is a piece of ground available, it would back immediately on the Eastern Gallery, in which the Indian collection is now housed, thus affording provision for the extension of the collection, which is growing annually by the addition of the pictures purchased under the Chantrey bequest, and to which it is certain that further considerable additions will constantly be made by gift and bequest as soon as there is a place in which they can be properly and permanently exhibited.

Also, there is for once, if advantage be taken of it, an opportunity for carrying out a reasonable and consistent scheme for both science and art.

Edward J. Poynter, R.A.

28 Albert Gate, S.W., May 11.

Having served on the Committee on Machinery and Inventions in connection with the Science and Art Department of the Committee of Council on Education, I desire most emphatically to endorse the protest of Sir Frederick Bramwell which appears in your columns of this day's date.

Although the fees received from patentees up to the end of 1885 exceeded the expenditure of the Patent Office by upwards of 2½ millions sterling, nothing practically has been done to put the Patent Museum and Museum of Machinery and Inventions in an efficient condition.

Year after year the Committee, of which I am a member, has urged that more space should be given to the authorities at South Kensington, and now, when it was thought the recommendations were about to be realized, it is asserted that the promised site is to be devoted to a picture gallery.

I sincerely trust that this intention may not be carried out, but that the site in question, which exactly faces the Royal College of Science, will be appropriated for the science collections, to which purpose it has long been assigned.

Jno. Coode, President.

The Institution of Civil Engineers, 25 Great George Street,
Westminster, May 11.

The deputation which waited upon Lord Cranbrook, the Lord President of the Council, and Mr. Goschen was large and representative. Mr. Plunket, M.P., First Commissioner of Works, was also present. Among the deputation were: Sir William Thomson (President of the Royal Society), Sir Bernhard Samuelson, M.P., Sir George Gabriel Stokes (Past President of the Royal Society), Mr. C. Acland, M.P., Sir Frederick Bramwell, F.R.S., Prof. Story-Maskelyne, M.P., Sir Douglas Galton, C.B., Mr. Poynter, R.A., Prof. Unwin, Mr. Francis

Galton, Prof. Ayrton, Prof. Flower, C.B., Prof. Armstrong (Secretary of the Chemical Society), and Mr. Fletcher and Mr. Woodward, of the British Museum.

Prof. Story-Maskelyne, in introducing the deputation, in the absence of Sir Henry Roscoe (who is laid up with influenza), said it embraced a body of gentlemen distinguished not so much by their numbers as by their character, representing as they did the Royal Society and the scientific men of England. They had come there to ask that the question of the site of the new National Gallery for British Art should be reconsidered. Those who were deeply concerned in what he might call the new University which had risen for science at South Kensington felt that the proposed building would be a wedge put in between the place now occupied by it and the place dedicated to science. Scientific men would have to go across the road to get to another and interesting branch of the National Science Collection in a portion of the ground which would then be considerably remote from where they at present were. They understood it to be very much a question of money, and it was believed that the Government would have to ask Parliament to supplement the grant of £80,000 given by the anonymous donor. What he asked was that they should not be told offhand that the scheme could not be altered, but that they should be allowed to take the sense of Parliament as to whether the site was to be occupied in the way proposed or not. They objected to the money being simply asked from Parliament and the control taken out of its hands.

The Chancellor of the Exchequer.—You may entirely exclude that. That will not be done. We shall take such precautions by trustees and by contracts that such a contingency will not occur.

Prof. Story-Maskelyne said he was very glad to hear that.

Sir William Thomson, on behalf of the Royal Society, said they respectfully protested against the proposal to take the site now occupied by the physical laboratory of the Royal College of Science for any other purpose. Sixty pupils were now actively engaged. There was also a mining school in the same locality. No other sites could be as convenient as the site which those departments at the present occupied. It would be most fatal to the science work if the present arrangements were interfered with or the scientific collections, so conveniently arranged, were disturbed. Nor would the proposed site be the most convenient one for the pictures. A far better one would be that at present occupied by the School of Cookery, which, while affording ample room for the present proposal, would also be perfectly convenient for subsequent expansion in a direction that would result in the most admirable collection of picture galleries in the world.

The Chancellor of the Exchequer.—Can you tell us—for I have not yet been able to make it out—in what way the proposed arrangement would be fatal to the work of the College of Science?

Sir William Thomson.—By cutting the school in two—by separating the school from the place in which the instruments are kept.

The Chancellor of the Exchequer.—You mean that it is too far to walk?

Sir William Thomson.—It would be dreadfully risky to have to carry about delicate instruments.

Sir Bernhard Samuelson, as a member of a departmental committee which considered the question of housing the College of Science, supported Sir William Thomson's views, and pointed out that already there had been an encroachment upon the land which had been acquired for the purposes of the Science Museum.

The Chancellor of the Exchequer.—It was purchased for science and art. You do not contend that the whole of it should be devoted to science?

Sir Bernhard Samuelson said he did. He would like to ask the Chancellor of the Exchequer, after the assurance which he had just given that there would be no occasion to go to the House of Commons for a vote in aid of this work, whether he meant that, if there should be an expansion of the art gallery, some one would be ready to extend the munificence of the present donor.

The Chancellor of the Exchequer.—I think that that is rather a matter for our grandchildren. I think there is plenty of space to fill with worthy pictures for a very long time to come.

Prof. Story-Maskelyne.—But £80,000 will not do it.

Sir B. Samuelson said he hoped the question of the site would be reconsidered, and that those representing science should have the assurance that ample space would be given them not only for their present requirements but also for the extension which appeared to be looming in the future.

The Chancellor of the Exchequer.—I am anxious to provide well for science. We hope to bring science into one centre fronting the Imperial Institute.

Sir B. Samuelson said that if they were given an area equal to the amount purchased last year for the purpose of science alone they ought, in his opinion, to be content. But already there had been a small encroachment, and the fact of their having no actual claim to the ground would lead to further encroachment, which would, in the end, make it impossible for science to be efficiently provided for.

The Chancellor of the Exchequer.—I am anxious to show you that, quite irrespective of my interposition, we have not been blind to the interests of science, and that one of our plans has been to satisfy science in the most ample manner for the future.

Sir B. Samuelson said the art gallery was looked upon with a great deal of jealousy, and in the next place they feared that the full area of 200,000 feet, which they considered to be absolutely necessary for the future requirements of science, would be encroached upon.

Sir Frederick Bramwell, who was chairman of the departmental committee which considered the question in 1888, said there was a site to the north of the City and Guilds Institute, and from the east to the west there were galleries, and a cross gallery was being made by the Imperial Institute which would give communication one with the other, and which would be in immediate connection with the site he suggested. That would be an admirable art gallery. He would be glad to see the Science School and everything belonging to it moved so that there might not be a road dividing it. He trusted that the anonymous donor might be induced to see that his gift would prove more graceful if he did not impose a condition that would have so prejudicial an effect as would be the case if the recommendations of the two committees he had referred to were disregarded.

Lord Cranbrook.—The question, of course, so far as it can be considered will be considered, and I quite agree with Sir Frederick Bramwell that nobody can predict what may be done hereafter. You may have a scheme which, in itself, is a good one, but which may possibly have to wait. But in the meantime I can assure you that the interests of science will be most carefully considered, and that we will do what we can in order to further them.

Prof. Story-Maskelyne, having thanked Lord Cranbrook and Mr. Goschen for the hearing that had been given to their views,

The deputation withdrew.

We have received the following communication on this subject:—

Sir,—The curious admissions made by Mr. Goschen to the deputation which waited upon him and the Lord President indicate very clearly that we have, in the present muddle touching the site of the Art Gallery, another of those instances in which we suffer from the system, or, rather, want of system, which is characteristic of the relation of Government to science, and from the absence of scientific knowledge in those branches of the public service by which matters of the highest scientific moment are settled. A reference to some of the facts will, I think, show this very clearly.

The particular site which has been allocated by the Government in this way for the purposes of an art gallery forms part of a piece of land which, as is well known, only last year was deliberately purchased by the same Government for scientific purposes—to be quite accurate for "science and the arts"—that is, science and its manifold applications. *The space of ground thus purchased was less than half the space allotted to the Natural History Museum.* I say deliberately, because the purchase of the land in 1890 had for its object the carrying out of one of the recommendations of the Duke of Devonshire's Commission, which dates from the year 1874—namely, the erection of a Science Museum.

This object so warmly commended itself to the Royal Commissioners of the 1851 Exhibition that in 1876 they offered the land on which the Imperial Institute is now being erected and a sum of £100,000 towards its realization. Few acquainted with the manners and customs of our Government Departments in relation to science will be surprised to hear that this magnificent offer was refused; and it is to prevent a like disastrous mistake being now made that the strong memorial was presented to Lord Salisbury.

The ideal arrangement for a great national collection of scientific apparatus which is to do for the sciences of experiment and observation what the British Museum does for literature and antiquities, the Natural History Museum for biology, and the National Gallery for art, is that it shall be in close connection with laboratories where the apparatus can be used, presided over by experts who are familiarly acquainted with its construction and uses.

This was the ideal recommended to the Government by the Duke of Devonshire's Commission in 1874, and such is the ideal now being carried out by several of our provincial Colleges.

As all Londoners know, at present the Science Schools and the collection of scientific apparatus, which are both necessary for the realization of this scheme, are placed one on the east side of Exhibition Road, and the other chiefly in the Western Galleries. If the apparatus is employed in teaching, it must necessarily be transported about a quarter of a mile and back from the one to the other. And this accounts for the strange processions occasionally met in the neighbourhood of the Museum carrying delicate apparatus along the street alike in wet and dusty weather.

When the new piece of land was purchased last year on the recommendation of a very strong Treasury Committee, it was naturally expected that, as the overcrowded state of the existing school buildings rendered immediate action imperative, plans would be at once drawn up for an extension in the closest possible contiguity with the present building—that is to say, on the part of the newly-acquired plot immediately fronting it.

It was also believed that the Science Museum would be built in close and organic relation with the new laboratories, and that a scheme would be initiated which would supply pressing needs, and could, in course of time, be developed into the ideal institution which has been sketched.

These plans, to the carrying out of which the friends of science confidently looked forward, would be rendered absolutely futile by the grant for art purposes of the particular plot the alienation of which from the use for which it was purchased will render the objects of its purchase nugatory.

All hope of a compact site, therefore, for the future worthy representation of physical science would disappear as the result of this action of the Government.

The public have a right to know who is responsible for this, and how far the scientific officers of the Science and Art Department have been consulted. If they have in any way been consenting parties, it seems probable that they will have a *mauvais quart d'heure* with their scientific brethren who have signed the memorial and who attended the deputation; if they have not been consulted, the whole transaction is a disgrace to our administrative system.

An idea of the *impasse* in which this decision has landed matters scientific at South Kensington was to be gathered from one of Mr. Goschen's replies as to the makeshift arrangements at first proposed:—

(1) The second half of the Science Schools is to be built somewhere at the back of the new Art Gallery. This at once prevents all close relationship between the two halves of the same institution.

(2) The scientific apparatus is to be distributed in galleries which, although built for artistic purposes, are not considered good enough for art.

These, I presume, are the Western Gallery, the present *terminus a quo* of the processions to which reference has been made, a corresponding Eastern Gallery, now occupied by the Indian Museum, and the upper part of a new gallery, also designed for art, situated between the Imperial Institute and the Royal College of Music. All these galleries are as far removed as the limits of the Government estate will permit from the Science Schools, with which they are supposed to be in organic connection.

It appears, therefore, that the provision to be made for the Science Museum, which ought to rank, and in the future must rank, with the British Museum, the National Gallery, and other like institutions, is that the two halves of the Science Schools are to be widely sundered, while any organic connection with the Science Museum is to be rendered impossible.

I do not think, Sir, I need occupy any more of your space with recent history; the whole question stands thus:—

(1) In our museum system Art, Antiquities, Literature, and Natural History are magnificently provided for.

(2) Science is not provided for at all in any permanent manner.

(3) During the last twenty years Royal Commissions, Treasury and Departmental Committees without number, and deputations, have pointed out this gap.

(4) Last year the Government bought, and the Royal Commissioners for the Exhibition of 1851 sold cheap, a plot of land to be used for this purpose, and for this purpose alone.

(5) The plot is less than half of that on which the Natural History Museum stands.

(6) The Government now barter away a large portion of this small site for a mess of pottage.

I am, Sir,
Your obedient servant,
F.R.S.

NOTES.

THE ladies' *soirée* of the Royal Society will take place on Wednesday, June 17.

ON Tuesday the Convocation of the University of London considered the Draft Charter drawn up by the Senate. A resolution to the effect that the scheme should be approved was moved by Lord Herschell, seconded by Sir Richard Quain, and supported by Dr. Pye Smith. Mr. Bompas, Q.C., Mr. R. H. Hutton, and others spoke on the other side. In the end the scheme was rejected, 461 voting against it, and only 197 recording their votes in its favour. The whole subject needs to be thoroughly reconsidered, as the question of the higher teaching, one of the points first insisted on, seems to be dropping out of view. To educationists this is, of course, the really important element of the subject; and it cannot be for ever tolerated that the existence of an Imperial Examining Board, because it has been wrongly named, should prevent the largest city in the world from securing educational advantages which have for centuries been possessed by many a small German town.

THE Government of New South Wales have granted for the purposes of the Sydney Biological Station a plot of land of two acres on the north shore of Port Jackson at a part where the littoral fauna is particularly rich, and where the conditions are in other respects highly favourable. The Royal Society have made a grant of £50 towards the cost of the proposed new station.

THE annual meeting of the German Ornithological Society is being held this year at Frankfort, and the attendance is somewhat larger than usual, as several ornithologists have stopped at Frankfort on their way to the Congress at Budapest. The subject of zoological nomenclature was considered on Tuesday, when a discussion on the rules proposed by Dr. Reichenow and Graf von Berlepsch ensued. The question will be further considered at the forthcoming Ornithological Congress at Budapest, where Dr. Reichenow will be the exponent in the systematic section.

THE *conversazione* of the Society of Arts will be held at the South Kensington Museum on Wednesday evening, June 17.

M. EDMOND BECQUEREL, son, and successor as Professor, of Antoine César Becquerel, died on Monday, in Paris, at the age of 71. He was the author of treatises on the solar spectrum, the electric light, magnetic phenomena, and other scientific subjects.

PROF. JAMES GEIKIE, of the University of Edinburgh, has been delivering a course of lectures at the Lowell Institute, Boston, on Europe during and after the Ice Age. The course began on March 13 and ended on April 10.

A SHOCK of earthquake was felt at Athens on Monday evening.

THE fourth summer meeting of University Extension and other students, to be held at Oxford in August, will be divided into two parts. The first part of the meeting will begin with an inaugural lecture by Mr. Frederick Harrison on Friday evening, July 31, and will end on Tuesday evening, August 11. The second part of the meeting will begin on Wednesday morning, August 12, and end on Monday evening, August 31. In natural science fifty-nine lectures will be delivered, and there will be classes for practical work in the University laboratory and observatory, &c. Among the scientific lecturers will be Mr. E. B. Poulton, Prof. A. H. Green, Mr. W. E. Plummer, and Mr. C. Carus-Wilson. Scholarships to the value of £120 have been offered by various gentlemen for the purpose of enabling University Extension students, who would not otherwise be enabled to afford it, to study for a short time at Oxford.

A GLASS case just placed in the Mammal Gallery of the British Museum contains a series of specimens of two of the largest species of Asiatic Wild Sheep, collected and presented to the nation by Mr. St. George Littledale, the well-known sportsman. Three of these represent Marco Polo's Sheep (*Ovis poli*) from the Pamir Range, and three of them the Ammon (*Ovis ammon*) of the Altai. These are, we believe, the first perfect specimens of *Ovis poli*, the finest and largest of all the Asiatic Sheep, that have yet been brought to England, the species been generally known only by its horns, which are remarkable for their enormous size and width.

THE Australasian Association for the Advancement of Science has published the Report of its second meeting, held at Melbourne in January 1890. The volume is edited by Prof. W. Baldwin Spencer. No one who glances over the volume can fail to recognize that the Association is likely to exercise a most important influence on the development of scientific research and thought among our kinsfolk in the Australasian colonies.

THE Ealing Microscopical and Natural History Society, of which the Rev. G. Henslow is President, has issued its Report and Proceedings for 1890. The Committee are able to record that the work of the Society proceeded quietly but steadily on the lines laid down in previous years; the evening meetings, the excursions, and the *conversazione* having all been held in their appointed seasons, and having had a full measure of success. Among the subjects brought before the evening meetings were "Adventures in Siberia," by Mr. H. Seebohm; "The Natural History of Malta," by the Rev. G. Henslow; "Diatoms," by Mr. E. M. Nelson; and "A Gossip on Mushrooms and Toadstools," by Dr. M. C. Cooke.

DURING the last fortnight, according to the Cairo correspondent of the *Times*, there have been in Upper and Lower Egypt large swarms of locusts, which have caused much alarm, as it is believed that they originate from eggs laid in the country last year. The damage done to the young maize, sugar, and cotton is as yet insignificant, though some individual growers have had to re-sow cotton patches which had been devastated. The provincial Mudirs have received orders to do everything in their power to secure the extermination of the locusts. The correspondent says that this is the most serious reappearance of an old Egyptian plague that has been recorded for about forty years.

A CIRCULAR relating to certain alterations in the Science and Art Directory for the session 1891-92 has been issued to managers of schools of science and art by the Lords of the Committee of Council on Education. The following is an outline of the alterations, so far as they refer to science, or to science and art together:—(1) Subject 6—Theoretical Mechanics—will be treated in two subdivisions: (*a*) the mechanics of solids, and (*b*) the mechanics of fluids—liquids and gases—payments being made on each subdivision as a separate subject. Subject 8—Sound, Light, and Heat—will be treated in three subdivisions in the advanced and honours stages, which may be taken, and will be paid upon, separately. The elementary stage will still include all three subjects, but the syllabus will be curtailed and rendered easier, especially in "Sound." (2) These subdivisions will not be considered as separate subjects in the interpretation of the rule which limits the number of subjects on which payments may be made on a student in any one year. (3) The number of National Scholarships in science to be competed for each year will be increased from 14 to 22. (6) In both science and art, the prizes of books, as distinguished from certificates, will be largely reduced in number, and only given in competition; those prizes which are now awarded simply on the student attaining a certain standard of excellence in the examinations being abolished. The time has passed when such prizes from a central authority, which entail a disproportionate cost and delay in administration, were justified by the necessity for stimulating science and art schools; and the Lords of the Committee of Council on Education are of opinion that the scholarships which will be substituted for them will be more useful. They trust that those interested in education in the several localities will themselves provide prizes of books for deserving students which may be useful to them in their studies.

ACCORDING to the Indian papers, a persistent effort is being made by the Geological Department of the Government of India, in association with the Burmah Government, to explore the tin resources of Tenasserim. The flourishing condition of the almost adjacent Malay States of Perak and Selangor, which are under British protection, is mainly due to the income derived from tin royalties. A year ago an expert was borrowed from the Straits Settlements and placed in Tenasserim under Mr. Hughes, of the Geological Department. The party has this year been joined by Dr. Warth, the officer who did very good work for the Government in the Punjab salt mines; and Dr. King, the Director of the Department, has left Calcutta for an inspection of the survey operations which have been conducted during the last twelve months. It is now two years since the Chief Commissioner of Burmah sent a special officer to report on the tin mines of the Straits Settlements, and the present explorations are being conducted in pursuance of the recommendations then made.

A PASSAGE in the correspondence of Leibnitz and John Bernoulli, to which Prof. Hellmann has recently called attention in the *Meteorologische Zeitschrift*, indicates that Leibnitz conceived the idea of the aneroid barometer, which was first practically realized by Vidi in 1847. Bernoulli, early in the eighteenth century, was considering the phosphorescence of mercury in the barometer, and the possibility of making a new instrument which would give the variations of air-pressure on a larger scale; also the idea of a barometer for travellers; and Leibnitz tells him he had thought of a portable barometer, without mercury, in which a metallic case should be compressed by the weight of the air. A bladder, or leather case, which he also suggested, Bernoulli considered would be too hygroscopic.

MESSRS. MACMILLAN AND CO. have just published "Natural Selection and Tropical Nature—Essays on Descriptive and Theoretical Biology," by Mr. Alfred Russel Wallace. The volume consists mainly of a reprint of two well-known volume of essays—"Contributions to the Theory of Natural Selection," and "Tropical Nature and other Essays." Several essays have been either wholly or in part omitted. On the other hand, the author has included essays on the antiquity of man in North America, and on the debt of science to Darwin, which have hitherto been accessible only in the periodicals where they originally appeared. The text has been carefully corrected, and some important additions have been made.

A SUPPLEMENT to Dr. T. Lauder Brunton's "Text-book of Pharmacology, Therapeutics, and Materia Medica" has been issued by Messrs. Macmillan and Co. It presents the additions made in 1890 to the British Pharmacopœia of 1885. Although the medicinal substances contained in the British Pharmacopœia of 1885 are considered in the body of the work under the natural divisions of the mineral, vegetable, and animal kingdoms to which they belong, the author thinks it is easier to remember the additions by grouping them together according to their uses. A complete alphabetical list of them is also given.

A "BOTANICAL ADDRESS-BOOK" has been issued by the well-known Leipzig publisher, Wilhelm Engelmann. It contains a list of living botanists, and of botanical institutions, societies, and periodicals.

F. A. BROCKHAUS, of Leipzig, has issued a catalogue of scientific works which are offered for sale at his establishment. It includes, besides books, a large number of scientific periodicals and the publications of many learned societies.

THE 92nd and 93rd Parts of the "Länderkunde von Europa," edited by Alfred Kirchhoff, have been published. They present an excellent account of various parts of the Balkan Peninsula.

WILLING'S (late May's) useful "British and Irish Press Guide" for 1891 has been published. This is the eighteenth annual issue.

THE first number of a monthly journal for civil, mechanical, and electrical engineers, was published last week. The new journal is called the *Engineering Review*, and is edited by Mr. H. C. E. Andrée and Mr. Edward Walker.

AT the meeting of the Linnean Society of New South Wales on March 25, the Rev. Dr. W. Woolls read a paper on the classification of Eucalypts. After critically reviewing the characters of Eucalypts which have, from time to time, been made use of for classificatory purposes, more particularly those of the anthers and of the bark as set forth in the anthereal and cortical systems of Bentham and Mueller, the author suggested the probable value of a classification based on the characters of the fruit—such as shape, position of the capsules, the number of cells, and the appearance of the valves, &c.

CAPTAIN PETERSEN, of the Swedish barque *Eleanora*, noted a submarine earthquake in the volcanic region of the Atlantic west of St. Paul Rocks on March 13 between 7 and 8 p.m. According to a statement in the printed matter prepared for publication on the Pilot Chart of the North Atlantic Ocean for the present month, the ship was heading north-west, going about 3 knots, with a light easterly wind and calm sea, when a noise was heard on the port side, like a heavy surf, and almost immediately the sea began to bubble and boil like a huge kettle, the broken water reaching as high as the poop-deck. No distinct shock was felt, but after the disturbance struck the ship she continued to tremble as long as it lasted. After about an hour it ceased for an hour, and was then followed by another similar disturbance. A bubbling sound was all that could be heard, and the water appeared foamy, but it was impossible, on account of the darkness, to say whether it was muddy. The next day weather and sea were as usual. Position at 8 p.m., lat. 3° 47′ N., long. 42° 03′ W. The region from St. Paul Rocks to and including the Windward Islands is especially subject to earthquakes, and reports similar to the above are often received.

AT the ordinary meeting of the Institution of Civil Engineers on May 5, Mr. William Langdon read an interesting paper on railway-train lighting. He pointed out that the main questions to be determined were whether electricity was safe, trustworthy, and less costly than other illuminants. The fact that electrically-lighted trains had now been running for a considerable period without accident appeared to him conclusive evidence of its safety, and experience had shown that there was no reason to doubt its trustworthiness where efficient provision had been made; and he believed that when the cost of applying any of the illuminants, whether oil, gas, or electricity, to a complete railway system was taken into account the latter would be found the most economical. Regarding electricity as the illuminant which would, at no distant date, be universally employed for train lighting, Mr. Langdon suggested the desirability of arriving at a common basis with regard to the following fundamental points: (1) electrical system; (2) form and position of the electrical couplings; (3) pressure of current. Unless this was effected it was to be feared that unnecessary difficulties might be created by the diversity of the plans adopted.

MR. C. J. HANSSEN, a civil engineer of Copenhagen, has proposed a new international system of measures and weights, to which he invites our attention. He hopes that England will adopt his system, and that then the United States and Russia will follow, and thus the new system would become entirely international. Mr. Hanssen proposes that the English foot should be increased in length by about 1/2500th part of its present length (from 1·00000 to 1·000403); the pound avoirdupois, the ounce, and the imperial gallon, remaining unaltered. The cubic foot, as Mr. Hanssen states, would then contain exactly 1000 ounces of distilled water at 4° C.; and its intercomparison with the metric units of weight, length, and volume, would become apparently easy. We fear, however, that there is little hope in this country of introducing any such new system. As Mr. Chaney has indicated in his report on the Metric Conference, there are only two things possible in the metrology of this country: either to adhere to the present Imperial system, or to introduce the metric system. No half-way or modified Imperial system, such as Mr. Hanssen would propose, appears to be possible.

THE Deutsche Seewarte has published, in vol. xiii. of its *Aus dem Archiv*, a paper by Captain C. H. Seemann, one of the assistants in that establishment, entitled "Weather Lexicon: an Index to the European Weather Charts from 1876–1885." The author considers that the principles we at present possess for forecasting the weather—*e.g.* Buys-Ballot's law, the relation of the tracks of depressions to the distribution of pressure and temperature, or the dependence of the lower air-currents upon the upper currents—are not sufficient for the purpose, and he has made an index of the various similar types of weather-charts. He has calculated the barometrical differences which occur each day in three directions: (1) from Hamburg towards the north-west (Stornoway); (2) from Hamburg to the south-west (Biarritz); and (3) from Hamburg to the north-east (Helsingfors); and, by knowing the difference for any day, a reference to a table of such differences shows the dates of other charts with similar conditions, so that, by selecting one which appears most suitable to the present conditions, we may judge of the probable weather from that which actually followed that particular type. In the paper in question, only barometer and wind have been taken into account; the distribution of temperature would, of course, have great influence upon the changes of weather, but the author preferred to postpone the consideration of that element in this primary classification.

In the new number of the Journal of the Bombay Natural History Society, Lieutenant H. E. Barnes continues his interesting papers on nesting in Western India. Speaking of house-sparrows, he says that no amount of persecution seems to deter them from building in a place when they have once made up their minds to it. At Deesa, he found that a pair had built a large nest in the antlers of a sambur in the veranda. Another pair made a nest in the soap-box in the bath-room, and although the nest was destroyed several times, they would not desist, and at last, "from sheer pity," he had to leave them alone. The most peculiar case was when a pair had a nest in a bird-cage hanging against the wall, just above where the "durzi" sat all day working, and close to a door through which people were passing in and out continually. The door of the cage had been left open, the previous occupant having been transferred elsewhere. Not only were four eggs laid, but the nestlings were reared, although the cage was frequently taken down to be shown to visitors. Once the eggs were nearly lost, a boy having taken them out. The fuss made by the birds led to the recovery of the eggs. The author has a curious note on another peculiarity of sparrows. "I have often," he says, "had to turn the face of a looking-glass to the wall to prevent them from injuring themselves, for immediately one of them catches a glimpse of himself in it, he commences a furious onslaught on what he imagines must be a rival, and, if not prevented, will continue fighting the whole day, only leaving off when darkness sets in, recommencing the battle at dawn the next day. I once tried to see how long it would be before the bird gave in, but after two days, seeing no likelihood of his retiring from the unequal contest, I took pity on him and had the glass covered up. The bird did not seem in any way exhausted, although I do not think that he had a morsel of food for two days."

Some remarkable electrical phenomena accompanying the production upon the large scale of solid carbon dioxide are described by Dr. Haussknecht, of Berlin, in the current number of the *Berichte* of the German Chemical Society. In order to obtain large quantities of solid carbonic acid it is found most convenient in practice to allow the liquid stored in the usual form of iron cylinder to escape into a stout canvas bag, best constructed of sail-cloth or some such strong fabric, instead of the usual lecture-room receiving apparatus, the cylinder being inclined from the vertical so as to permit of a ready and uniform exit from the opened valve. The liquid under these circumstances issues at pressures varying from 60–80 atmospheres, and a compact snow-like mass of solid carbon dioxide is formed in the canvas receiver, owing, as is well known, to the extreme lowering of the temperature of the liquid due to its sudden expansion and the accompanying absorption of heat. When the experiment is performed in the dark, the canvas receiver is seen to be illuminated within by a pale greenish-violet light, and Dr. Haussknecht states that electric sparks 10–20 cm. long dart out from the pores of the cloth. If the hand is held in these sparks the usual pricking sensation is felt, similar to that perceived on touching the conductor of an electric machine at work. Dr. Haussknecht further states that the phenomenon is very noticeable in the dark whenever there is a leakage in any portion of the compressing apparatus or the manometers connected therewith. The reason assigned for this development of statical electricity is similar in principle to that usually accepted in explanation of the hydro-electric machine of Sir William Armstrong. As the liquid carbonic acid is issuing from the valve it becomes partly converted into gas which is violently forced through every pore of the canvas. Moreover, carried along with this stream of gas are great quantities of minute globules of liquid, which are brought in forcible contact with the solid particles already deposited. Dr. Haussknecht therefore considers that the electrical excitation is due mainly to the violent friction between these liquid globules and the solid snow. It is very essential for the successful reproduction of these electrical phenomena that the carbon dioxide should be absolutely free from admixed air; that prepared artificially yielding much finer results than that obtained from natural waters, which latter contains considerable quantities of air. The luminosity is not generally developed in the interior of the receiver until a crust of solid carbonic acid 0·5–1 cm. thick has been deposited, which renders the probability of the correctness of the above theory all the greater. Dr. Haussknecht has constructed a special form of apparatus, with which he is now experimenting, with the view of being able to determine the sign, nature, and quantity of the generated electricity.

The additions to the Zoological Society's Gardens during the past week include two Brown Capuchins (*Cebus fatuellus* ♂ ♂), an Ocelot (*Felis pardalis*), a Coypu (*Myopotamus coypus*), two Ring-tailed Coatis (*Nasua rufa*), two Cayenne Lapwings (*Vanellus cayennensis*), seven Burrowing Owls (*Speotyto cunicularia*) from South America, presented by Mr. James Meldrum; a Pig-tailed Monkey (*Macacus nemestrinus* ♀) from Java, presented by Mr. C. Powell; a Common Hare (*Lepus europæus*), British, presented by Mr. H. T. Bowes; three Pintails (*Dafila acuta* ♂ ♂ ♀), European, a Mandarin Duck (*Æx galericulata* ♀) from China, presented by Mr. G. F. Mathews, R.N., F.Z.S.; a Common Boa (*Boa constrictor*) from South America, presented by the Directors of the Museum, Demerara; two Cheer Pheasants (*Phasianus wallachii* ♂ ♀) from Northern India, twelve Common Teal (*Querquedula crecca*, 4 ♂, 8 ♀), European, purchased; a Viscacha (*Lagostomus trichodactylus*), a Red Kangaroo (*Macropus rufus*), born in the Gardens.

THE IRON AND STEEL INSTITUTE.

On Wednesday and Thursday of last week the annual spring meeting of the Iron and Steel Institute was held. The gathering was announced to extend over Friday also, but for some reason, best known to those who had the control of the meeting, the second day's proceedings were so hurried through that all the business was disposed of by half-past one o'clock on the second day; no less than six papers being taken at the one sitting. Naturally there was very little discussion; and indeed the second day of the meeting might almost as well have been dispensed with, and copies of the papers given to members to take home to read at their leisure. It is seldom that we have been present at a duller gathering than that which the meeting became towards its close, there not being a dozen members present to hear the Secretary hurry through the papers one after another, the President apparently being only anxious that there should be no discussion to prolong the proceedings.

The following is a list of the papers read:—On the manufacture of war material in the United States, by Mr. W. H. Jaques, of Bethlehem, U.S.A.; on tests for steel used in the manufacture of artillery, by Dr. Wm. Anderson, Director-General of Ordnance; on certain pyrometric measurements and the method of recording them, by Prof. Roberts-Austen, F.R.S.; on the changes in iron produced by thermal treatment, by Dr. E. J. Ball, London; on a graphic method of calculating the composition of furnace charges, by Mr. H. C. Jenkins; on economical puddling and puddling cinder, by Prof. Thomas Turner, Birmingham; on the micro-structure of steel, by M. Osmond, of Paris. There were three other papers which were not read.

Upon the members assembling in the theatre of the Institution of Civil Engineers, which was lent for the occasion by the Council of the latter Society, according to their hospitable custom, Sir James Kitson, the retiring President, occupied the chair. After the usual formal business had been transacted, the new President, Sir Frederick Abel, F.R.S., was duly installed, and at once proceeded to deliver his inaugural address. Sir Frederick is also this year President of the British Association, and should spend a busy autumn attending both the meeting of the Iron and Steel Institute in Birmingham, and of the Association in Cardiff. The address was of considerable length, embracing a wide range of subjects and a long span of time. The duration of Sir Frederick Abel's official life has been long, exceptionally long for the years he has lived, for he obtained employment in the Government service at an early age. It was shortly after the outbreak of the Russian War that he succeeded the illustrious Faraday in the Professorship of Chemistry at the Royal Military Academy, and since then he may be said to have seen almost the whole history of the birth and subsequent growth of applied science in connection with the industries of iron and steel making. At the beginning of his career, he tells us in his address, those who, in this country, appraised at their proper value the services which the analytical and scientific chemist could render to the iron-master and manufacturer of steel might be counted on the fingers. Systematic mineral analysis was just in process of application, volumetric analysis was altogether in its infancy, and spectroscopic analysis was not even dreamt of. The metallurgic operations in the Arsenal at Woolwich were limited to the production of small castings of brass for fittings of gun carriages, and to the casting of bronze ordnance for field service. Our supplies of cast-iron ordnance for siege and naval use were drawn from a very few of our most renowned iron-works, and our shot and shell were exclusively supplied from private works. What Woolwich has become since those days—and in spite of its faults of administration it is something of which the country may be proud—and how large a part Sir Frederick has borne in this development, most of our readers must be well aware. In those days our most powerful guns were 8-inch smooth bore 68-pounders of cast-iron, weighing 95 hundredweight, and fired with a charge of 18 pounds of powder. Now we have the 110-ton breech-loading rifled gun, built up of steel hoops and tubes, the calibre of which is 16¼ inches, and which throws a steel projectile weighing 1800 pounds with a powder charge of 960 pounds. Notwithstanding the fact that the 110-ton gun is in advance of its time—our mechanical skill and engineering knowledge not yet being sufficient to properly carry out the design—it would be difficult perhaps to find a more striking example of the application of scientific principles to the industrial arts; although we must not forget that the credit of the advance is due rather to Elswick than to Woolwich.

Leaving the region of historical retrospection, the address makes reference to the proposal of Prof. Langley, of Michigan University, made at the last Bath meeting of the British Association, that a series of samples of steel should be distributed between the metallurgical experts of different countries, in order that they might be analyzed and a part deposited as standards in each of the countries. The sets of samples supplied to each country were to be identical in composition, but each set would contain specimens varying in composition. The results of the analyses were to be compared, the object being to promote greater uniformity of procedure and a selection of the best methods. The Crescent Steel Works of Pittsburg have supplied the samples, and the English experts have almost completed their work. Should the Commission succeed in bringing about uniformity of practice in this respect, it will do much towards lightening the work of those who have to compare the results arrived at in different countries. Sir Frederick next referred to Dr. Sorby's method of examination of iron and steel by microscopic examination of carefully prepared samples, in which the structure has been developed by treatment with a weak acid. It will be remembered that Dr. Sorby gave a description of his process in a paper read before the Iron and Steel Institute two or three years ago; and since then Dr. Herman Wedding has followed the matter up with success. Many years previously Faraday had pursued an analogous course of investigation. It is satisfactory to learn that "the systematic application of Sorby's system of microscopic examination of prepared surfaces of steel and iron is continually extending at the German works, and that many series of experiments have demonstrated that by this system of examination characteristic features of grades of iron may be discovered, physical differences co-existing with identity of chemical composition explained, and evidences of the true grounds of disasters obtained." A very interesting subject next occupied a place in the address. This was the self-destruction, if one may use the term, of steel projectiles by the development of cracks. It is well known that steel projectiles may be received from the manufacturer to all appearance perfectly sound, and after a time cracks will develop themselves. In extreme cases the occurrence has been so sudden that a violent rupture, attended by a sharp report, has taken place. The cause doubtless is the surface treatment to which the shot is subjected in order to get the requisite hardness, and which leads to internal strains being set up. In one case mentioned in the address the head of the projectile had been thrown to a distance of many feet by the violent spontaneous rupture of the metal. The importance of rest in bringing about a diminution, if not entire disappearance, of internal strains in masses of metal is illustrated by the behaviour of chrome steel projectiles, which had to be stored for several months before their transport to a distance could be ventured upon. In connection with this subject Sir Frederick referred to a previous report in which he dwelt upon the effect of time in establishing chemical equilibrium in masses of metal. He also quoted a letter written to him by Thomas Graham, when Master of the Mint, in which was discussed the tendency to the development of cracks in tempered steel dies, and stating that in the Mint it was generally considered that if such dies were kept in store for a year or two, they became less apt to crack when in use, and coined more pieces than dies newly tempered. The same phenomena have to be considered in the manufacture of steel ordnance; and an instance was given by the lecturer of the tube of a large gun which had fired three proof rounds. A circumferential crack was found to have become developed in the front threads of the breech screw, and, upon removing the jacket from the tube the crack extended forward along the chamber and into the rifling. When the tube was placed in the lathe, with a view to cutting off the injured portion, the crack suddenly developed itself with a loud report, and ran along to within eight feet of the muzzle; a spiral crack at the same time ran completely round the tube, which fell in two upon removal from the lathe. This instance will strengthen the hands of those who are opposed to oil-hardening the parts of a steel gun; and Sir Frederick's own words in connection with this vexed question are worth quoting. "One effect which the oil-hardening treatment has occasionally exercised in the case of particular qualities of steel is that of developing minute fissures or cracks in the metal, either superficially or in the interior of the mass. This cannot, of course, be rectified by any annealing process, and it is still a question, to be determined by the teachings of experience and the result of investigations, whether any definite or reliable modifications in the composition of steel used for guns, tending to secure the desired combination of hardness and tenacity may not be introduced, with the result that a method of treatment of the metal may be discarded which, however carefully applied, and however efficient the means adopted for reducing or neutralizing its possible prejudicial influence upon the physical stability of the parts of which a gun is built up, carries with it inherent elements of uncertainty and possible danger." Dr. Anderson's remarks on the subject of oil-hardening should also be read in connection with the observations contained in the President's report. On the whole, perhaps, it would not be rash to predict that the days of this process are numbered in connection with the manufacture of steel ordnance for Her Majesty's service. For a long time many of our best authorities have been opposed to it.

We have not space to follow the address into the subject of the effect of silicon in cast-iron. General interest in this matter was aroused a year or two ago by a paper read before the Iron and Steel Institute by Thomas Turner, of Mason's College; and since then the investigation has been followed up by German experimentalists, with a general result that, under certain conditions, it is concluded that silicon will contribute to the production of dense and homogeneous castings.

The following passage from the address speaks for itself. It would be well if it could be printed and distributed to every British iron or steel maker:—

"The absolute dependence of the development of new metallurgic processes upon the results of the labours of the analyst, the chemical investigator, the physicist, and the microscopist, and the thoroughness with which this all-important fact is appreciated by the German metallurgic establishments, afford new occasion for a regretful recognition of the distance which we are still behind our Continental brethren in availing ourselves of the advantages afforded by the constant pursuit of scientific research, and the thoroughly efficient, systematic, and direct application of the labours of the scientific investigator to the daily operations at works of all kinds, although it must be acknowledged that of late years we have made important progress in these directions. It has certainly been humiliating to have to admit that industries which the genius of individual

Englishmen, possessed of exceptional powers of applying to important practical purposes the results of research, have created and have developed to an extent foreshadowing their high importance, gradually passed out of our hands through the far-sightedness of the Germans, who have very long since recognized the absolute dependence of progress in such industries upon the constant pursuit of chemical research into the far-reaching and continually spreading ramifications of organic chemistry. Thus, in fields of work, where, in days past, and even of late, our industrial chemists have been content to pursue their attempts at progress with the co-operation of one or two young chemical assistants, small armies of highly-trained chemists, who have gained academic honours, and have won their spurs in original investigation, are in constant employment at the magnificent manufacturing establishments in Germany, systematically pursuing researches which constitute successive indispensable links in a great network of exhaustive inquiry, and which, while conferring large benefits on the science itself, are continuously productive of improvements in existing processes, or of the development of new methods, while, ever and anon, they result in some fresh discovery of great technical importance and high commercial value. Similarly elaborate and comprehensive arrangements now exist at important German iron and steel works for systematic investigation and comparison of materials of products and processes."

We must hurry over the remaining parts of Sir Frederick's address, and can only mention some of the chief subjects touched upon, referring our readers to the Proceedings of the Institute for fuller information. Thus we find the following matters occupying attention: the presence and effect of nitrogen in iron; the state in which carbon exists in steel; Osmond's study, by means of the Le Chatelier pyrometer, of the slow cooling of iron and steel, together with the phenomena of recalescence, and the existence of two allotropic forms of iron; the effect of aluminium in iron; Hadfield's researches in connection with manganese steel; the progress of nickel steel; and the interesting discovery of Langer, Quincke, and Ludwig Mond of the action of carbonic acid upon finely divided nickel at high temperature, in which it was found that the metal had the power of separating carbon from the gas, with production of carbonic acid in place of the oxide. These and other matters were dealt with at greater or less length, and constituted a most interesting and characteristic address.

Only one paper was read on the first day of the meeting. This was Dr. Anderson's contribution on tests for steel used in the manufacture of artillery. The announcement of a contribution on this subject by the Director-General of Ordnance Factories had caused a good deal of interest both among the scientific and manufacturing members of the Institute, more especially as it was known that the Government authorities had been overhauling the official test regulations. Unfortunately, however, the meeting was a little too early, so far as Dr. Anderson's paper was concerned, for the new regulations have not yet been officially published, and, until they are, it is against official etiquette, if not official rules, that they should be made known. The paper was therefore very like the play of "Hamlet" with the Prince of Denmark left out, and bore evidence of having been brought forward rather with a view of fulfilling a promise than because the author had anything new to advance. It was not Dr. Anderson's fault that his paper was robbed of its chief interest, and certainly the thanks of the Council were due to him for good-naturedly allowing it to stand on the programme. Notwithstanding what we have said, the paper was very interesting, but as we hope to hear Dr. Anderson again on the subject, when the official veto has been removed by publication of the new tests, we shall treat the matter briefly. It is first pointed out that the mechanical properties of steel, and of alloys generally, are affected in a remarkable manner by extremely minute quantities of substances, by the relative proportions, by the changes in some or all, produced by the more or less rapid changes of temperature, which influence dissociation and reveal their effects by recalescence; indicating, to a less degree, allotropic changes in some or all of the components. Chemical analysis sufficiently minute to detect even traces of every substance associated with iron would be tedious and costly. Years must pass away before chemical and physical science together will succeed in determining the laws which govern the mechanical properties of alloys. For these reasons, and others, the specifications of gun-steel used in Her Majesty's service exclude all definitions of chemical composition, so far, at any rate, as ordinary ingredients are concerned. The author thinks it is not sufficiently realized that metals are incapable of appreciable cubical compression under any stress that can in practice be brought to bear on them, whether fluid, pasty, or cold. Like ice and water, steel and cast-iron have a greater volume in a solid than in the liquid state, and, therefore, red-hot solid cast-iron or steel floats on the surface of the molten mass: although, it should be added, cold cast-iron will at first sink in a bath of liquid iron, but will rise to the surface and float when it has acquired a sufficient degree of heat to bring it about to a cherry red. This was shown by the well-known experiments of Mr. Wrightson, referred to at the meeting. The manner in which, during cooling, compressive stress is suddenly turned into tension high enough to cause rupture (due to the swelling during solidification) is dealt with; this being a subject also treated upon in the President's address. The bearing of these phenomena upon the process of hardening is also discussed. The relative influence of carbon in iron as a definite compound of carbon and iron dissolved in an excess of iron, and as a finely subdivided carbon diffused through the mass, is considered, and the author expresses an opinion that the "apparently capricious behaviour of steel" is due not only to the internal stresses engendered by oil-hardening, but also to the circumstance that the chemical condition of the steel and its molecular structure are greatly influenced by comparatively slight errors of judgment, or by carelessness in the adjustment of the temperatures at which the operations are performed.

A discussion followed the reading of the paper, in which the most interesting incident was Mr. Wrightson's description of his experiments to determine the volume of cast-iron at different temperatures. Mr. Edmunds, of Woolwich, defended the practice of oil-hardening for gun-steel; and Mr. Hadfield would attribute cracks in steel rather to contraction than expansion.

On the second day of the meeting the proceedings were opened by Prof. Roberts-Austen giving a verbal description of the Le Chatelier pyrometer, an instrument which is now well known to the scientific world. It may be of interest to state that Sir Lowthian Bell and other practical men spoke of the great assistance this pyrometer had been to them in the course of manufacturing operations.

M. Osmond's paper on the micro-structure of steel was no more than a note which accompanied the presentation of a series of micro-photographs. The paper of Dr. E. J. Ball, which followed, was supplemental to a previous paper contributed by him (see Journal Iron and Steel Inst., 1890, No. 1, p. 85); and, as the present paper will be supplemented by another, we will refer our readers to the Proceedings, merely giving the general conclusions arrived at by the author, which are as follows:—"(1) That in iron containing 0·1 per cent. of carbon, the tenacity of the metal increases by hardening, either in oil or in water, with the temperature at which the metal is quenched with a view to hardening, a maximum tensile strength being reached at a temperature of about 1300° C. This temperature once exceeded, however, the tenacity of the metal diminishes, although the extensibility increases. (2) By raising the percentage of carbon from 0·1 to 0·2, the maximum tenacity is attained, not at 1300° C., but at a much lower temperature—about 1000°—below the melting-point of iron oxide, which, moreover, was not present. (3) By further considerably increasing the percentage of carbon, this point of maximum tenacity apparently disappears almost entirely, the annealed metal having nearly as high a tensile strength as the same metal which has been quenched in oil from any temperature up to a bright red heat. Beyond this temperature, or when quenched in water, the hardened metal became so hard and brittle that it could not be gripped by the jaws of the testing machine." It will be remembered by those who attended the meeting when Dr. Ball's last paper was read that M. Osmond put forward the idea that the fourth point in change (in addition to Osmond's three points), which occurs, according to Dr. Ball, in very mild steel at a temperature approaching the melting-point, might be due to the fusion of iron oxide. The present paper is founded on this remark, but for the results, beyond the salient features given, we must, as we have said, refer our readers to the originals. Mr. Turner's paper, which was read next, does not require a detailed notice at our hands. It was an economic paper on a subject which is rapidly losing economic interest; and the author does not appear to have made himself well acquainted with the labours of previous investigators in this field. The paper of Mr. Jenkins does not admit of an abstract being made; whilst the last paper read, that of Lieutenant Jaques, U.S.N., was of such a voluminous nature that it might better be described as a treatise, and is far beyond our scope, as may be judged from its title.

The autumn meeting of the Institute will probably be held at Birmingham.

THE ROYAL SOCIETY SOIRÉE.

THE *soirées* given by the Royal Society become every year more pleasant. The one held on Wednesday, May 6, was in every sense most successful. We note some of the objects exhibited:—

Mr. J. Wimshurst exhibited an electrical influence machine (alternating and experimental).

The Trotter curve ranger was shown by Mr. A. P. Trotter. This portable instrument is intended to facilitate setting out large curves for railway and other work. It dispenses with tables of angles and with the use of chains and assistants. No cumulative error can arise as with theodolite work.

Profs, Rücker and Thorpe, FF.R.S., exhibited a map showing the probable connection of lines towards which the magnet is attracted in England and France. Profs. Rücker and Thorpe found that the north pole of a magnet is attracted to a line which runs south from Reading, and enters the Channel near Chichester. M. Moureaux has traced a similar line from Fécamp to the south of Paris, but its southern termination has not yet been discovered. The directions of the two lines make it probable that they are parts of the same axis of disturbance.

The Director-General of the Geological Survey exhibited:—(1) Specimens illustrating the phosphatic chalks in England, France, and Belgium, arranged by Mr. A. Strahan, Geological Survey of England and Wales. Phosphatic band in the upper chalk of Taplow, containing about 30 per cent. of phosphate of lime. Taplow phosphatic chalk separated by washing into: (1) brown sand composed of phosphatised organisms, and containing about 50 per cent. of phosphate of lime; (2) chalky mud composed largely of rhabdoliths, coccoliths, and discoliths. Microscopic preparations of the phosphatised organisms of the Taplow chalk, showing Foraminifera, prisms of Inoceramus shell, fish-scales, fish-bones, and fish-pellets. Photographs of the Taplow phosphatised organisms, by Mr. J. J. H. Teall, F.R.S. Phosphatic chalk of Beauval (Somme), and microscopic preparation. Phosphatic chalk of Ciply (Belgium), and microscopic preparation.—(2) Illustrations of a former Arctic climate in the Lowlands of Scotland, determined by Mr. Clement Reid, Geological Survey. At Hailes, about three miles south-west of Edinburgh, in a thin seam of silt, resting immediately on boulder clay, Mr. J. Bennil, of the Geological Survey of Scotland, has lately found numerous remains of plants. These show a climate probably 15° or 20° colder than that of the Lowlands at the present day. In the following list the peculiarly Arctic species are marked with an asterisk. The only *tree* is an alder. The willows are all dwarf species; two of them (*Salix herbacea* and *S. reticulata*) still live on the higher mountains of Scotland, the third (*S. polaris*) is an Arctic form now extinct in Britain. At the same locality there is another deposit, probably of later date, which contains only plants still living in the neighbourhood, including several trees.

Thalictrum
Ranunculus aquatilis
Viola
Stellaria media
Oxalis acetosella
Hippuris vulgaris
**Loiseleuria procumbens*
Menyanthes trifoliata
Stachys palustris
Ajuga reptans
Chrysanthemum Leucanthemum
Polygonum aviculare
**Salix herbacea*
**Salix polaris*
**Salix reticulata*
Alnus
Empetrum nigrum
Potamogeton
Eleocharis palustris
Scirpus pauciflorus
Scirpus lacustris
Scirpus?
Carex?
Isoetes lacustris

The Executive Committee of the Silchester Excavation Fund exhibited (by permission of the Duke of Wellington):—(1) Iron tools and utensils of the Roman period, found together in a pit in the Romano-British city at Silchester, Hants, in September 1890.—(2) Bronze objects of the Roman period found at Silchester.

Prof. H. Carrington Bolton, Ph.D. (of New York), exhibited musical sand, from Arabia, United States of America, and the Hawaiian Islands, collected by the exhibitor.

Prof. H. G. Seeley, F.R.S., exhibited remains of Anomodont Reptiles from the Trias, Karoo, Cape Colony.

The Director of the Royal Gardens, Kew, exhibited a collection of views in the Royal Gardens, Kew, showing the development of the Gardens during the last fifty years. This series is a portion of a very extensive and valuable collection of prints, drawings, and photographs of the most interesting features in the Royal Gardens. It has been brought together during the past twenty years, and is now deposited for exhibition in Museum No. 3.

Messrs. J. E. H. Gordon and Co. exhibited Tomlinson regulator for electric light mains. The Tomlinson regulator is intended for use in transformer sub-stations. It is worked by a wire from the central station, but automatically corrects any error of the attendant at the central station. Ordinary automatic apparatus cannot be safely used for this purpose, as, though should such get out of order when taking out transformers, no harm is done except the waste of coal, yet if it gets out of order when putting in transformers it may burn up the sub-station. The peculiarity of the new apparatus is that if anything whatever goes wrong, all transformers are at once put in, thus ensuring absolute safety. By the courtesy of the Brush Company, and of the Metropolitan Electric Supply Company, who have lent the necessary machinery, the apparatus has been tried on a large scale in the Brush Company's works. A plant of 1950 lights capacity has been run for 24 hours with, and for 24 hours without, the new apparatus, with the result of a saving of 4¼ cwt. of coal, or, in other words, with the new apparatus there was a saving of 89 pounds of coal per 8 c.p. lamp per annum, or about 26 per cent. of the total coal bill.

Sir J. B. Lawes, Bart., F.R.S., and Dr. J. H. Gilbert, F.R.S., showed:—(1) Three enlarged photographs of Leguminous plants, grown in 1889, in experiments on the question of the fixation of free nitrogen. The plants were grown, in some cases with sterilization, and in others with microbe-seeding of the soil. With suitable microbe-infection of the soil, there was abundant formation of the so-called *leguminous nodules* on the roots of the plants, and there was, coincidently, very considerable fixation of free nitrogen. The evidence at command points to the conclusion that the free nitrogen is fixed in the course of the development of the organisms within the nodules, and that the resulting nitrogenous compounds are absorbed and utilized by the higher plant.—(2) Coloured drawing, by Lady Lawes, of the Rothamsted rain-gauges.—(3) Coloured drawing, by Lady Lawes, of the Rothamsted drain-gauges.

Old plan of the Mint in the Tower of London, exhibited by the Hon. Sir C. W. Fremantle, K.C.B. This document is described as an exact survey of "The Ground Plot or Plan of His Majesty's Office of Mint in the Tower of London." It bears the date 170^{0}_{1}, and must have been prepared by the order of Sir Isaac Newton, who was appointed Master of the Mint in 1699. The position of Newton's official residence is shown at A.

Mr. R. E. Crompton, M.Inst.C.E., exhibited:—(1) Section of armature winding, showing copper divided, twisted, and compressed, to avoid loss from eddy currents.—(2) Crompton's method of obtaining accurately sub-multiples of the ohm; for current-measuring purposes.

Prof. Oliver Lodge, D.Sc., F.R.S., exhibited:—(1) Revolving mirror. Rapid revolving mirror driven by clock-work, with detachable fan to give moderate speeds, with adjustable main spring to vary the speed, and with vacuum cover for highest speeds (the last not yet satisfactory). Slow moving index, to enable the speed to be determined; and electro-magnetic brake to regulate its going, or to stop it gradually. Mirror, 2·3 × 1 cm., silvered back and front; very light, but giving fair definition. It makes 5760 revolutions for 1 of the winding arbor. Used for analyzing sparks, and observing the speed of electric pulses along conductors of various kinds. Made by Mr. W. Groves.—(2) Clock for pointing out continually the direction of the earth's orbital motion. (Two home-made forms.) A disk, or dial, set on a polar axis with the obliquity of the ecliptic, is driven by a clock against the rotation of the earth. On the dial are recorded 365 days of the year. It is set once for all in the plane of the ecliptic, with the actual date pointing 90° from the sun. In the first instrument I devised, the direction of the right-dated radius of the dial henceforth points out the direction of the earth's motion at any instant, if the clock keeps sidereal time. A modified and improved instrument, devised by my assistant, Mr. Edward E. Robinson, adds a sighted pointer to the dial, this pointer being moved by hand to the right date; and the clock may then keep ordinary time. The dial is geared down 1 : 24, and driven by the minute hand, so as to be under the ordinary control of clock-regulation. In each instrument a one-day hand-shift is needed every 29th February. —(3) Resonant Leyden jars. A couple of independent but similar Leyden jar circuits arranged at a moderate distance from each other, the self-induction or capacity of one of them being adjustable, with an easy overflow path. On discharging one of the jars, the other resounds and overflows, being provided with an easy overflow path. The oscillations are much more numerous than with ordinary linear (Hertz) vibrators, and therefore some precision is demanded in the tuning.

Self-recording instruments, exhibited by MM. Richard Frères.

Method of recording pyrometric measurements at temperatures between 600° C. and 1200° C., exhibited by Prof. Roberts-Austen, C.B., F.R.S. The apparatus is that employed in a research undertaken for the Institution of Mechanical Engineers, and is used for automatically recording, by the aid of photography, the indications of a platinum and platinum-rhodium thermo-couple. The experiments shown illustrate a method of recording the rate of cooling of heated masses of metal. Curves are shown to illustrate the kind of results which are obtained by the aid of the apparatus.

Length-measuring instrument, exhibited by Prof. W. C. Unwin, F.R.S. In ordinary screw or vernier micrometers the straining of the instrument alters the readings, and in using the instrument much depends on personal skill. In this instrument the contact is with fixed pressure, and independent of feeling. Delicate levels show when the instrument is adjusted.

Portraits of deceased astronomers and physicists, exhibited by Mr. W. B. Croft.

Mr. Killingworth Hedges exhibited:—(1) Electrical safety-valve.—(2) Exhausted bulbs, used to ascertain the space traversed by high tension alternating currents. The electrical safety-valve is designed for attachment to low pressure service lines, in order to prevent their being charged at a dangerous difference of potential from the earth. The glass bulbs were exhausted to different pressures, and fitted with electrodes of various forms, in order to ascertain if an arc could be started with an E.M.F. of 300 volts, which is the limit of potential fixed by the Board of Trade for currents of low pressure.

Focometer, exhibited by Prof. Silvanus P. Thompson. By this instrument can be determined the position of the two principal "focal planes" and of the two "principal planes" of Gauss, for any compound system of lenses, such as a microscopic objective or the lens of a photographic camera; thus giving the true focal length, and the positions and distance apart of the two virtual optical centres of the lens-system. The principle applied is that of finding directly the two principal foci, and then, by means of a right-and-left-handed screw, moving two micrometers placed at these foci to the two symmetric points where each micrometer coincides with the image of the other. The displacement so given by the screw is equal to the true focal length.

Mr. Shelford Bidwell, F.R.S., exhibited: (1) Selenium cells, the electrical conductivity of which is greater in the light than in the dark. (2) A selenium lamp-lighter, lighting an incandescent lamp automatically when darkness comes on. (3) A selenium alarm, for calling attention to the accidental extinction of a ship's light or railway signal lamp.—Mr. W. Crookes, F.R.S., exhibited electricity and high vacua.—Mr. G. J. Symons, F.R.S., exhibited photographs of damage produced by the tornado of August 18, 1890, at Dreux (Eure et Loire), France. —Prof. C. Piazzi Smyth exhibited examples of photographic enlargements of the solar spectrum, each magnified from the original negative from 25 to 27 times linear.—Mr. George Higgs exhibited photographs of the normal solar spectrum.

M. G. Lippmann exhibited colour photographs of the spectrum:—(1) Small spectrum, exposure about 3 minutes.—(2) Large spectrum, exposure about 6 minutes, without coloured screens. The colours seen on these plates are produced by the direct action of light; they are not due to any pigments, the substance of the films remaining colourless, but are of the same kind as the colours of soap-bubbles and mother-of-pearl, viz. interference phenomena; they are due to the structure imparted to the film by the stationary waves of incident light during exposure in the camera. These colours are perfectly permanent.

Prof. A. Schuster, F.R.S., exhibited some forms of Clark cells.

Prof. Emerson Reynolds, F.R.S., exhibited: (1) Specimens of tetrathiocarbamid-ammonium bromide, $(H_5N_2CS)_4NBr$, and related substances. (2) Series of photographs illustrating the application by Colonel Waterhouse of the above bromide to the reversal of the photographic image on gelatino-bromide of silver films.—Mr. W. Saville-Kent exhibited photographs of living corals, taken in Torres Straits.—Dr. W. Hunter exhibited a series of ptomaines—alkaloidal products formed by bacteria from animal tissues.—The Committee of the Camera Club exhibited allotropic forms of silver, prepared by Mr. Carey Lea, of Philadelphia, and described in *Amer. Journ. of Science* for 1889, and *Phil. Mag* for 1891.—Prof. G. F. Fitzgerald, F.R.S., exhibited crystals of platinum and palladium (prepared Mr. J Joly).—Prof. J. A. Ewing, F.R.S., exhibited Prof. Sekiya's model of a Japanese earthquake.—The Council of the Royal Society exhibited a cabinet containing medals struck in honour of Fellows of the Royal Society.—Mr. Edward Schunck, F.R.S., exhibited indigo-blue and allied substances and derivatives of chlorophyll.—Mr. Fred Enock exhibited microscopic preparations of the British Mymaridæ (egg parasites).—Dr. H. Woodward, F.R.S., exhibited skull and shoulder-girdle of *Procolophon trigoniceps* (Owen), collected by Dr. Exon in the Orange Free State (figured Phil. Trans., 1889, p. 267).—Mr. J. Howard Mummery exhibited specimens illustrating some points in the structure and development of dentine.—Mr. Allan Dick exhibited a new form of polarizing microscope.

Meteorological photographs, exhibited by Mr. Arthur W. Clayden. The photographs of clouds have been taken by reflection from a mirror of black glass, placed in front of the camera, so that the plane of its surface makes the polarizing angle with the axis of the lens. Those of hoar-frost show how the crystals attach themselves to the projecting portions of objects, such as the margins of leaves, the loose fibres of a string, and the thorns of a briar, and also their tendency to grow towards the direction from which the air has been moving.

THE BENUÉ AND THE KIBBÉ.

AT Monday's meeting of the Royal Geographical Society, Major Claude M. Macdonald, H.M. Commissioner to West Africa, gave an account of a journey up the Benué and its northern tributary the Kibbé, in the summer of 1889. The Benué, we need scarcely say, is the great tributary of the Niger. Major Macdonald referred to the previous explorations of Barth and others, and to the fact that it has been maintained that a connection existed between Lake Chad and the Benué, by the overflow of the Shari on one side and the Kibbé on the other. Major Macdonald has been the first to explore the Kibbé. After describing the ascent of the Benué, Major Macdonald went on to say that he and his party started on their journey up the Kibbé in the Royal Niger Company's stern-wheeler the *Benué*, on August 21.

The Kibbé at its mouth is some 250 yards wide, while the Benué is upwards of 600. The average depth of the Kibbé at this season of the year, nearly high water, is from 10 to 12 feet. On both banks for the first five miles the country is flat and well wooded, with patches of bright green grass, and looks very gamey, though owing to the high grass we saw no deer. A noticeable feature some five or six miles from the river is Mount Katie, a rounded hill, some 800 feet high, well wooded to its summit. This hill, from its isolated position, served as an excellent point on which to take angles for mapping purposes. Patches of cultivation were now to be seen on both banks, and after two hours' steaming the party passed the Fulbe village of Dinghi. The inhabitants, though they had never before seen a steamer or a white man, did not seem much disconcerted, and, when shouted to in their language, returned the salutations in a very friendly manner. On August 22 the *Benué* anchored off a large village on the left bank. "We very soon saw," Major Macdonald states, "that we had to deal with the purest-bred Fulbe we had seen so far. The crowd consisted almost entirely of women—by far the best-looking we had as yet seen on the Niger, and indeed the best-looking I have seen in either east or west Equatorial Africa. They wore the usual piece of cloth wound round their bodies, leaving their arms and shoulders bare, and reaching down below the knee. Their features, in most cases, approached the European, and their expression most gentle and modest, yet full of vivacity. They told us that the name of their village was Pamu, and that it was governed by an Emir, who was under the jurisdiction of the Emir of Yola. The men were armed with spear and bow and arrows, though they are said to be an agricultural people, and certainly it would seem so, for every yard of ground in the neighbourhood of Pamu was under cultivation. We asked them if they would bring us provisions in exchange for cloth; this they readily did, and we soon were hard at it, bartering pieces of cloth, salt, &c., for live stock, weapons, ornaments, and indeed anything. The whole time nothing but the greatest good temper prevailed, and I was much struck by their gentleness and courtesy; albeit the ladies were very good at a bargain, and I noticed that when it came to bartering their ornaments, members of the fair sex, who were not so young or so fair as their more fortunate sisters in this respect, surreptitiously handed their ornaments to the latter to dispose of, hoping thereby to get better value, and I am bound to confess they did."

Shortly after this the steamer came to a deserted strip of country, some fifteen miles in length, which was evidently the barrier between the Mahommedan and Pagan tribes; it was of an undulating character, with isolated hills, and well wooded. The river was still about 100 yards wide, but commenced to be dotted with grassy islands, and was in parts very shallow with a sandy bottom.

Next day, as the steamer advanced, the river narrowed again and made a sharp bend to the eastward, and approached a grassy range of mountains, leaving a higher range to the north. Half an hour after starting the party arrived at the foot of the grassy slopes of the former; a pathway, which could be traced for a considerable distance, wound up the face of the mountain and disappeared over one of its grassy ridges. Patches of cultivation could be seen dotted here and there; the main valley stretched back some three or four miles, but we could see no signs of a village.

"We were, however," Major Macdonald stated, "not left long in doubt as to whether the country was inhabited or not, nor as to the character of the inhabitants, for down the winding path, which was distant some 600 yards from where we were, came a line of warriors, some 200 in number; the majority of them were quite naked, though some few had a small cloth round their waists. They were all armed, mostly with spears, the almost invariable number being three. Leaving the pathway, they advanced in excellent order across the boulder-covered grassy piece of ground which lay between the river and the mountain side. We accordingly moved into mid-stream, which was only some 15 yards from the bank, and dropped anchor in about 4 feet of water. Our friends advanced straight at us, not a word being spoken, but an excellent line being maintained, when suddenly they all took cover behind boulders and tufts of grass, nothing being visible but the gleaming points of their spears. It was a source of some gratification to us that the points were gleaming, for it showed that at any rate they were not poisoned. There was now a pause. Then our Fulbe interpreter, under my directions, opened fire in a dialect of the Battawa, with satisfactory results, for they appeared to understand him. Their first question was as to whether 'we were Mahommedans? because if so we could not pass, as they were the outposts of the Pagan tribes, and had orders not to allow Mahommedans to pass.' We assured them that we were not Mahommedans. They then told us, in answer to our queries, that the name of their village was Katsho, and that it lay back from the river amongst the hills; they said that if we went on we would come to more villages. After a great deal of persuasion two of their number consented to come on board. So we sent a six-oared gig, which we had towed up with us in case of accidents, to fetch them. They were fine, well-made men, but were trembling with fright at the sight of the steamer and white men, and prostrated themselves on the deck at our feet. These two men wore loin cloths of native manufacture; the great majority of the others were, as I have said, naked. After getting as much information out of these men as we could, which information, on account of their terror and the difficulty in interpreting, was somewhat meagre, we proceeded on our way. By this time large numbers of men and boys had assembled, and ran along the banks gesticulating and pointing at our little ship. They, men and boys alike, were all armed, mostly with spears; we saw very few bows and arrows.

"The scenery now was very picturesque; to our right, *i.e.* the south of the river, some few yards from the water's edge, the mountains rose in some places quite abruptly. These mountains were for the most part covered with green wavy grass very pleasant to the eye. One or two streams trickled down the mountain side, forming now and again picturesque waterfalls. The river had suddenly broadened out to a lake, or, more properly speaking, marsh, some three miles long by two wide. The range of grassy mountains I have mentioned ran along the southern shores of the lake and terminated with it. The country on the east and north shores of the lake, as far as the eye could see in the direction of the Tuburi marsh (near the Shari river) was open and gently undulating, while from the western shores of the lake the beautiful range of mountains, with their needle-shaped peaks, stretched back apparently for many miles. In the north-east corner of the lake we saw a very large village some two miles distant; this we afterwards ascertained was Bifaré. The channel of the river evidently followed the base of the southern hills. We accordingly steamed gaily along, followed on the shore by an ever-increasing crowd, till we arrived at a large village prettily situated almost on the edge of the lake. The houses or huts were built in clusters, each cluster apparently belonging to a different family. The huts were very well constructed, having round walls some 6 feet high, with flat roofs formed by beams covered over with mud and thatch. The walls of the huts were made of black and in some places red mud, and the workmanship of both walls and roof was excellent. Several hamlets were prettily situated on the slopes of the hill, surrounded with patches of cultivation, and had the appearance of the country places of the richer inhabitants of the village.

"A large crowd had now assembled, and regarded our movements with great curiosity. We asked to see the chief of the village, and after a good deal of palaver, a man appeared attired in a very tattered 'tobe' or gown. He had something of the Fulbe in his countenance, and was a tall fine man, though of rather a forbidding appearance. He came on board, and we endeavoured to get what information we could out of him. He said the name of the big water we saw was Nabaret, but that it was only a fourth that size in the dry season. The name of his village was Kaku. The channel of the river ran along by the mountains. He knew of the Tuburi marsh, but had never been there; he did not think the river came from there as it was distant many days' journey. He knew of no other big water, but would give us a guide to show us the way. The people of the Nabaret district are possessed of cattle, but no horses; they live principally on dhurra, which they cultivate largely, and on fish which abound in the lake. They also hunt the hippopotami, of which we saw a dozen in the lake, though doubtless there may be many more.

"We took our guide on board and endeavoured to make for Bifaré, already mentioned, which appeared to be a village of quite 6000 inhabitants, situate on the north-east shores of the lake, and distant some two miles from where we were. After proceeding about 100 yards we found that the water shoaled about a foot, and even less, and though we made every effort to proceed, we were completely baffled; turning back, by direction of the guide, we went for an opening in the high dhurra, which grew in immense quantities about here, and found ourselves once more in the channel of the stream, which was, however, only some 8 yards wide and 2½ feet deep, flowing with a swift current. After proceeding with great difficulty for almost a mile, with fields of dhurra growing to a height of 8 feet on either side and completely shutting out the view, the navigation became so difficult that we had to turn back, having already smashed in the bow of our gig, bent our rudder into the shape of a bow, and more than once berthed our little ship amongst the dhurra stalks. The stream was so narrow that we could not turn, but had to float down backwards for a good half mile. The highest point reached was a mile and a half from the village of Kaku, and from what the people said, a good thirty miles from Dawa, in the Tuburi country, the furthest point reached by any European entering Africa from the north, viz. Dr. Vogel in 1854. The stream at the point where we reluctantly turned back was not more than 2 feet deep, and from 15 to 20 feet wide, and this at the period of high water. I should say that in the dry season (and this is corroborated by the natives themselves) that a man could step across it. It is more than probable, therefore, that had we been able to proceed another three miles or so, we should have arrived at its source."

It seems evident, then, from Major Macdonald's observations, that no connection can exist between the Shari and the Benué.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. J. W. Clark, Superintendent of the Museum of Zoology and Comparative Anatomy, has been elected to the Office of Registrary of the University, vacant by the death of Dr. Luard.

The degree of M.A. *honoris causâ* has been conferred on Mr. J. Y. Buchanan, F.R.S., University Lecturer in Geography.

The Electors to the new Isaac Newton Studentships, founded by Mr. F. McClean, are Sir G. G. Stokes, Profs. Darwin and Thomson, Dr. Glaisher, and Mr. Glazebrook.

SOCIETIES AND ACADEMIES.

LONDON.

Zoological Society, May 5.—Prof. Flower, C.B., F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of April 1891, and called special attention to the arrival of what appeared to be an adult male example of the Lesser Orang (*Simia morio*) of Owen, presented by Commander Ernest Rason, R.N., who had obtained it at Sarawak, and to a Great-billed Tern (*Phaethusa magnirostris*), obtained by purchase, new to the collection.—Mr. Sclater opened a discussion on the fauna of British Central Africa, by pointing out the limits of this new territory, which was computed to embrace some 54,000 square miles of land lying immediately north of the Zambezi and west of Lake Nyassa. Mr. Sclater gave an account of the principal authorities that have already written on the subject. Mr. Sclater was followed by Mr. G. A. Boulenger, who read a paper "On the State of our Knowledge of the Reptiles and Batrachians of British Central Africa." The discussion was continued by Mr. Edgar A. Smith, who read a note on the Molluscan fauna of British Central Africa; and by Mr. E. T. Newton, who communicated some general remarks on what is known of the geology of British Central Africa, stating several points to which special attention should be directed. Remarks on various branches of the same subject were made by Dr. Günther, Mr. O. Thomas, Mr. Stebbing, Mr. Slavin, and Mr. Beddard.—Mr. T. D. A. Cockerell read notes on some Slugs of the Ethiopian Region, based on specimens in the collection of the British Museum.—Dr. C. J. Forsyth-Major read a paper containing a summary of our knowledge of the extinct Mammals of the family Giraffidæ.—A communication was read from the Hon. L. W. Rothschild, F.Z.S., containing the description of a new Pigeon of the genus *Carpophaga*, from Chatham Island, South Pacific, proposed to be called *Carpophaga chathamensis*.—Colonel Beddome read descriptions of some new Land-Shells from the Indian Region.

PARIS.

Academy of Sciences, May 4.—M. Duchartre in the chair.—On the time of evaporation of water in boilers, by M. Haton de la Goupillière. The author has mathematically determined the rate of lowering of the level of the water in steam boilers of various forms.—A geometrical theorem, by M. Tarry.—On a class of ordinary linear differential equations, by M. Jules Cels.—On the convergence of recurring simple fractions, by M. H. Padé.—On an induction inclination needle, by M. H. Wild. A short note is given on some measures of magnetic inclination made with a new form of needle. It appears from the experiments that the inclination at a place can be determined within 4″·5 by a single observation. Skilled observers make the determination within 2″·5. Inclination may therefore now be determined as accurately as declination.—On a process for constructing screws suitable for the instruments to be used for the measurement of the photographic map of the heavens, by M. P. Gautier.—Quantitative studies of the chemical action of light; Part ii., reactions with different thicknesses of glass and with different forms of vessels, by M. Georges Lemoine.—On some compounds formed by mercuric chloride, by M. G. André. The methods of preparation of the subjoined compounds are given, together with analyses establishing their composition:—

(1) $4ZnCl_2, HgCl_2, 10NH_3 + 2H_2O$;
(2) $2ZnCl_2, HgCl_2, 6NH_3 + \frac{1}{2}H_2O$;
(3) $C_6H_5NHHgCl$;
(4) $C_6H_5NH_2, HgCl_2$;
(5) $5C_6H_5NHHgCl + 2HgCl_2$;
(6) $3C_6H_5NHHgCl + 2HgCl_2$;
(7) $C_6H_5.CH_2.NH_2, HgCl_2$;
(8) $C_6H_5.CH_2.NH.HgCl$.

—A general law determining, as a simple function of the chemical constitution of bodies, the temperatures of their changes of state under all pressures, by M. G. Hinrichs.—On boron selenide, by M. Paul Sabatier. The compound is prepared by the action of vapours of selenium upon amorphous boron at a red heat, or of seleniuretted hydrogen on amorphous boron at a bright red heat in a tube of Bohemian glass. The action of water upon the selenide shows it to have the same composition as the sulphide and oxide. Its formula is therefore B_2Se_3, a conclusion supported by the results of a rapid analysis.—On the action of hydriodic acid on boron bromide, by M. A. Besson. At a raised temperature the three compounds BBr_2I, $BBrI_2$, and BI_3, have been obtained.—On the basic chromites of magnesium and zinc, and the neutral chromite of cadmium, by M. G. Viard.—Preparation of disodic erythrate, by M. de Forcrand.—Discussion of the experiments of Biot on aqueous solutions of tartaric acid in presence of potash or soda, by M. G. Aignan.—Formation of dimethacrylic acid in the preparation of the acid amides of isovaleric acid, by M. E. Duvillier.—Methyl-methylcyanosuccinate, methylethenyltricarboxylic ether, by M. L. Barthe.—On the "dextrosity" of certain Gastropods called "sinisters" (Lanistes, Peraclis, Limacina, larvæ of Cymbuliidæ), by M. Paul Pelseneer.—On the structure of the composite eye of certain Crustaceæ, by M. H. Vaillanes.—Comparative structure of the inflated roots of certain umbelliferous plants, by M. Géneau de Lamarlière. It is shown that the anomaly which is observed in the inflated lateral roots of certain umbelliferous plants (Œnanthe, Carum) is more apparent than real. In plants of the same family an intermediate series should be found between the structure called normal and the structure of a normal inflated root (Daucus, Apium).—On the microscopic structure of the phosphate rocks of Dekma (Department of Constantine), by M. Bleicher. The rocks examined are said to show under the microscope the mixture of a fair proportion of osseous *débris*, whence it is thought that this is the origin of the phosphorus in rocks rich in calcium phosphate.—Note on the Quaternary strata of Éragny and Cergy (Seine-et-Oise), by M. E. Rivière.—On the production of diabetes after the destruction of the pancreas, by M. E. Hédon.—Meteorological observations on the Pamir, by M. Guillaume Capus. An account is given of thermometric observations made between March 13 and April 19, 1887, on the high plain of Pamir, the centre of the highlands of Europasia.

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THURSDAY, MAY 21, 1891.

PYCNOGONIDS.

Den Norske Nordhavs-Expedition, 1876–78. *XX. Zoologi—Pycnogonidea.* Ved G. O. Sars. Med 15 Plancher og 1 Kart. (Christiania: Grondahl & Sons, Bogtrykkeri, 1891.)

Studies from the Biological Laboratory, Johns Hopkins University, Baltimore: A Contribution to the Embryology and Phylogeny of the Pycnogonids. By T. H. Morgan. With Eight Plates. (Baltimore: The Johns Hopkins Press, 1891.)

THE group of sea spiders, or the Pycnogonidea, was for a long time among the least known, though by no means the least interesting, of the divisions of the marine invertebrates. Linnæus described a species as a Phalangium, placing it among terrestrial forms, and though a century and a quarter has passed since then, the problem of where to place these Pycnogonids cannot be said to be finally settled.

Within the last ten years or so, an immense advance has been made in our knowledge of the morphology, anatomy, and embryology of the group, thanks to the labours of Anton Dohrn, who, in 1881, described the forms found in the Gulf of Naples, and of Hoek, who about the same date described the species found during the cruises of the *Willem Barents* and the *Challenger*. During all this period opinions varied as to whether these forms should be placed among the Arachnids or the Crustacea, but apparently both the authors just referred to have agreed that the Pycnogonids should be placed with neither, but that they, with the Arachnids and the Crustacea, have come down the stream of evolution in parallel lines.

To the existing recent memoirs of these Arthropods, the splendid volume just published on the Pycnogonidea found during the Norwegian North Atlantic Expedition, 1876–78, by Prof. G. O. Sars, adds, perhaps, from a morphological point of view, the most important of the recent publications on the group; for, valuable as beyond question are the structural and developmental details, a special knowledge of general morphological detail is also needed for the convenient understanding and classifying of any group.

The material at Prof. Sars's disposal was very large, and in addition he has made use of collections made by himself during many years back on the coasts of Norway, and also of some few forms sent to him by Dr. A. Stuxberg, which had been found in the Kara Sea during Nordenskiöld's expedition. A very great contrast is to be seen on comparing these northern forms with such a collection as that of Dohrn from the Mediterranean. The great number of species belonging to the family Nymphonidæ is specially characteristic of the Northern Seas as contrasted with the Mediterranean, while again the Northern Sea species attain very generally much larger dimensions, some being gigantic in comparison with those of the Mediterranean.

In working out the classification of the group, Sars has found it necessary to treat the families in a somewhat more restricted sense than has been done by most of the previous writers, and has been obliged to increase their number. While fully agreeing that the descriptions and even figures of the Pycnogonids given by the earlier writers leave much to be desired, and are as a rule even exceedingly defective, in some cases indeed being so bad as not to be intelligible, yet he thinks that some quite recent describers have rejected as bad a greater number of descriptions than with a little patient research was really necessary. Thus he finds it hard to believe that, while not a few species have been described from the Gulf of Naples, all the species described as found there by Dohrn, with one exception, should be new. Most certainly as regards the northern species we cannot sufficiently admire the pains which Sars has taken in working out all the imperfect descriptions and rough figures of our past recorders of new forms, with the result that he has succeeded in re-establishing many wholly forgotten or ignored species of Goodsir and others.

As regards the terminology used in describing the various parts, some, classing the Pycnogonids with the Crustacea, adopted terms in use among the latter; while others, holding their affinity to be with the Arachnids, employed again a different set of terms. Dohrn, to avoid the difficulty as regards the limbs, rejects all special terms, describing them as No. I., II., &c.; Sars uses a terminology the terms of which involve as little as possible of any homologous references.

Forty-three species are described and figured. Several of them are here fully described for the first time, though short diagnoses of them appeared in a preliminary report. The fourteen genera are arranged in eight families, and these are grouped into three orders, the ordinal characters being based on the relations of the "chelifors." Thus in Order 1, Achelata, these chelæ are, except in the larval state, entirely absent; in Order 2, Euchelata, the chelæ are well developed throughout all the stages of life; while in Order 3, Cryptochela, the chelæ are present, as a rule, in the young stages (not alone in the larvæ), but in the fully developed condition they become atrophied or disappear. This arrangement no doubt will have to be modified so as to fit it to receive the very numerous forms from other parts of the world, but it is a first step in the right direction of an intelligent grouping of the genera.

The second memoir on our list treats of the Pycnogonids from a different standpoint, being a contribution to our knowledge of the embryology and phylogeny of the group, by T. H. Morgan, Fellow of Johns Hopkins University. After a short allusion to the work of Dohrn and Hoek, who have "placed the morphology of the order on a very firm basis," he proceeds to treat of the early stages of the embryology of the Pycnogonids, stages which have been practically unexamined, and a knowledge of which is needed to enable the relationship of the group to be guessed at.

The material for this work was collected at Wood's Holl. Three genera, each with a single species, are to be found at this place—*Pallene empusa*, *Phoxichilidium maxillare*, and *Tanystylum orbiculare;* and during July, August, and September, these were found carrying ova. The alcoholic picro-sulphuric acid process was adopted for hardening; the eggs being cut in paraffin. The eggs of Pallene were large, 0·25 mm., and well adapted for investigation. After a minute description of the early stages of development, the author considers that from them there is little or no ground for a comparison between the Pycnogonids and the Crustacea, certainly not with any existing forms. The multipolar delamination of the endoderm in the Pycnogonids has no homologue amongst the Crustacea, nor is there any special similarity in the formations of the organs. There seems to be no trace of gastrulation like that in the Crustacean in the ontogeny of the group. And if there be reason for rejecting a relationship between the Pantopod larva and the Nauplius, and with Dohrn he believes that there is, then there remains nothing in common to the ontogeny of the two groups.

Nor are there any special affinities between the insects and Pycnogonids; but between these latter and Peripatus a striking similarity is met with in the paired ventral organs, both in the structure and position of these, but for the present there is no proof forthcoming as to a real homology of these bodies. The process of the formation of the endoderm, as described by Heider and by Wheeler in insects, shows a certain resemblance to multipolar delamination; but if it be such, it is a more complicated form than is shown by the Pycnogonids. With these two exceptions there would seem to be nothing else in common in the ontogeny of the two groups.

Lastly, as to a decision as to the relationship with the Arachnids, or as to their being an independent phylum. While Dohrn and Hoek ably maintain the latter, though not agreeing as to the why in all details, yet the study of the early stages of the embryology has brought to light certain facts which lead the author of this memoir to believe in a community of descent between the two. The reasons for this belief are given in full detail, with difficulty admitting of abbreviation. The Pycnogonids form the endoderm by a process of multipolar delamination, which is shown in its simplest form in Phoxichilidium and Tanystylum, and in a more modified condition in Pallene. In no other group of the Triploblastica is a similar phenomenon found except in the Arachnids. In the spiders the process is not so well marked, but if Balfour's conception of the formation of the yolk nuclei be correct, then a direct comparison may be made between the two groups. The first trace of the embryo to appear in Pallene is a round opaque area at the spot where the stomodæum invaginates. In Schimkewitsch's recent account of the development of the spiders, he shows that the primitive cumulus in them is the place where the stomodæum invaginates; and in calling attention to the fact that the stomodæum of spiders in its earliest development is a triangular invagination, he actually compares it with the triangular invagination of the œsophagus of the Pycnogonids. It is also exceedingly probable that the early formation of the body cavity surrounded by mesoblast in the legs of spiders has an exact parallel in Pallene and Phoxichilidium. In both Arachnids and Pycnogonids there are well-marked diverticula from the mid gut into the legs. In both Arachnids and Pycnogonids the first pair of appendages are chelate, and in both this first pair is innervated from the brain; these facts alone, it will be remembered, were considered by Balfour to indicate a relationship between the groups. Mr. Morgan was unable to find any post-oral ganglia for Pallene, but the first pair of appendages arises on the sides of the stomodæum and moves forward later. In this respect, it compares closely with the spiders, and the early innervation of this pair from the brain itself may be regarded as a more abbreviated condition than what was seen (by Balfour) in the spiders. Metchnikoff's figures for Chelifer show the first pair of appendages to arise above and on each side of the proboscis-like upper lip, and if future investigation verifies Metchnikoff's suggestion that this proboscis is homologous, entirely or in part, to the proboscis of the Pycnogonids, as his figure seems to indicate, then does the whole development of the Chelifer show remarkably close resemblances to that of the Pycnogonids. The fourth pair of ambulatory legs—the seventh pair of appendages—has been a stumbling-block in the way of an Arachnid relationship, and the attempts to solve the difficulty have been many. Here, again, Balfour's suggestion that this last segment and its appendages may represent the first abdominal segment of the Arachnids is of value, as we know that the embryos of spiders have rudimentary appendages on the abdomen. In a second part of this memoir the metamorphosis of Tanystylum is described, and in a third part we have a very complete study of the structure and development of the eyes of Pycnogonids and a comparison with the Arachnid simple eyes, a comparison that seems to verify the relationship pointed out in the first part of the memoir. E. P. W.

A TEXT-BOOK OF CHEMISTRY BASED ON THE PERIODIC SYSTEM.

A System of Inorganic Chemistry. By William Ramsay, Ph.D., F.R.S. Pp. 700. (London: J. and A. Churchill, 1891.)

During the twenty-five years or so which have elapsed since the recognition of the periodic law of the chemical elements as a valid relationship, the pronounced influence which it has exercised both on the aspect and aims of chemical science cannot be questioned. Whether in the prediction of undiscovered elements, or as an indicator of needful research, especially in the department of atomic weight estimations, it has met with signal success. In connecting the physical properties of the elements themselves and of their compounds with atomic weight, it has opened up new fields of investigation, and thrown fresh interest into old ones. Properties so widely different as those measured by refraction equivalent and breaking stress find an explanation, nowadays, in the magnitudes of the atomic weights.

As a means of classification, too, the success of the periodic arrangement has not been less striking. Indeed, to its power as an instrument of classification it owes its general acceptance in the first instance. When the ideas of Avogadro had become recognized, and by their means the old system of "equivalents" had been replaced by the true atomic weights, then the periodic arrangement resulted in a grouping of the elements so much in harmony with existing notions of their relationships, that the far-reaching power of the generalization could no longer be resisted.

The distinguishing feature of the book before us consists in the use of the periodic arrangement as a means of classifying the subject-matter of inorganic chemistry. Here, the time-honoured methods of putting the facts and theories of chemistry before the student are set aside, and as the method adopted is novel to English text-books, it may be advisable to consider its characteristics. After a short historical introduction, the author proceeds to describe the occurrence, preparation, and properties of the elements in the order in which they are found in the periodic table. First, Group I., hydrogen and the alkali metals; then Group II., metals of the alkaline earths, and so forth. The descriptions refer, as far as possible, to the elements of the same group taken collectively.

The compounds of elements of the different groups with the halogens form the next part, and in the introductory portion the student meets for the first time with matter which it is customary to discuss at an earlier stage in the text-books; such matter as the distinction between element and compound, the use of chemical symbols, the gaseous laws, &c. The fourth part deals with the oxides, sulphides, selenides, and tellurides, and under these headings are to be found hydroxides, hydrosulphides, &c., classed as compounds of the oxides with water, hydrogen sulphide, &c. Here, also, are treated the salts of the oxyacids, classed as double oxides, and compounds as $POCl_3$ treated as double compounds with the halogens.

Part v. gives an account of the borides, carbides, and silicides; such of the hydrocarbons as are considered, and the organo-metallic compounds occur in this part. Compounds with the elements of the nitrogen group, including the cyanides, form Part vi. Alloys and amalgams are discussed in Part vii. The first chapter of the next part gives a short account of spectrum analysis and the rare earths. The second chapter is chiefly concerned with the criteria for fixing atomic and molecular weights, the Raoult methods finding a place, and the last chapter is devoted to the periodic law. The closing part of the book takes up, mainly with regard to the chemical principles involved, the manufacturing processes usually treated in the text-books.

It will be seen, as the author states in his preface, that the method adopted does away with the distinction between metals and non-metals; no special stress is laid on the properties of acids as contrasted with bases; equal prominence is given to rare and more common substances; and the commercial importance of a substance or process is not considered an argument for its special consideration.

Such a work as this may be looked at from two points of view. Regarded as a systematic arrangement of the facts of inorganic chemistry, from which any desired information may be speedily taken after one has become familiarized with the method of classification adopted, its success is undoubted. The book is quite in touch with recent investigations, nothing of importance seems to be omitted from the descriptive portion, and, what is a recommendation to a large class of readers, the size of the book is not excessive. Whatever be the results of the system adopted, economy of space is assuredly achieved. To the teacher or to the advanced student who wishes to use the book as a work of reference, or desires to systematize his knowledge, it will be eminently useful.

If, on the other hand, the system be regarded from the point of view of a basis for teaching, its construction from its very novelty must be open to discussion. A method of teaching chemistry often employed may be said to consist in giving the learner in as easy a manner as possible the leading facts of chemical science with regard, in the first instance, more to the correct appreciation of the meaning of the facts themselves, than of the exact arrangement or classification of the same. To this end the student is led from the study of the chemical properties of commonly occurring bodies to the description of the elements contained in them, explanations of chemical terms being given as they crop up, or in short reviews at intervals not far apart. When the properties of the elements are being explained, their reactions with other elements have to be noticed, and hence it appears natural to describe the important compounds of an element after its own properties have been discussed. The periodic system does not seem to provide the means for such a course of teaching, and this appears to us to be the main reason for its non-adoption in the text-books.

Indeed, the new method has little in common with that indicated above. The entire series of the elements apart from their compounds are described, and chemical and physical terms are freely used without any attempt being made to define them till all the elements have been treated. In fact, a few terms, as critical point and heat of formation, are used, but as far as we can see, not defined in the book. Again, compounds containing a common constituent are classed together, but compounds of what may be taken as a parent element are scattered throughout the various groups. Surely, in connection with this point, reasons similar to those which lead to the grouping of compounds containing the same element, on the new system, would hold for the old method of considering compounds. The position of the iron group of elements after the aluminium group and of the copper group—the last one described—may be taken as an indication that even in the author's opinion the periodic law does not in all cases indicate most clearly the relationships of the elements. Such considerations as these must weigh with a teacher before he can adopt the system; during four years' experience, however, the author has had no reason to doubt its success.

The book is clearly printed, and the illustrations, though not very numerous, are for the most part new. The frequent use of vapour jackets in the apparatus represented is suggestive of the author's more recent contributions to scientific literature. The useful system adopted by Ostwald in his "Lehrbuch," of indicating the state of aggregation of a substance by the type, has been employed.

Setting aside the points which may be urged against the work as a basis for teaching, the periodic law, as expounded by Prof. Ramsay, does more than any other system of classification to put the matter of inorganic chemistry on a footing resembling that which holds for organic chemistry.

OUR BOOK SHELF.

Eighteen Years of University Extension. By R. D. Roberts, M.A., D.Sc. (Lond.). (Cambridge: University Press, 1891.)

THE University Extension movement takes so prominent a place among the educational influences of the age that a good account of the system has for some time been needed. This is supplied by Mr. Roberts, who, first as lecturer, then since 1881 as assistant and organizing secretary to the Cambridge Syndicate, and since 1886 as secretary to the London Society, has had the best possible opportunities of studying the new method, and of forming a judgment as to its fitness for the uses to which it is applied. He begins with an account of the origin and growth of the movement, then describes the character of the audiences, the reception of the idea by artisans, and the signs of earnestness displayed by various classes of students. Mr. Roberts also discusses the conditions of success, has a chapter on the consolidation of the work, and presents a summary of results. No essential fact has been omitted, and the general impression which will be left on the minds of most readers probably is that those connected with the movement have done much to foster and to satisfy the desire of a very large number of persons for intellectual training. There are certain rules—some of them rather difficult—with which the system must be brought into accord if it is to be capable of further development; and these are stated with much force and precision in the present useful little volume.

Evening Work for Amateur Photographers. By T. C. Hepworth, F.C.S. (London: Hazell, Watson, and Viney, Ltd., 1890.)

IN this book the author has written, in an interesting manner, a series of chapters relating to many points in photography that are generally found most useful to amateurs. The following are the subjects of some of the chapters: lantern entertainments, lantern-slides on gelatine plates, clouds in lantern pictures, frame-making, enlarging, photography by magnesium light. There are also two or three chapters on electric light, light by incandescence, and methods of making cheap batteries.

The subjects are treated in a manner that makes the book well worth reading, and its value is increased by numerous illustrations obtained from photographs and drawings by the author.

LETTERS TO THE EDITOR.

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The University of London Question.

THE Convocation of the University of London has, by a large majority, thrown out the scheme for the reconstitution of the University proposed by the Senate. Even those who had little love for it must feel some sympathy at the frustration of labours which were as patient as they were undoubtedly disinterested.

For the moment the whole question remains in abeyance. I am disposed to think that it may be useful to discuss, in the interval which must elapse before any further step is taken, some of the fundamental questions which seem to me to underlie the solution of the problem, and have never, as it seems to me, been properly considered.

On such a subject one might easily write a great deal. For the sake of brevity I shall therefore attempt to sum up what I have to say under separate heads.

The Examination System.

One factor in the present situation is undoubtedly the growing dissatisfaction of many distinguished teachers with the examination system as applied to University education. And as the University of London at present does nothing but examine, it is obvious that the question lies at the root of any judgment that may be pronounced on its present work and constitution. Those who wish to know all that can be said against the present use of examinations in University work cannot do better than study a paper by Prof. Lankester, which he has reprinted in his "Advancement of Science" (pp. 175–192). He has stated his case with all the force and lucidity of which he is a happy master. He sees "the most injurious result of the system" in "the degradation of the teacher." The "intrusive board of examiners" draws "away from him the attention and the respect of his pupils," or urges "him to put aside his own thought and experience, and to teach the conventional and commonplace."

I am free to admit that there is a certain element of truth in what Prof. Lankester says. But having had, like him, a good deal of experience both of examining and of being examined, I am disposed to think the picture somewhat over-coloured. No doubt the University of London in the past has exalted examination into a sort of idol. But as regards the superior degrees in science and medicine, at any rate, examination is now practically dispensed with, and the test of competence is the performance of some kind of original investigation.

For the inferior degrees, as far as I am aware, the examination system in more or less prescribed subjects obtains everywhere in the three kingdoms. For my part, I think the system may be defended, and upon the same lines as those on which Prof. Lankester defends "leaving examinations" at schools. For he says, and I think rightly, that such an examination "may be regarded as a means of criticizing and testing the performance not merely of the schoolboys but of the schoolmasters." Now in University education, as carried on in this country, I can only see a prolongation of school education, with methods and a moral discipline modified to suit the more advanced age of the pupils. And the inferior degree (I am not speaking, of course, of professional subjects) is, in my view, nothing more than the corresponding "leaving examination." It is a test of whether teaching has been faithfully done and learning diligently pursued.

I am quite ready to admit that impending examinations are more or less irksome both to teachers and taught; but I am not convinced that that discipline is in itself an evil. It is not undesirable that some restraint should be put on the possible vagaries of the one and the very probable desultoriness of the other. It is necessary in entering upon the study of a subject to go over its fundamental groundwork in a methodical manner. To many teachers and to many pupils this is not a little dull. It is easy and it is pleasant to dwell at some length on attractive parts of a subject and to skim superficially over others. There are probably few persons who, looking back upon their own student days, will not admit the truth of this. The fact is that to get any mastery of a subject one must learn its grammar; and the majority of young people require some degree of compulsion to make them do it. It may be irksome at the time, but the advantage is life-long. I know, speaking from my own experience, that the compulsion of schedules which is so odious to Prof. Lankester has made me devote my energies to the mastery of the rudiments at any rate of many subjects which I should certainly have carefully avoided if I had not been compelled to do otherwise. And I do not believe that, if students are carefully and soundly taught, they suffer any real injustice at the hands of competent examiners. But then I agree with Prof. Lankester that the examiners must know their business, and must not be either ill-informed or pedantic. No one, I think, can urge that the kind of men that the University of London enlists in its service as examiners are open to the charge of being either.

If these views are correct, and I believe in the main they are, then the evil consists not in the examination system as the incentive to the orderly performance of a curriculum, but in another and perfectly distinct evil on which Prof. Lankester very sagaciously puts his finger—the mischievous importance which the outside world attaches to academic achievement. "A man refers throughout his life to the fact that he obtained a 'first-class' as a sort of perpetual testimonial." Of course, in so far as this is true it is very absurd. A course of University study is a means, not an end; it is a sort of apprenticeship to a subject. The student learns its technique, its language, and something of its literature. If he has done this earnestly and well, his University will applaud him, will call him in academic language "a good boy." But when the congratulation of his friends has subsided, the real question arises, what will he do with the tools he has learnt to use? Here, I think, University work enters upon a new phase, and one, it seems to me, too little regarded—I mean post-graduate study. To control this in any measure by means of examination seems to me in the highest degree absurd. And I must contend that by making original investigation, at any rate for its doctorate of science, the qualification for that degree, the University of London has taken a step in advance of many of the older Universities towards destroying the idea that the passing of examinations is the final end of University study.

A Teaching University.

I have always found it not a little difficult to understand what those people exactly mean who so strenuously demand a teaching University for London. What Prof. Lankester means, there can, as is usually the case, be no sort of doubt about; and this I shall discuss presently. But, as far as I can make out, all other persons seem to think that London University students labour under some special disadvantage which undergraduates at Oxford and Cambridge do not experience. Perhaps, then, it may surprise many to be told that there is no essential difference in the two cases. Examining in the two older Universities is in the hands of the University, and is just as much distinct from the teaching in their case as in that of London. I can speak with some positiveness upon this point, for having been for four years an examiner for the Natural Science Tripos at Cambridge, a University with which I have no connection, I found the functions I was called upon to perform exactly the same as those I have also fulfilled at Burlington Gardens. In fact, I can see no essential difference between the position of an undergraduate of New College, Oxford, examined for his degree by the local University and an undergraduate of University College, London, examined by the University of the capital. If Oxford and Cambridge are teaching Universities in any intelligible sense of the phrase, then I contend that the University of London is equally so.

Prof. Lankester adopts the view of Fichte, who says "that a University is not a place where instruction is given, but an institution for the training of experts in the art of making knowledge, and that this end is attained by the association of the pupil with his professor in the inquiries which the latter initiates and pursues." Most excellent, and I can imagine nothing more delightful than for some wealthy man to give, say, half a million of money to found such a University in some quiet country town in England, where professor and pupils might labour together, undisturbed by the life and movement of a big city, or the worry of the examination-room, for the advancement of knowledge But if such "a seat of learning," in the true sense of the words, could be brought into existence, it would probably be found in practice that the students would be men who had already graduated, *i.e.* in my view acquired that knowledge of the elements of a subject which is essential to the proper performance of any work in it. A Professor of Biology, for example, would not care to have to teach a pupil at the commencement of a research how to interpret what he saw through the microscope, or how to cut a section. And if we firmly grasp the idea of the non-finality of the graduation course, we get an intelligible distribution of labour amongst the staffs of the older Universities: the college lecturers will prepare men for their degrees; the professors will guide their maturer studies afterwards.

While I cannot help thinking that those who advocate the creation of a so-called teaching University in London, have got hold of an idea which they have only imperfectly assimilated, it is still worth while to examine some of the ways in which it might be realized.

With an adequate endowment a new so-called teaching University might no doubt be established in London. It would have a staff of professors who, we may assume, would be adequately paid. The posts would in that case be no doubt filled with men of distinction and eminence. Would they be able to spend their time, full of enthusiam but free from care, in leading students in the paths of research after Prof. Lankester's ideal? Not a bit of it. Such an institution would not be very different from a Scotch University, where one of the most distinguished scholars of his age is said to have found his time largely taken up with teaching schoolboys of larger growth the mysteries of Greek irregular verbs. In proportion as the new institution became a success the drudgery would increase and the advantage diminish. The bigger a professor's class the less personal contact he can have with his pupils, till at last he has to rely for any influence at all on the stimulus of lecture-room oratory. As Mrs. Garrett-Anderson has, it seems to me, correctly pointed out in the *Times*, there is very little really to be said in favour of anything like a great central teaching institution for such a city as London.

The other alternative is to combine University and King's Colleges into a teaching University. But can this be regarded as in any way a statesman-like proposal? Why should two out of many institutions be picked out for University honours? And can anyone really suppose that such a settlement would have any finality about it? Why, for example, should Bedford College be left out, developing, as it apparently is, in usefulness and activity every day? Then how can the Royal College of Science at South Kensington be ignored? It is already in popular esteem ranked as a University, and bids fair to become in time in actual fact the great science University of the country. Why, too, ignore the City and Guilds Institute? It is difficult, then, to believe that a teaching University founded on University and King's Colleges can be regarded as in any way a final solution of the problem. If it is sought in this direction it must be based on a wider federation of institutions of academic rank. But in this case all the teachers will have something to say as to the conditions of common examination. Yet, according to Prof. Lankester, the essence of a true teaching University idea is the "absence of examiners—the professor himself is examiner and teacher in one." Schedules will nevertheless reproduce themselves, and the influence of colleagues will be quite as much an obstacle to the independence of the individual professor as the oppression of boards of examiners.

Furthermore, it is quite a mistake to suppose that unless the existing University is abolished, it will be possible for a younger one to escape its influence. Notwithstanding the establishment of the Victoria University, it is still found necessary, and at the request of Owens College, to hold the examinations of the University of London in Manchester. Consequently, the professors of Owens College have to adapt their teaching to a double curriculum. If the proposed University of Westminster were founded, it cannot be doubted that the same thing would happen. The professors would still have to bow their necks to the yoke of the Burlington Gardens schedules.

Expansion of Existing University.

It may be taken as quite certain that the existing University of London is too well rooted in the esteem of the community to be got rid of. Nor, with its own consent, will it readily submit to be mutilated or dismembered. And its pride and confidence in itself admits of easy justification. With all its demerits it can hardly be denied that it has accomplished a great work in raising the standard, throughout the country, of academic education. This need not be wondered at, seeing that it has always succeeded in enlisting in its service the most accomplished and distinguished men in every branch of education. If examination is to be conducted at all, I can hardly imagine conditions more favourable to its conduct than the University of London affords.

Instead of trying to diminish and curtail the usefulness of an institution which has such strong claims on public gratitude, I prefer to make the suggestion—and it is odd that it should have any novelty about it—that the future needs of University education in London should be provided for by an expansion of the existing University. This has always been the ambition of Convocation, and many, I know, share my own opinion that, if the Senate would have given greater heed to the representations which the former body has from time to time made to it, the present crisis in the history of the University would never have arisen.

I will briefly indicate the by no means drastic changes by which this might be gradually provided for.

Organization of the Faculties.

I am myself personally impressed with the conviction that the first step that should be taken in the interests of the higher education in London, and of those parts of the country which look to London for academic guidance, is the organization of the faculties. Everyone is agreed, whatever view they take on the examination question, that the teaching bodies should be brought into as intimate a relation as possible with the central University. At present there is no recognized channel of communication between them, and it has been long felt that this is a great evil. Examination is an art, and it is a progressive art. To minimize its possible harmfulness it should keep touch with the teaching. And it must be admitted that the system which now obtains at the University of London does not make this always easy. The Senate is hard to move and slow to act. This would not be so if those who had the right to move it possessed the momentum which would be derived from a more obvious authority. In fact this tendency to inaction arises from a natural timidity. The Senate is too largely composed of persons who have no direct touch with actual education.

The momentum to which I have referred above would come with all needful force from the faculties if they were organized in a comprehensive way to include every competent authority in academic education in London. I will not stop to discuss the precise machinery by which this should be brought about. It seems to me that it would be probably sufficient if the Senate were to have power to admit to the faculties the teachers of all institutions of academic rank which supplied it with candidates. To these should be added the past and present examiners, a certain number of non-graduates conspicuous for their distinction in the subjects with which the faculty was occupied, and a proper proportion of members of Convocation.

Such a body would occupy itself with any and every subject relating to academic education. Its resolutions would embody the deliberate conviction of instructed and competent persons, and would afford the Senate a solid basis for administrative procedure. I need hardly say that the faculties—if they took, as I doubt not they would do, a just view of their functions—would look to the advance of academic interests as a whole; they would not seek the sole advantage of the central University, but would watch and work for the interests of the collegiate institutions they represented—whether in London or the provinces—as well.

Boards of Studies.

Delegations from the faculties should be intrusted with the duty of watching the examination work and advising the Senate thereupon. This they would do in two ways: (1) they would consider from time to time all alterations necessary in the schedules so as to keep the examinations as closely as possible in touch with the best teaching; (2) they would review the conduct of the examinations, though without in any way interfering with the examiners. It would be their duty to consider the papers set, and criticize them if necessary, and they would consider and report on any apparent variation in the standard as evidenced by any sudden change in the percentage of passes and rejections.

Reform of the Senate.

I think it is generally admitted that the time has come when some change in the constitution of the Senate is advisable. At present it is an assembly of notables appointed for life. Many of them never attend, and some, appointed apparently on purely political grounds—and these are not always the least competent—never perhaps have attended. On the whole, the Senate, though individually eminent, is, it must be confessed, ill-informed on educational matters. As I have already hinted, it is apt in consequence to be somewhat timid and irresolute when it ought to act with decision; it is equally apt, I am afraid, to act with precipitancy when it ultimately realizes the necessity of moving at all.

The Senate must, however, remain the supreme governing body with whom the final decision must always remain in matters of importance. This being so, it seems not too much to ask that it should be an efficiently constituted body, and that the members should attend to their duties. Tenure of office for life it would seem desirable to abolish, and prolonged absence from attendance, say for a year, should *ipso facto* vacate a seat. As for the Crown nominees, who are in great part statesmen of high rank, it would be on obvious grounds unwise to dispense with them, if they took, as many of them do, sufficient interest in the work to attend with some regularity. Where the Senate needs strengthening is in experts in academic education; and it appears to me that the faculties, if constituted as above, might be intrusted with the duty of selecting these members of the Senate from their own ranks. On the whole, it might be convenient to constitute the Senate something on the lines of the Hebdomadal Council at Oxford: a third to be appointed by the Crown, a third to be appointed by the faculties, and a third by Convocation.

Higher Teaching.

There is still, however, one direction in which the University of London might even more closely associate itself with actual teaching, and so far become in actual fact a teaching University. This was pointed out in 1872 by the late Registrar, Dr. Carpenter, in his evidence before the Royal Commission on Scientific Instruction. He said (Minutes of Evidence, 10,925), "I think it very important that the State should provide for the carrying on of those higher researches, and that higher teaching, which are not provided for in any shape at present." Again (10,926), "I think that a body like the University of London might very advantageously be empowered to take up such higher and more special teaching. At present the University of London has nothing to do with teaching. The principle of the University is to recognize existing institutions. I do not think that it would be at all the function of the University to interfere or compete in any way with the institutions which it recognizes. But I should myself be very glad to see the University empowered to carry out courses of instruction of a higher and more special kind than are given in any of the institutions affiliated to it." The scope of this higher teaching was brought out more clearly in a subsequent part of Dr. Carpenter's evidence in answer to a question of Prof. Henry Smith's (10,953). He asked, "The Senate might at some future time endeavour, might they not, to have such lectures given in connection with the University of London as are now given in the Collège de France?—Yes, more of that character."

Such lectures would serve for the post-graduate study, provision for which seems to me the great defect in University education as it exists in London. And the professorships themselves would be positions which could be filled by eminent scientific men whom it is difficult as things are to retain in the capital. To take biological subjects as an example, the continual draining away of men like Michael Foster, Burdon Sanderson, and Lankester seems to me a real loss to the intellectual life of London.

It is just possible that it may be objected that the proposal to have a superior professoriate attached to the University is in some degree a slight on the Colleges and their teachers. And it may be urged that, if there were any demand for post-graduate teaching, the Colleges are quite competent to provide it. It may be so; but in practice I do not believe it feasible. The working day is inelastic, and from what I myself know of the labour involved in what may be called systematic graduation courses, I do not believe that the same man can superadd the higher work as well. Besides, to be of any value, it must not be formal and perfunctory; the essence of the higher teaching is that it should reflect the research to which the occupant of each chair should be able to devote the whole of his time.

I do not think that such professorships will be founded as long as the University is under the control of the State. For this and other reasons I should gladly see the University cease to be a quasi-Government institution, and launch out on its own resources. It seems almost incredible, but it is a fact, that at the present time not the slightest alteration can be made in a schedule without the approval of the Home Office, or the slightest alteration in the amount of prizes without that of the Treasury. There is no inducement now to the public to provide endowments, because, as the University nearly pays its way, any public benefaction would only tend to create a surplus, which would have to be paid over to the Exchequer. But I can hardly doubt that if the University were cut adrift from the State it would receive endowments which would enable it from time to time to found useful and important chairs. These would form not an unwelcome addition to the too few prizes accessible to those who devote themselves to learning for its own sake.

I had it in my mind to say a few words about the very complicated but independent problem which medical University education in London presents. But this paper has already run to an intolerable length, and the subject is perhaps of limited interest to the readers of NATURE. But I may say that I believe that the organization of a strong medical faculty would bring about the solution of all existing difficulties.

W. T. THISELTON DYER.

Royal Gardens, Kew, May 18.

A NOTE in the last issue of NATURE (p. 39) seems to assume that the present University of London is nothing but an Imperial Examining Board that has got wrongly named, and stands in the way of London possessing the educational advantages of a German University town.

I venture to offer some facts and considerations which may modify this view, and perhaps aid in forming a juster conception of the real nature of the University question than is commonly entertained.

Much more important matters are involved in the question than the maintenance or extension of existing institutions, though these are quite legitimate subjects of discussion and defence; and in the columns of NATURE it is only upon the broad ground of the advancement of science and learning that the question can be dealt with.

The epithet "Imperial" is intended to imply some unfitness on the part of the present University for other than "Imperial" functions, whatever these may be. But the University has not, and never has had, the least claim to any such title. It has never at any time held colonial examinations of its own motion. It has never at any time held any colonial examinations whatever in the faculty of science, or in the faculty of medicine, or for honours in any faculty, or for any of the higher degrees. What examinations it holds in any colony are held only at the request of the Governor of the colony, transmitted through the Colonial Office, and are practically confined to matriculation and the intermediate examination in arts. Occasionally, but very rarely, an examination in laws or for the Bachelor of Arts is held in some colony. In 1890, 16 candidates matriculated in the colonies, and 5 passed the intermediate examination in arts out of a total of some 5000 candidates. Not a single degree examination was held in any colony. In fact, these colonial examinations, which, few as they are, yearly diminish in number, never formed part of the University scheme. They were instituted about 1864 at the request of the colony of Mauritius, but were extended and have been maintained chiefly to facilitate the award of the scholarships at the disposal of the Gilchrist Trustees. Not only is the University of London not an Imperial University, but it is even less British in character than probably either of the older Universities. Very few of its candidates come from Scotland, fewer still from Ireland, and my strong impression is that the great majority come from midland and southern England. I should not be surprised even to find that a considerable majority are now drawn from an area having London for its centre with a radius of not more than 100 miles. The probable establishment, at no very remote period, of provincial Universities will practically give a still more exclusive sense to the name University of London.

It may next be asked what precisely is meant by a "teaching University in and for London," the creation of which is constantly put forward as the principal educational need of the metropolis. Is the proposed University to be "for" London in some sense in which the University of Oxford is not "for" Oxford, or that of Edinburgh not "for" Edinburgh? I know of no University, British or German, which is "for" the particular town or district in which it has its local habitation. Or is the proposed University to be "for" London in some sense in which the existing University is not "for" London as well as the rest of the country? The words seem mere surplusage, unless intended to impose local limitations which no University has ever yet imposed upon itself.

The expression "teaching University," too, stands in need of exacter definition. The University of Edinburgh is a teaching University, so is that of Dublin, so are the German Universities. Oxford and Cambridge are only in part teaching Universities; the greater part of the teaching is done by the Colleges. The Victoria University is not, in fact, a teaching University at all; the teaching is the work of its Colleges, and the proposed "teaching University in and for London" would, as far as actual teaching is concerned, resemble the Victoria University rather than a Scotch or German University. At this point the *crux* of the whole question reveals itself. The really distinguishing feature of the new University as contrasted with the University of London would be the examination of collegiate candidates (and those only) by their teachers in alleged conformity with the principle that examination should follow teaching. But it may be admitted that teaching ought to be adapted to examination, or examination to teaching, without admitting any advantage in the system of teachers settling the examination of their own students, collegiate or not. The combined teacher-examiner system is not wholly trusted by its supporters. At the older Universities the examiners are by no means usually the teachers of the candidates; at the Victoria University one of the examiners is always an "external" one. I am not quite sure how the matter stands at the Scotch and Irish Universities. To assert that such partial or semi-partial modes of testing knowledge are superior to disinterested and independent methods is merely to make an assumption, announce an opinion. What comparison of the working of both systems proves any superiority on the part of the first-mentioned of them? Do the pass degrees of Scotch or Irish Universities, or even of Oxford or Cambridge, stand higher than those of London?

Further, is it not misleading to characterize the University of London as a mere Examining Board? Of the three functions of such a teaching University as that of Edinburgh, it performs two. It directs teaching by syllabuses and regulations (prepared with extreme care, and not without ample reference to the best authorities on all matters of special knowledge), and it tests teaching by absolutely impartial and disinterested examinations, but it does not—without space, funds, and appliances it could not—pretend to teach. Nothing, however, in its nature or essence forbids its development, alone or in union or conjunction with other institutions, into what would be an ideal University of the non-residential order, neither coercive nor exclusive—one that should offer proper University instruction to all comers, and, at the same time, confer degrees upon open examinations independently (save for obvious reasons in relation to medical degrees) of place or mode of instruction.

The part the existing University of London has played in the advancement of learning may be indicated by the fact mentioned by the Vice-Chancellor in his Presentation speech, that during the last thirty years—that is, since its examinations were thrown open—the number of degrees conferred by the University has increased tenfold. This, however, is only one of the ways in which its influence is shown; the great advance in scientific education the last fifty years have witnessed is almost wholly due to the stimulus and example of the University of London. But the subject is too large a one to be dealt with on the present occasion, and indeed, from its nature, scarcely lends itself to treatment capable of doing full justice to the University.

The work of a University should not be confined to the education of graduates. Its crowning function is the exposition and illustration of the higher learning along the whole line of advance. Such is the task so admirably accomplished by the Sorbonne and the Collège de France, and to the world of science and learning in London the University of London is peculiarly well adapted, by its independence and impartiality, to render similar services. Some years ago an attempt was made to work out a scheme having this end in view, but, in deference to reasons that no longer exist, it was found necessary to abandon its further prosecution. Its resumption has now become, or may shortly become, simply a question of means, and the time is at hand when a strong effort ought to be made to afford scholars and men of science in London some of the advantages their brethren have so long enjoyed in Paris.

Richmond, May 19. F. VICTOR DICKINS.

Co-adaptation.

WRITTEN letters remain. It is for anyone who may read this correspondence through at one time to judge on which side lie the "valid" distinctions, and on which the "invalid" confusions—not to mention comparisons in respect of "verbiage" or mere personalities. But I am obliged to write once more to insist, for the fourth time, that my agreement with Prof. Meldola does *not* extend to the "conclusion as to the non-existence of co-adaptation," but only to stating that co adaptation must be proved not to exist, if "Mr. Spencer's argument" is to be logically met. And if, as Prof. Meldola now says, any such statement is to be found in his "review of Mr. Pascoe's book" (which, I repeat, merely reproduces "Mr. Wallace's argument" as to the *accumulation* of *adaptations*, without remarking that this has no relevancy to the argument from *co-adaptation*), it must be in that "language of their own" which the neo-Darwinians find "to be intelligible among themselves."

Christ Church, Oxford, May 15. GEORGE J. ROMANES.

A priori Reasoning.

I SEEM to have failed to make my contention clear to Mr. Cockerell, and will try once more. What I maintain is this:

that it is unscientific—unphilosophical—to state an hypothesis or formulate a theory, and much more so to make a categorical statement, when no antecedent facts are given nor any subsequent verification attempted. Thus, Mr. Cockerell asks the question, "Why is it that plants growing on exposed sea-shores have a tendency to lie upon the ground or otherwise *to evade the violence of the winds*" (my italics)? Now, what evidence has he to bring forward that the *purpose* of lying down is to evade the violence of the winds? So far, it is nothing more than his private opinion—an *a priori* assumption. It is true that he adds a reason, but it is also drawn from his own consciousness, and not from nature: "When a plant is growing among others, it has to compete with them in raising itself into conspicuousness." But do not dwarf plants ever compete? My experience of the South Downs, where plants are for the most part considerably dwarfed, is that the struggle between them is a severe one. Yet their flowers and foliage are fully exposed to sunlight and insects, as well as to severe gales of wind. Mr. Cockerell also appears to forget that what is true for one plant is true for another, and each must try to overtop all the others.

I would venture to warn our younger naturalists most earnestly against this *facilis descensus* of *a priori* reasoning without facts or verification. It has been the bane of metaphysics; and when a scientific man like Dr. Weismann puts forth, in the name of science, most deplorable illustrations of it in his late attempt to apply his theory to plants, it is time that some one should venture to protest.

In reply to his request, I would refer Mr. Cockerell to M. Verlot's pamphlet "Sur la Production et la Fixation des Variétés," in which he describes his method of creating and fixing dwarf plants by sowing seed late in the season. Also to M. Roujou's experiments in selecting the smallest seeds of plants (*Journ. d'Hist. Nat. de Bordeaux et du Sud-Ouest*, 1884). Mr. McNab also raised dwarf rhododendrons by using pollen from the smallest stamens. Want of space forbids me adding more on the subject. GEORGE HENSLOW.

The Natural Selection of Indian Corn.

In a former letter I had occasion to mention that *Zea maïs* varies in its period of maturing, and that at certain altitudes and latitudes, only some of the varieties (*i.e.* the early maturing) are able to mature at all, the rest being absolutely eliminated by natural selection in a single generation. A few days ago I received, through the kindness of Mr. James Fletcher, the new (1891) Report on Experimental Farms for 1890, published by the Canadian Government, in which are numerous statistics of experimental planting in different parts of the Dominion. On p. 296, Mr. T. A. Sharpe gives an account of the result of planting twenty-nine different varieties of Indian corn at Agassiz, British Columbia, which perhaps deserves notice, as illustrating the above-mentioned facts in a particularly clear way. Of the varieties planted (all exposed to the same kind of environment), the majority did not form any ears at all. Some formed very small ears, and others reached various stages of maturity, but only a very few actually matured.

For example, I will quote some of them:—

No. 1. Moore's Early Concord, corn matured, one of the best.
No. 3. Early Adams, corn matured to glazing stage.
No. 6. Mitchell's Extra Early White Flint, produced some matured ears.
No. 11. Marblehead Sugar, matured corn, ears very small.
No. 12. Narraganset, sweet, corn did not fill to tips of cob.
No. 14. Chester Co. Mammoth, no corn formed.
No. 21. Golden Dent, no ears formed.

T. D. A. COCKERELL.

3 Fairfax Road, Bedford Park, Chiswick, W., May 10.

The Soaring of Birds.

It seems a great pity that the simpler form of this question—wherein birds soaring steadily rise, in a gentle breeze, over a large plain—is needlessly complicated by the flight of sea-birds over waves.

We shall get the solution best by taking the former and less complicated case, wherein the pelicans, adjutants, cyrus, vultures, &c., slowly rise, by soaring alone, to great heights, under conditions where up-rushes of air are quite out of the question.

Upper Asam is a dead level, some 60 miles wide by 200 long, and over this area, wherein these birds rise by soaring alone, the air-drift is almost invariably from north-north-east, or else south-west, and at about 5 to 10 miles an hour. They do not seem to rise in a dead calm, nor yet in stormy weather, and I take it the desideratum is a slow air-drift, or gentle *steady* breeze.

That there are no up-rushes of air, I have fairly good proof in the small tufts of cotton, from the *Bombyx malabaricum*, which cross the field of my telescope when examining the Noga Hills at 10 to 20 or 30 miles; these are always beautifully horizontal at elevations from 200 to 2000 feet, coming from the plains and hills north-east of us.

So that out here there is no complication of the case by vertical movements of the air, as at sea. The question is not how large birds sustain themselves (without flapping their wings) in a wind, when there are rising and falling and strata of "different velocities"; but how large birds like the cyrus, adjutant, pelican, and vulture can rise from 300 to 3000 feet, in a *steady* breeze, without flapping their wings.

It is not mere flotation; they have to *raise* 20 or 30 pounds some 2000 feet, in addition to what the albatross does.

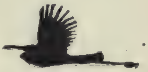
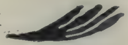
Surely this is the major question, at once simpler to see, and more difficult to answer.

In NATURE (vol. xxiii. p. 10) I drew attention to this, and sent a small diagram, to show how I thought it was done. I have frequently observed the phenomena since, and see no reason to modify my views.

Firstly, these large birds do not soar in a dead calm, or a storm, or during high winds. They prefer a steady breeze.

Secondly, they rise from the ground, by flapping the wings, and continue this till they are 100 or 200 feet up, and then begin to soar, in right or left hand spirals, 100 or 200 yards across. At each lap they rise 10 or 20 feet, and make as many yards leeway, drifting slowly *with the wind*, and continue thus to rise until out of sight above.

With a good telescope a bird can be easily followed after a little practice, and the only motion which can be seen is slight and occasional movement of the tail, in steering.

The legs (of the waders) are extended at full length behind, the neck thrown on the back, and beak projecting over the breast . The tips of the primary wing-feathers are always well separated in different planes, , and strongly curved up, thus,  evidently under great strain.

The lifting power is evidently applied to them mainly, and the plane of the outspread wings is not horizontal, but forms part of an obtuse, inverted cone, as though a little centrifugal force was implicated.

The speed of the bird is always greater than the breeze, and the resistance is unequal on opposite sides of the loop of the spiral; least when it travels with the breeze, and greatest when on the opposite half, meeting it.

It seems to me the solution is that, when going with the wind, the bird gathers momentum by going down a slight incline, and when it turns and meets the breeze, this extra momentum is used in lifting the bird and carrying it over a *shorter* course. Thus it starts the next lap at a *slightly* higher level, but some 20 yards to leeward. Variation of the speed of the wind at different levels is here quite out of the question; the bird, too, keeps to its steady spiral, and as steadily ascends at each lap.

I feel sure that Prof. Tait, Sir W. Thomson, and Lord Rayleigh will find the case I state a more profitable one to study than the erratic flight or floating of sea-birds. The telescope I use to watch and follow these birds when soaring is a 3·5" O.G. power 50, with long tripod legs, and on a mattrass below I find no difficulty in keeping a bird in the field, if at 1000 feet up. My own idea is that all these birds go up there to sleep or doze.

Sibsagar, Asam, March 30. S. E. PEAL.

ON SOME POINTS IN THE EARLY HISTORY OF ASTRONOMY.[1]

III.

WE now come to the important point for our present inquiry—the direction in which the temple is built, or, technically, its orientation. Confining ourselves for the moment to Karnak, is there any meaning in the direction of that line, some 500 yards long, which is obviously the main feature of the building, and to which all parts are accessories?

How can we instrumentally determine this? I have the necessary apparatus here, and the question may be answered in a few minutes; we have simply to determine either the azimuth or the amplitude (and as we have seen one of these gives the other) of the point of the horizon towards which this long line is directed.

The azimuth compass is an instrument familiar to most of you. It consists of a magnetic needle fastened to a card carrying a circle divided into 360°, which can be conveniently read by a prism when the instrument is turned toward any definite direction marked by a vertical wire.

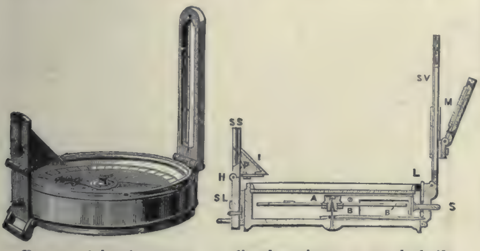


FIG. 11.—Azimuth compass. SV, direction pointer; M, sun shade (for observations of sun); I, reading prism; A B, graduated card with attached compass-needle.

A theodolite armed with a delicately hung magnetic needle which can be rotated on a vertical axis will do equally well; it has first of all to be levelled; there is a little telescope with which we can see along the line.

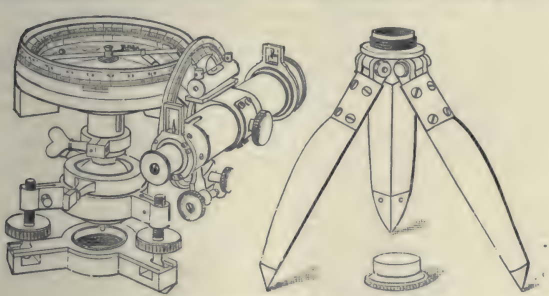


FIG. 12.—Compass theodolite and tripod.

When we wish, for instance, to observe the amplitude of a temple, the theodolite is set up on its tripod in such a position that we can look along the temple wall or line of columns, &c., by means of the telescope. We then get a magnetic reading of the direction, after having unclamped the compass; the compass showing the angle made between the line and the magnetic north (or south), as in the azimuth compass.

Having made such an observation as that I have described, the next thing we have to do is to determine astronomically the real north, which is the only thing of value. There are two ways of getting this *astronomical* bearing or azimuth.

It is sad to think how much time has been lost in the investigation of a great many of these questions, for the reason that the observations were made only with reference to the magnetic north, which is vastly different at different places, and is always varying; few indeed have tried to get at the astronomical conditions of the problem. Had this been done either by the French or Prussian Commissions to which I have referred, it is perfectly certain that years ago the solstitial orientation of Karnak and other temples which I shall have to demonstrate to you would have been long known to all scholars.

If the magnetic variation has been determined for the region we may use a map. Such a map as that shown in Fig. 13 gives us the lines along which in the British Isles the compass variation west of north reaches certain values. From such a map for Egypt we learn that in 1798 a magnet swung along a line extending from a little to the west of Cairo to the second cataract would have had a variation of 12° to the west; in 1844 of 8° to the west; and at the present time the variation is such that observations made along the same part of the Nile valley will have a variation closely approximating 5° to the west. By means of such a map it is quite possible to get approximately the astronomical bearings of all temples which were observed by the French in 1798 or by the Germans in 1844, or which can be observed in the present day.

If we are not fortunate enough to possess such a map, the theodolite will enable us to observe the direction in which the sun culminates at noon. This gives us the south point astronomically. From observations of the pole star at night, the astronomical north can be determined. From either of these observations the magnetic variation is obtained without any difficulty.

This being premised about the method, we next come to the results. The amplitude of the point to which the axis of the great temple at Karnak points is 26° N. of W., which we learn from the table already given is precisely the amplitude of the place of sunset at the summer solstice. The amplitude of the point to which the axis of the small temple points is 26° S. of E., exactly the position of sunrise at the winter solstice.

There is more evidence of this kind. Abydos, one of the oldest temples in Egypt, built, according to tradition, by the servants of Hor, is now, it is true, a heap of ruins, the brick walls best showing its direction; but it is possible to gather the orientation of it by these guiding lines. It is 27° N. of W.—as it should be, being in a higher latitude than Karnak—and evidently was oriented to the solstice.

At Abydos, then, as at Karnak, we get exactly, within a degree, the amplitude shown in the tables for the sun in the Nile valley at sunset at the summer solstice. So that the Egyptians who were employed in building those temples must have known exactly what they were going to do, and what they did was to build a temple such that the sun at setting should, at the summer solstice, pour its light along the axis of the temple. If Maspéro and the great authorities in Egyptian archæology are right—namely, that the Abydos temple was founded before 4000 B.C.—and if we can depend upon the French figures, we are driven to the conclusion that we have in this temple a building which was orientated to the solstitial sunset place in the valley of the Nile. The Nile valley holds other solar temples besides those we have named, but it is best to fully study Karnak; instead of being a mere heap, the orientation of which is obtainable only by the general lie of the remains, this temple is still in such preservation that the Germans, in the year 1844, could give us an infinite number of details about it, and locate the position of the innumerable courts. Its orientation to the solstice we can claim as an early astronomical observation. So it is quite fair to say that, many thousand years ago at all events, the Egyptians were perfectly familiar with the solstices, and therefore more or less fully with the yearly path of the sun.

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[1] Continued from p. 11.

But so far we have only dealt with solstices. Did the Egyptians know anything about the equinoxes? Certainly. Nothing is more remarkable than to go from the description and the plans of such temples as we have seen at Abydos and Karnak to regions where, apparently, the thought is totally and completely different, such as we find on the Pyramid Plains at Ghizeh; the orientation lines of the German surveyors show, beyond all question, that these structures are just as true to the sun-rising at the equinoxes as the temples at Abydos and Karnak were to the sun-rising and setting at the solstices, and the Sphinx was merely a mysterious nondescript sort of thing which was there watching for the rising of the sun at an equinox, as the Colossi of the plain at Thebes were watching for the rising of the sun at the winter solstice.

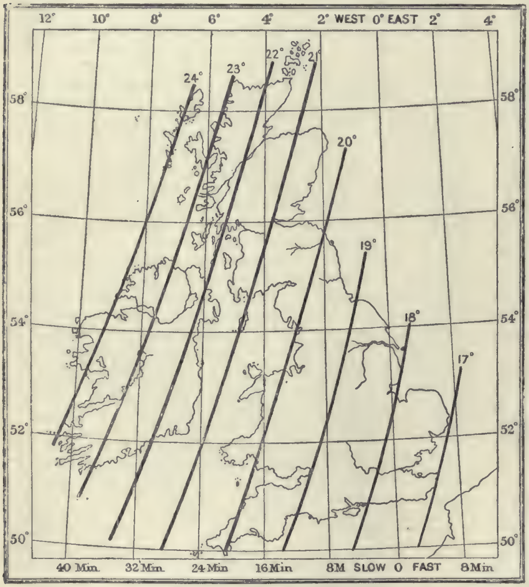

Fig. 13.—Map of British Isles showing the magnetic variation.[1]

The observations which have been made in Babylonia are very discordant among themselves, and at present it is impossible to say, from the monuments in any of this region along the Euphrates valley, whether the temples indicate that the solstices were familiar to the Babylonians; but no doubt some of the temples were as perfectly squared to the equinox as some walls at Memphis or the Pyramids at Ghizeh; and certainly there is no doubt that as early as Solomon's time the temple at Jerusalem was orientated to the east with care. We find there that the direction of the axis of the temple shows the existence of a cult connected with the possibility of seeing the rising of the sun on the day of an equinox, possibly at the time which we now call Easter.

All the doors being opened, the sunlight would penetrate over the high altar, where the sacrifices were offered, into the very Holy of Holies, which we may remember was only entered by the High Priest once a year.

Have we any other evidence except the evidence afforded by temples? Yes. It has been stated that we have no temple evidence from China, but there is a good deal of written evidence, and there is no doubt that in China the solstices and the equinoxes were perfectly well known 1100 years B.C. Was it difficult to obtain this knowledge? Did it indicate that the people were great astronomers? Nothing of the kind; nothing is so easy as to determine a solstice or an equinox.

We know from the Egyptian tombs that their stock-in-trade, so far as building went, was very considerable; they had squares, they had plumb-lines, they had scales, and all that sort of thing just as we have. Suppose an Egyptian wished to determine the time of an equinox. He would first of all make a platform quite flat; he could do that by means of the square or plumb line; then he would get a ruler with pretty sharp edges (and such rulers are found in their tombs), and in the morning of any day he would direct this ruler to the position of the sun when it is rising and he would draw a line; he would do the same thing in the evening when the sun set; he would bisect the angle made by these two lines, and it would give him naturally the north and south points, and a right angle to those would give him the east and west. So that from observation of the sun on any two days in the year he would practically be in a position to determine the position at which the sun would rise and set at the equinox.

There is another way of doing it. Take a vertical rod. Suppose that the sun is rising, let the rod throw a shadow; mark the position of the shadow; at sunset we again note where the shadow falls. If the sun rises exactly in the east and sets exactly in the west, those two shadows will be continuous and we shall have made an observation at the absolute equinox. But suppose the sun not at the equinox, a line joining the ends of the shadows equally long before and after noon will be an east and west line.

It is true that there may be a slight error unless we are very careful about the time of the year at which we make the observations, because when the sun is exactly east or west at the time of rising or setting it is moving most rapidly. So it is better to make the above observations of the sun nearer the solstices than the equinoxes, because the sun changes its declination most quickly at the equinoxes.

Such a rod as this, which I may state is sometimes called a *gnomon*, may be used with another object in view: we may observe the length of the shadow cast by the sun when it is lowest at the winter solstice, and when it is highest; at these two positions of the sun obviously the lengths of the shadows thrown will be different. When the sun is nearest overhead in the summer the shadow will be least, when the sun is most removed from the vertical the shadow will be longest.

The day on which the shortest shadow is thrown at noon will define the summer solstice; when the shadow is longest we shall have the winter solstice.

This in fact was the method adopted by the Chinese to determine the solstices, and from it very early they found a value of the obliquity of the ecliptic.

It may be said that it is only a statement, and that the record has been falsified; some years ago anyone who was driven by facts to come to the conclusion that any very considerable antiquity was possible in these observations met with very great difficulty. But the shortest and the longest shadows recorded (700 years B.C.) do not really represent the true lengths according to recent knowledge. If anyone had forged these observations he would state such lengths as people would find to-day or to-morrow, but the lengths given were different from those which would be made to-day. Laplace, who gave considerable attention to this matter, determined what the real obliquity was at that time, and proved that the record does represent an actual observation and not one which had been made in later years.

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[1] For Figs. 11–13 I am indebted to the kindness of Mr. Stanley, Great Turnstile, Lincoln's Inn Fields.

The solstices and the equinoxes were therefore in all probability thoroughly known to the Egyptians 4000 years B.C., perhaps even 5000. We are then justified in considering that the temples at Abydos and at Karnak are really solar temples. The Egyptians marked the solstices and the equinoxes not only by their temples but in their calendars, which these temples enabled them to construct. The Chinese had also this knowledge, but we have no information that they possessed it at so early a date.

In the next place, then, I propose to make a special study of the temples at Karnak, because they are those which are most capable of minute investigation. I do this in order to see whether any other indications can be obtained of any higher knowledge possessed by the Egyptians of those early times.

I must again point out that we deal with the solstices in the case of the temples at Abydos and Karnak, and with the equinoxes in the case of the pyramids, some mounds in Babylonia, and the Temple at Jerusalem. Since the labours of the French and Prussian Governments who have given such full records of Karnak, a memoir on the temples has been published by Mariette, which gives us not only plans, but precious information relating to the periods at which, and the kings by whom, the various parts of the temples were constructed or modified.

We may begin by the general plan of Thebes. We find there a perfect nest of temples. No doubt those which are still traceable form only a very small portion of those which once existed, but however that may be, I have now only to call attention to one or two among them. In the general plan we see indications that on both sides of the Nile there were temples pointing to those special amplitudes which I have before referred to. What we have first to do is to refer to the solstitial temples, those which point to 26° N. or S. of E. or W., in which we have undoubtedly indications of the early attempts to observe, or to worship, the sun at sun-rising and at sun-setting, at the critical times—the solstitial times of the year.

The first point that I wish to make is that these temples—whatever views may be entertained with regard to their worship or the ceremonial in them—were undoubtedly constructed among other reasons for the purpose of obtaining an exact observation of the precise time of the solstice. The priests having this power at their disposal, would not be likely to neglect it, for they ruled by knowledge. The temples were, then, astronomical observatories, and the first observatories that we know of in the world.

If we consider them as horizontal telescopes used for the purpose I have suggested, we at once understand the long axis, and the series of gradually narrowing diaphragms, for, the longer the beam of light used, the greater is the accuracy that can be obtained.

It is worthy of note that the direction of the temple at Karnak is quite independent of the locality, it has nothing to do with the presentation of the temple to the Nile or to any other particular part of the landscape, and that point, I think, is absolutely settled by the consideration that we have temples at the same amplitude in different localities up and down the Nile Valley, where, although they are parallel to each other, their presentation to the river in the different localities is very various.

What then was the real use of these pylons and these diaphragms? It was to keep all stray light out of the carefully roofed and darkened sanctuary; but why was the sanctuary to be kept in darkness?

Independently of ceremonial reasons—there is a good deal to be said under that head—it is quite clear that the darker the sanctuary the more obvious will be the patch of light on the end wall, and the more easily can its position be located. It was important to do this on the two or three days near the solstice in order to get an idea of the exact time at which the solstice took place. We find that a narrow beam of sunlight coming through a narrow entrance some 500 yards away from the door of the Holy of Holies would, provided the temple were properly orientated to the solstice, and provided the solstice occurred at the absolute moment of sunrise or sunset according to which the temple was being utilized, practically flash into the sanctuary and remain there for about a couple of minutes, and then pass away. The flash would be a crescendo and diminuendo, but the whole thing would not last above two minutes or thereabouts, and might be considerably reduced by arrangements of curtains. Supposing the solstice did not occur at the precise moment of sunrise or sunset, and provided the Egyptians by any means whatever were able to divide the days and the nights into more or less equal intervals of time, two or three observations of the sun-rising at the solstice on three different mornings, or of the sunset at the solstice on three different evenings, would enable a careful observer to say whether the solstice had occurred at the exact moment of sunrise or at some interval between two successive sunrises, and if the latter, what that interval was.

I now come to my next point, which is that here we have the true origin of our present means of measuring time—that our year as we know it was first determined in these Egyptian temples and by the Egyptians. We have seen that it did not require any great amount of astronomical knowledge to determine either the moment of the solstice or the moment of the equinox. I think you will agree with me that the most natural thing to begin with was the observation of the solstice, for the reason that at the solstice you can watch the sun day after day getting more and more north or more and more south until it comes to a standstill. But for the observation of the equinox, of course, the sun is moving most rapidly either north or south, and therefore it would be more difficult to determine in those days the exact moment, so that I have little doubt that what they attempted in the first instance was to mark the absolute moment of the solstice. If that be so, and if Maspéro is right that Abydos was built before Menes, then we know definitely that the Egyptians could and did observe the solstices, and knew what they were doing, 7000 years ago.

Before I say anything more about the use of these temples in determining the year, it is worth while to note how very different the treatment of this subject was in Egypt to what it was in Chaldæa and Babylonia and among the Jews. We do of course in the Egyptian inscriptions read of the moon, but in Chaldæa it would seem that the moon was the chief thing worshipped, and it was thus naturally the chief means used for measuring time, and, as far of course as months were concerned, this was quite right. In Chaldæa, where they were not dependent upon the rising of the Nile, and where much desert travel had to be undertaken at night, the moon and the month were the points considered, and the sun was hardly regarded at all from that point of view. An interesting point connected with this is that, among any of these ancient peoples, the celestial bodies which gave them the longest period of time by which they reckoned were practically looked upon in the same category.

Thus, for instance, in Egypt the sun being used, the unit of time was a year; but in Chaldæa the unit of time was a month, for the reason that the standard of time was the moon. So that when people began speaking about periods of time it was quite easy for one nation to conceive that a period of time was a year when really it was a month, and *vice versâ*. It has been suggested that the years of Methuselah and other persons who are stated to have lived a considerable number of years were not solar years but lunar years—that is, properly, lunar months. This is reasonable, since if we divide the numbers by 12 we find that they come out very much the same length as lives are in the present day.

The Egyptians, taking the sun as their measurer of time, began very early with a year of 360 days. For some reason or other they divided these 360 days into months, probably with some lunar connection, so that they had 12 months of 30 days. Now, we know that that is not the true length of the year, and it is clear that any nation which uses such a year as that will find its festivals going through the year. Further, such a year as that is absolutely useless for the agriculturist or the gardener, because after a time the same month, to say nothing of the same day of the month, will not mean reaping-time, will not mean sowing-time, or anything else. So that this 360-day year did not last very long; so long as it lasted, however, they knew that they got the seasons back to months of the same name in a period of 70 years.

This method led to complications, which possibly may have had something to do with the building of these temples. Egypt being exclusively the gift of the Nile, you can quite understand that their earliest calendar would be connected with the Nile, and so one finds it. We and other peoples occupying the zone in the north divide the year into four seasons; the Egyptians divided it, and still divide it, into three: they have four months of the flood of the Nile, then they have four months after the Nile has retired, in which they do their sowing, and then they have other four months which they call their summer, in which they gather their harvest.

We began, then, with a year of 360 days, and, having 360 days instead of 365¼, we had a cycle of 70 years, and during that cycle each day of the year meant something different with regard to the advance of the seasons, and with regard to the work of the agriculturist and the gardener to what it had meant in the preceding year. But this state of things did not last long. The 1st of the first month fell at the summer solstice on June 20, and the reason that it fell then was, that the inundation of the Nile reached Memphis on that day. Whether with the help of the temples or not, they soon got very much nearer, and changed the year of 360 for one of 365 days, which is, roughly, within a quarter of a day of the truth. They had still their 12 months of 30 days, and then they added an extra month of 5 days. With their perfectly orientated temples they must have soon found that their festival at the summer solstice—which festival is known all over the world to-day—did not fall precisely on the same day of the new year, because, if 365 days had exactly measured the year, that flash of bright sunlight would have fallen into the sanctuary just as it did 365 days before. But what they must have found was, that after an interval of four years it did not fall on the first day of the month, but on the day following it. They at once faced this, and found out that 365 days did not exactly make a year, but that they had to do with a quarter day in addition. What the Chinese did was this: every fourth year, instead of adding 5 days to their 360, they added 6 days, and in that way they practically brought the calendar right.

Theory indicated that retaining the 365-day year, the 1st of the first month would come back to its exact relationship to the inundation of the Nile after a period of 1460 years, the 1460 years of course depending upon the quarter being added (365 × 4 = 1460).

This was known in Egypt to the priests alone. They would not allow the year of 365 days, called the *vague* year, to be altered, and so strongly did they feel on this point that every king had to swear when he was crowned that he would not alter the year. We can surmise why this was. It gave great power to the priests; they alone could tell on what particular day of what particular month the Nile would rise in each year, because they alone knew in what part of the cycle of 1460 years they were, and in order to get that knowledge they had simply to continue going every year into their Holy of Holies one day in the year as the priests did in Jerusalem, and watch the little patch of bright sunlight coming into the sanctuary. That would tell them exactly the relation of the true solar solstice to their year, which was supposed to begin at the solstice, and the exact date of the inundation of the Nile could be found by those who could determine observationally the solstice, but by no others.

In reading books on Egypt we come across another cycle which is supposed to be a very mysterious one; in fact it is one which, I think, has not yet been sufficiently investigated, and it is very well worth the trouble of anybody who will give the time. They begin with a year of twelve months, each of which has thirty days, thus giving 360 days; this was found not to work. They then tried 365 days, but that also would not work, because then the first day of Thoth (their first month) would only indicate the inundation of the Nile one year out of 1460; and then the priests interpolated the other day and got the cycle right, but it was not yet quite right. In the time of Hipparchus 365·25 did not really represent the true length of the solar year; instead of 365·25 we must write 365·242392—that is to say, the real length of the year was a little less than 365¼ days.

Now the length of the year being a little less, of course we should only get the absolute coincidence of the 1st of Thoth with the inundation of the Nile in a longer period than the 1460 years cycle; and, as a matter of fact, the 1460 years had to be expanded into 1506 to fit the months into the years with this slightly shortened length of the year; so we have a period which is called *sothic*, of 1460 years; and a period which is called *phœnix*, of 1506 years.

There is a great wealth of interest connected with the uses of the temples from the point of view of worship, but that does not concern us here, except that it is intimately connected with the next part of the subject, for I have next to point out that it necessitated in Egypt, Chaldæa, and elsewhere contemporaneous observations of the stars. I therefore now pass from the sun to the stars.

J. NORMAN LOCKYER.

(*To be continued.*)

FORESTRY IN NORTH AMERICA.

IN continuation of the notes under the above heading which appeared in NATURE last January, I wish to refer to a splendid paper[1] recently read by Sir Dietrich Brandis, F.R.S., to the Natural History Society of Bonn. It consists chiefly of a compilation from Dr. Mayr's book, "Die Waldungen von Nord America" (Munich, 1890), and from works by Prof. Sargent Bernhard Fernow, the present Chief of Forestry at Washington, and some other authors, as well as from the Agricultural Reports of the United States.

Dr. Mayr is the son of a Bavarian State forest officer, and, after studying forestry and botany at Munich, he was sent, at the expense of the Bavarian Government, to observe in their native forests, at different ages, certain important

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[1] "Der Wald in den Vereinigten Staaten von Nord America," von Dr. D. Brandis in Bonn, 1891. (Sonder Abdruck aus den *Verhandlungen des Naturhistorischen Vereins*, 47 Jahrg.)

North American forest trees, experimental plantings of which have from time to time been made in Germany. After spending seven months on these researches, and extending his tour through Japan, Java, Ceylon, and Northern Hindustan, Dr. Mayr returned to Germany in 1888, and was shortly afterwards appointed Professor of Forestry and Forest Botany at the College of Agriculture and Forestry at Tokio in Japan. The present writer had the great pleasure of accompanying him in January 1888 for about three weeks through some of the coniferous and oak forests of the North-Western Himalayas and the subtropical forests of the lower hills near Dehra.

After leaving Germany a second time for Japan, Dr. Mayr had a further opportunity of visiting North America, and thus has twice traversed the length and breadth of the country between the Dominion of Canada and Mexico.

Mayr treats of the demands of the most important North American trees as regards climate and soil, with a summary account of their anatomical structure and of the physical and technical qualities of the most important woods, and his book contains numerous illustrations. He also gives lists of destructive fungi and insects observed by him on the different species.

Brandis has some criticisms to mete out for a few somewhat rash generalizations made by Mayr. These are that evergreen broad-leaved (not coniferous) forest requires a higher winter temperature than deciduous forest, and that deciduous forest vegetation is always absent in tropical countries on account of the uniformity of the climate throughout the year. Brandis shows clearly, from a comparison with the deciduous forests of teak and other species in India, Burma, and Java, that this statement will not hold wherever there is a prolonged dry season, which renders the trees leafless for a certain period of the year.

Another statement of Mayr's controverted by Brandis is that conifers never grow in tropical countries except where the altitude renders the climate non-tropical, and that in North America they have longer needles, supply heavier timber, and contain the more resin, the nearer they grow to the tropics. The latter statements may be true for *Pinus australis*, the pitch pine of the Southern States of North America, but do not hold good in India, where the *Pinus longifolia* of the Himalayas has the longest needles and probably yields as much resin as the tropical pine (*P. Merkensii*), which, however, has the heaviest wood of all the Indian pines, and grows in latitude 17° N., in Tenasserim, at about 600 feet above sea-level, in an absolutely tropical climate.

Mayr's statement that oranges will only grow to perfection in a hot dry climate is also not true for India, as oranges of splendid flavour are grown in enormous quantities in the damp lower hills below Cherapunji, in Assam, where the rainy season lasts for eight months, as well as in the dry regions near Delhi, and the comparatively dry country near Nagpur, in the Central Provinces of India.

Apart from these criticisms and an interesting discussion on the origin of prairies, we find in Brandis's paper a most complete account of the distribution of North American forest trees.

Forest vegetation is much richer in North America than in Europe, containing about 412 species, distributed as follows:—

Atlantic region	176
Pacific region	106
Common to both	10
Central region on and surrounding Rocky Mountains	46
Tropical species near the coasts of Florida ...	74
	412

as against 158 species in Europe.

At least six North American species of forest trees, according to Brandis, are also indigenous in Europe, being—

Cercis canadensis = *Siliquastrnm*
Diospyros virginiana = *Lotus*
Celtis occidentalis = *australis*
Platanus occidentalis = *orientalis*
Ostrya virginica = *carpinifolia*
Castanea americana = *vulgaris.*

All these species now grow naturally in Europe south of the Alps, and since many American forest genera existed in Europe in Tertiary times, whilst only five European forest genera (Ceratonia, Laburnum, Olea, Syringa, Laurus) are not found in America, it is possible that other species formerly common to both countries were destroyed in Europe north of the Alps by the Glacial epoch.

It would take too long to describe each region in detail, and I must here merely glance at them in the briefest manner.

A small outlier of the West Indian tropical flora extends into the south of Florida, and is followed by a broad zone of evergreen broad-leaved forest, of which *Magnolia grandiflora* is the chief representative. We then get the pitch pine forests on the sandy formations of Florida, Georgia, North and South Carolina, extending westwards to Alabama and Mississippi. The wood of the pitch pine (*P. australis*) is the best coniferous wood in the world, but the forests are being utterly ruined. They are tapped in the most wasteful manner for turpentine, 8,000,000 dollars being the estimated local value of the annual return. More wood is burned than is utilized, and, according to Mayr, already wide belts of white sterile shifting sands border both sides of the railways of the Gulf States, showing what the poorer tracts of the country will come to, if the farmers do not give up their pernicious habit of burning thousands of square miles of forest every year.

Another tree of the Southern Atlantic zone is the swamp cypress (*Taxodium distichum*), growing on annually inundated land, and presumably safe from fire, if not from ill-regulated and wasteful felling.

The valuable pencil cedar (*Juniperus virginiana*) also flourishes at its best in the Southern Atlantic region, but grows almost everywhere in the United States and British America, from latitude 54° southwards. To the north and in the prairies it has, however, only a stunted growth. Hardly any sound wood of this species is now procurable, as I learned last year from Messrs. Faber and Co. at Nuremberg. Next to this zone comes the description of the broad-leaved deciduous forest of the temperate region, containing many oaks, walnuts, hickories, and the tulip tree (*Liriodendron tulipifera*). The heavy seeded trees are found chiefly in the south, and lighter seeded ones, as maples, birches, and elms, more to the north.

There is a long account by Brandis of the prairie region, and the region of thinly-stocked forest bordering on it; and it appears that here, as cultivation extends, and the fires do not sweep over such vast extents of land as they did formerly, woods of Mesquit bean (*Prosopis juliflora*), and other trees are spreading by seed or coppice shoots, in Western Texas, and also in Wisconsin, Illinois, Iowa, and other States. Much has been done in the prairie region by plantations, and these succeed admirably wherever the climate is sufficiently moist; but in the central and western parts of Kansas all planting has hitherto failed, owing to the extremely dry climate.

In the northern pine zone of the Atlantic forest region, *Pinus Strobus*, the Weymouth or white pine is the most important species, and formerly covered enormous tracts from the Gulf of St. Lawrence to North Georgia, and beyond the sources of the Mississippi. At present, the only considerable supply of white pine is in Canada, and in the lake districts of the States of Michigan, Wisconsin, and Minnesota. The timber operations in the white pine forests have only one object, which is to bring as much timber as possible out of the forest in the shortest possible time, and to make money. Only the best trees are felled, and the rest burned. A forest after a timber gang has left it presents a remarkable appearance: between the standing blackened and partially charred stems of the broad-leaved and other trees which have not been felled are the stumps of the felled pines, whilst the ground is covered with wood, which would not have paid for its removal, and rots, or is burned by the annual fires.

In 1880, there were in the three lake districts 7000 million cubic feet of standing white pine timber, whilst in the last ten years 6205 millions of cubic feet have been felled and exported, 750 millions in 1889 alone. There is, therefore, little more left than can be exported in a single year. Many of the large saw-mills have already been obliged to stop work, or get timber from Canada. Chicago, which owes its rapid rise to the timber trade, imports yearly 166,000,000 cubic feet of white pine timber. This is about three-fourths of the whole forest yield of Prussia, the produce of 6,750,000 acres or 10,547 square miles of forest. Besides the Weymouth pine, *Pinus Banksiana*, the grey pine, and *Pinus resinosa*, and various broad-leaved trees are found. The sub-Arctic region of Alaska and British North America is poor in species; *Picea alba* and *nigra*, the white and black spruce, being characteristic trees.

Merely glancing at the North Mexican forest region, with forests of *Prosopis juliflora*, and grassy tracts containing gigantic cacti, and *Yucca baccata*, a palm lily, attaining 40 feet in height, we come to the Pacific forest region, where the Douglas fir, *Pseudotsuga Douglasii*, is the most important tree, and yields, in suitable localities, perhaps the greatest quantity of timber per acre of any known species.

We finally come to the red wood forests of the Pacific coast, where *Sequoia sempervirens* prevails, its congener *Sequoia gigantea* only occurring over a limited area. Unregulated fellings also prevail in the Douglas and red wood forests, and their supply cannot last much longer.

Besides the wholesale destruction of forests which goes on in America, and has already driven the United States to remove all duty from Canadian wood, the most appalling destruction is now being annually caused by the floods which pour down the slopes of the mountains, bringing down boulders, stones, and gravel on the cultivated lands below. Mayr has seen standing trees covered with mud up to a height of 15 feet in some of the Southern and Central States, whilst hundreds of magnificent trees lay uprooted in the full vigour of their growth. This can clearly be traced to the destruction of the hill forests.

How long will rulers of the United States shut their eyes to the appalling waste of the resources of their country which is still rampant! Brandis hopes that private capitalists may invest their money in forests, tempted by the rapid rise in the price of wood, and may manage them properly; but all European experience points to the necessity of State forests, and a trained State Forest Service to manage them, as the only efficacious remedy against the impoverishment of the soil and natural resources of America. W. R. FISHER.

DAILY INTERNATIONAL WEATHER CHARTS.

AT the meeting of the Meteorological Congress at Vienna in September 1873, General Myer, the Chief Signal Officer of the United States Army, submitted the following proposal:—

"That it is desirable that, with a view to their exchange, at least *one* uniform observation of such a character as to be suitable for the preparation of synoptic charts be taken and recorded daily and simultaneously at as many stations as practicable throughout the world."

Although various suggestions had been made before, and synoptic charts had been previously constructed for large areas, this proposal was a bold step in advance, as the charts hitherto published—those of the English Meteorological Office excepted—were mostly synoptic only, but not strictly synchronous, whereas the plan now proposed was to treat the whole observational area of the globe as a unit, and to represent the actual conditions existing at the same instant of physical time.

The proposal was well received, and on January 1, 1875, General Myer was able to publish his daily International Bulletin, and to supplement this, on July 1, 1878, by the daily International Weather Map. These publications were continued until the end of March 1884, after which time the daily Bulletin was discontinued, but the chart was issued on an enlarged scale, containing data referring to pressure and wind direction and force at all reporting stations in the northern hemisphere and over the northern portions of the Atlantic and Pacific Oceans, and this has been published up to the end of December 1887. We have before referred to the ability with which this great undertaking has been carried out by the Signal Service. The necessity of obtaining strictly simultaneous observations was generally acknowledged after the discovery of Buys Ballot's law of the relation between wind force and barometric pressure, about the year 1857, and it is almost entirely due to the construction of synoptic charts over large areas that so much progress has been made in weather prediction in the last quarter of a century. This progress would hardly have been possible while each country dealt exclusively with its own area, notwithstanding the great advance made over the old system of dealing with means of observations by the publication of telegraphic weather reports and weather charts. But notwithstanding the progress already made, we are still unable to foresee what may occur for more than a day or so in advance. Much more research is required, and the thousands of observations now taken on land and sea over the globe should be plotted at least once a day. We should therefore much regret the discontinuance of such work as that now before us, which deals with nearly half the globe.

To take one or two of the facts shown by the charts themselves: the very severe gale which visited these islands on December 8 and 9, 1886, in which about the lowest barometer reading on record was observed, will be remembered in connection with the capsizing of the Southport and St. Anne's lifeboats near Formby, resulting in the loss of twenty-seven lives out of twenty-nine which constituted the two crews. In a paper upon this storm, read before the Royal Meteorological Society on April 20, 1887, by Mr. C. Harding, it is stated, after a careful examination of the materials then available, that "the Atlantic was in such a disturbed condition at this time that it is not possible to track the passage of the storm across the Atlantic with any certainty." The daily International Charts, however, show the position of the storm day by day, and also that it did actually cross the Atlantic from shore to shore, and was central over the Gulf of St. Lawrence on December 3.

Another instance of remarkable weather, it will be remembered, occurred in June 1887—the Jubilee year; the weather was remarkably dry and fine in this country, there being an extraordinary drought of about thirty days. The charts for that period show that similar anticyclonic conditions also embraced a very large part of the eastern portion of the Atlantic, and extended abnormally over a portion of Europe; while the travelling disturbances are plainly shown to be confined to the American side of the ocean.

It is only Government organizations that can undertake the laborious work of producing such charts; but when they are published, the matter should not be left there: the meteorologist should make use of the materials provided for him, and endeavour to solve the problems which underlie weather changes and the general movements of the atmosphere.

JOSEPH LEIDY, M.D.

THIS well-known American naturalist was born on September 9, 1823. He very early in life showed a fondness for collecting and observing insects, one of his first contributions being a paper on the mechanism which closes the membranous wings of the genus Locusta, published in 1845 in the Proceedings of the Academy of Natural Sciences of Philadelphia. Having taken his degree in medicine, he devoted himself more and more to the study of natural history, and few men of any nation have left behind them a longer list of work done than this distinguished man, whose death we announced in a recent number. Leidy was gifted with great powers of observation, he possessed a correct eye and steady hand for the delineation of whatever objects he was observing, he was endowed with a faculty for work; and as he had also an excellent memory, one reflects upon his half-century of work with less of surprise than admiration. To give an account of his writings would be to write a volume, to give but their titles would be to fill many of our columns, so that it must suffice to call to mind rather the subjects about which he wrote than the writings. Commencing with a study of entomology, and working more at the anatomy than at the general morphology of insects, he quickly passed on to the study of the entophytic worms, his "Flora and Fauna within Living Animals," published as one of the Smithsonian Contributions in 1852, having made its mark at the time. Then he took up the fresh-water Polyzoa, his labours on which will be understood only when a monograph on this group as inhabiting America comes to be published. Leaving for a time the study of invertebrate forms, he next entered on the field of research among the fossil vertebrates, describing in quick succession a number of remarkable fossil reptiles and fish, and he was the author of the first volume of the quarto series of reports issued by the United States Geological Survey of the Territories, under the title of "Contributions to the Extinct Vertebrate Fauna of the Western Territories." It was during his journeys to the Western Territories, that, not content with investigating the fossil vertebrates of the district, he worked very diligently at the study of the microscopic forms of life which inhabit the waters met with therein, and these researches, so far as one group of animals is concerned, were happily published by the United States Geological Survey in 1879, in one large quarto volume, "The Fresh-water Rhizopods of North America," which is illustrated by forty-eight coloured plates after Leidy's own drawings. This work on its appearance was received with great enthusiasm, and is still a worthy model for a monograph. During all these years, and amid so many and so varied labours, Leidy still discharged his duties as Professor of Anatomy to the University of Pennsylvania, and also of teacher of natural history to the classes of boys and girls at the Swarthmore College. No doubt many of these latter pupils will now call to mind the warm personal interest their master always took in their labours. In one of his books he tells us that since he was fourteen years of age the study of natural history was to him a constant source of happiness; but that on this joy a shadow was constantly cast when he thought how few, how very few, of those around him gave any attention to intellectual pursuits of any kind, and it saddened him to feel that the command "that man shall not live by bread alone" remained so unappreciated by the great mass of even so-called enlightened humanity. The results of Leidy's intellectual pursuits will long remain to testify to the manner of man that he was.

THE SCIENCE MUSEUM.

THE discussion on this all-important question continues in the press. The Whitsuntide holidays have prevented any questions being asked in the House of Commons, where the feeling is very strong against the action of the Government.

As before, we reprint the most important items in the discussion. These consist of letters from Sir H. Roscoe and Profs. Armstrong and Ayrton to the *Times*. We commend to our readers the reference by the latter to Mr. Goschen's treatment of the deputation, and also their judgment as to the present position of science in this country, and the teaching of it in London, as compared with Göttingen and Zürich. No one can speak with greater authority than Profs. Armstrong and Ayrton on this subject.

Our administrative system, however, is such that the present question, which is acknowledged to be of such high importance, is being settled exclusively by officials who are quite ignorant of science. This is not said to their disparagement: it is only a statement of fact. The letters run as follow:—

ONE cannot but feel much sympathy for Ministers, on the one hand pressed by the advocates of scientific and technical education, and on the other nervous at the prospect of not securing the gifts of the munificent but somewhat *exigeant* art donor. But the question is so vitally important from the point of view of science that I feel sure no excuse is necessary if I urge most strenuously that an irrevocable step be not taken without full and careful consideration; and, further, that a definite scheme for providing for the science collections and Science School be formulated before what many of us believe to be a most unwise interpolation of an art gallery, on land which when bought was universally believed to have been acquired for scientific ends, is finally decided on.

At the present moment it is impossible to say under which thimble the scientific pea is housed, and it was no doubt due to this that the discussion which the deputation had with the Chancellor of the Exchequer and Lord President of the Council on Tuesday last was to some extent abortive.

The Chancellor of the Exchequer, in reply to myself on March 18, said:—

"It would be possible to make adequate provision for chemical and physical laboratories on the land between the Imperial Institute Road and the Technical Institute. This site adjoins the east galleries, and it is in these galleries, together with the west and southern galleries, and a proposed cross gallery joining the east and west galleries, that the science collections may ultimately be housed."

But by April 15 the impracticability of the scheme of putting part of the Science School at the south end of the eastern gallery seems to have been discovered. For on that day Mr. W. H. Smith, in reply to Mr. Mundella, propounded another scheme for the Science School, while leaving the collections to be housed in the east and west and cross galleries. He said:—

"A portion of these vacant lands" (facing the Imperial Institute) "can be utilized for the extension of the College of Science and for future growth of the science collections. Additions to the College of Science must in any case take the form of a separate building, divided from the present building by Exhibition Road; and, as access to the lands mentioned above from Exhibition Road will be secured by means of a corridor, the interposition of the Gallery of British Art need have no more serious effect than to increase by some 60 yards (which will be under cover) the distance between the two portions of the Science College."

By the former plan a portion of the Science School would no doubt have been in immediate contact with the splendid picture galleries in which the science objects were to be housed; but it would be far removed from the other part of the school—the Exhibition Road thus becoming a school of peripatetic philosophy. By the latter scheme the two parts of the school would be brought somewhat closer together—less of Exhibition Road and more of covered corridor—but then both portions would be entirely separated from the science collections—two roads to cross, and a walk of half a mile, or thereabouts, to the further part.

When receiving the deputation on Tuesday last, a third scheme was suggested, if not distinctly enunciated, by the Chancellor of the Exchequer, that the Science School extension and the Science Museum should be built on the other side of the plot given to the Art Gallery, but both on the ground recently acquired facing the Imperial Institute.

The two earlier projects having as it were blown themselves up, it is only necessary for me to deal with the last.

It has been argued that the recent Committee on the science collections, of which I was a member, only asked for 90,000 square feet of exhibiting space, and that more than that area can be obtained on the vacant ground opposite the Imperial Institute. But it must be remembered that, as stated by our Committee, this space did not provide for offices, workshops, &c.—a considerable item; that it did not in any way provide for the extension of the Science School; and that it was made some time before an immense impetus was given to technical education by the Technical Instruction Acts and the grants under the Customs and Excise Act of last year.

Now, the vacant ground recently acquired—omitting the strip part of which has already been sold, and the remainder of which is going to be sold for private dwelling houses—is about one-third of the land devoted to the Natural History Museum, and almost exactly of the same area as that already covered by the Natural History Museum buildings, which are shortly to be enlarged.

Is it unreasonable for the scientific man to urge that this vacant land is not too much to provide for the whole range of sciences other than those accommodated in the Natural History Museum; for a proper Museum of Machinery and Inventions; for a large extension of the Science School; and possibly for the collections from the Jermyn Street Museum? Surely there can be but one answer to this question.

Why—and we have never yet obtained an answer to this inquiry—will not the munificent donor be satisfied with another site? Why are the existing physical laboratory and scientific class-rooms to be removed, to allow an art gallery to be interposed between portions of the school?

Even if it be maintained that the ground south of the Imperial Institute Road will provide for the immediate wants of the Science School and collections, is it too much to ask that we should look a little ahead, and not now initiate another hugger-mugger arrangement of the collections and schools at South Kensington, which all will lament in a few years?

10 Bramham Gardens, S.W., HENRY E. ROSCOE.
May 15.

NOTWITHSTANDING that the recent deputation to the President of the Council and the Chancellor of the Exchequer was headed by Sir William Thomson—the man of science whom we in this country regard as first among all others, both on account of his individual achievements and on account of his occupying the representative position of President of the Royal Society—not one single word was said by Mr. Goschen in explanation or justification of the course which he has adopted; we therefore venture, with all respect, to assert that the Royal Society has just cause to complain when one of its Fellows—for Mr. Goschen is one of us—thus treats representations urged by its President.

Where the science collections are to be lodged, where the extensions of the Science Schools are to be placed, are in themselves all-important questions; but a still graver issue remains—whether a weight of opinion of the magnitude represented by the memorial recently published in your columns is to be entirely set aside because an anonymous donor has offered £80,000 *plus* a collection of pictures, valued at another £75,000. That a Government which has at its head a Prime Minister whose interest in science is so marked, should thus disregard the opinion offered by so representative a body of men, is one of those things which even an Englishmen can scarcely understand: in no other country in Europe would such action be possible.

We cannot help thinking that a mistake has been made in calling public attention too exclusively to the housing of the science collections—the extension of the Science Schools appears to be a far more important matter. Attention has often been called of late to what is going on abroad, especially in Germany; to the unremitting attention that is being given to scientific instruction, and to the effect that is being produced on manufacturing industries of all kinds by the high development of science and of the application of every kind of scientific requirement. Unfortunately, in this country such matters have not yet entered into the domain of practical politics. But in the opinion of many among us there cannot be a question that almost superhuman efforts are necessary if this country is to regain the position which it has given away to foreigners by its neglect to apply the highest developments of chemical and physical science to industry.

The accommodation at present afforded by the Royal College of Science laboratories is not only inadequate, but beneath contempt in comparison with that to be found in Continental cities, such even as Göttingen and Zürich, for example; and those of us who have some knowledge of modern requirements know full well that every inch of space on the Imperial Institute Road side of the block of land on which stands the Natural History Museum will before long be required for the purposes of the Royal College of Science. The intrusion of an art gallery into this space would have a most disastrous effect by irretrievably preventing the proper and natural expansion of the Royal College of Science laboratories. This expansion must necessarily be rapid, for science is developing throughout the civilized world at a compound interest rate, and the grants recently made by the Chancellor of the Exchequer in aid of technical instruction must lead even this country to fully appreciate the value of experimental studies, and to insist on proper laboratory accommodation being provided.

Surely the munificent donor will accept for his gallery some other site equally good for art, and not insist on striking a blow at science by taking a piece of land already set apart for laboratories.

HENRY E. ARMSTRONG,
Secretary of the Chemical Society.
W. E. AYRTON,
President of the Physical Society.

It seems probable that, as the discussion goes on, some side light will be thrown upon the motives of those who have the "munificent donor" in hand. Although we have not room for the whole of a letter from Mr. Marshall of Edinburgh, the general drift of it may be stated as follows:—

Mr. Marshall's main point is that, according to the statements made by Sir Frederick Leighton in his speech at the Royal Academy banquet, the new gallery is to be used as "a worthy home for the permanent display of the works of contemporary native artists"—which "means, being interpreted," says Mr. Marshall, "a worthy home for the works of Royal Academicians and their friends." The object for which Sir James Linton, Sir J.C. Robinson, Mr. Orrocks, and others have been contending is that there should be adequate "recognition throughout its whole range, both as regards masters and mediums of work, of the artistic triumphs of the masters of our English school." What these gentlemen have urged and incontestably proved is that while foreign art, and especially early Italian art, is fully if not excessively represented in our National Gallery, and while a few of our great native artists (notably Turner and Constable), and many of our small ones, are represented far beyond what is necessary or even desirable, our native water-colour art is practically not recognized at all, and many of the very greatest of our masters in oil, who were (most of them) masters in water-colour also—Cox, Miller, Barret, De Wint, Crome, Cotman, Stark, Vincent, and others—are either conspicuous by their absence, or miserably represented as regards quality or quantity or both. If the public wants a "permanent display of the works of contemporary native artists," and if a generous millionaire is willing to provide "a worthy home" for such productions, the thing can be done. "But I object," continues Mr. Marshall, "to our astute Academicians, with the accomplished President at their head, calmly stepping in and absorbing a movement at the very moment of its success, diverting it from its legitimate purpose, and, after having stoned the prophets of English art while they lived, now endeavouring to steal the stones that others have quarried and hewn for the martyrs' monument in order to erect with them another comfortable mansion for themselves." Mr. Marshall is of opinion that "provincials" have opportunities more than enough of seeing contemporary art. Their wish now is to have a chance of studying fine specimens, authoritatively selected, of the acknowledged masters of our English school.

The possible existence of such special motives as those here suggested among the persons who are attempting to get a grant of land for the carrying out of their so-called national objects should form an additional inducement to men of science to redouble their efforts.

NOTES.

THE general programme for the Cardiff meeting of the British Association has now been arranged. The first meeting will be held on Wednesday, August 19, at 8 p.m., when Sir Frederick Abel, K.C.B., will resign the chair, and Dr. William Huggins, President-elect, will assume the presidency and deliver an address. On Thursday evening, August 20, at 8 p.m., there will be a *soirée;* on Friday evening, August 21, at 8.30 p.m., a discourse on "Some Difficulties in the Life of Aquatic Insects," by Prof. L. C. Miall; on Monday evening, August 24, at 8.30 p.m., a discourse by Prof. T. E. Thorpe, F.R.S.; and on Tuesday evening, August 25, at 8 p.m., a *soirée.* On Wednesday, August 26, the concluding general meeting will be held at 2.30 p.m.

THE arrangements for the International Congress of Hygiene and Demography are nearly complete, and the programme, corrected up to May 1, has been issued in the form of a pamphlet. It has been definitely fixed that the opening meeting, at which the Prince of Wales is to preside, shall be held on Monday, August 10, at 3.30. The sections (of which there are ten) will meet on the four following days from 10 to 2. The six medical and scientific sections will meet in the rooms of the Royal and other learned Societies at Burlington House. The University of London will give the use of its large theatre to the section for the hygiene of infancy and childhood, and two examination halls to the sections for architecture and engineering. The division of demography will meet in the Theatre of the School of Mines, Jermyn Street. Much attention is being given to the necessary social preparations; and there is already a long list of proposed entertainments and excursions.

A GENERAL meeting of the Federated Institution of Mining Engineers will be held in London on Thursday, the 28th inst., at 12 noon, and on Friday, the 29th, at 10 a.m., in the rooms of the Institution of Civil Engineers, 25 Great George Street, Westminster. Various works will be visited on the 29th inst.

THE Committee of the Cardiff Naturalists' Society have put on foot a petition in favour of Mr. Pease's "Bill to Amend the Wild Birds' Protection Act, 1880." They are appealing to other scientific societies to join with them in order to make the petition as effective as possible.

AT Mowbray, a suburb of Cape Town, Mr. Cecil Rhodes has bought for £16,000 land on which, it is understood, the proposed University is to be built.

THE death of Prof. Carl Wilhelm von Nägeli, the eminent botanist, is announced. He died at Munich, on the 10th inst., in the 74th year of his age, and will be buried at Zürich, in accordance with a wish expressed before his death. Prof. von Nägeli was a Foreign Member of the Royal Society. We hope on a future occasion to give some account of his scientific labours.

THE Australian papers announce the death of Dr. Richard Schomburgk, brother of the late Sir Robert Schomburgk, and for many years Director of the Botanic Gardens at Adelaide, South Australia. Dr. Schomburgk was associated with his brother in the Boundary Demarcation Commission of British Guiana in 1840, and, some years later, settled with another brother in South Australia as a farmer and wine-grower. On the death of Mr. Francis, in 1866, he was offered, and accepted, the post of Director of the Adelaide Botanic Gardens, which he held with much distinction until his death. He was an enthusiastic horticulturist, rather than a botanist—that is to say, as an author; and his services in connection with the establishment he directed were very highly appreciated, as the sketches of his career testify. Indeed, so long ago as 1883, a large number of his admirers subscribed the funds to procure his portrait for the Museum of Economic Botany, founded by himself. His literary work commenced, we believe, with his "Reisen in Britisch Guiana in den Jahren 1840–1844," the third volume of which is devoted to a "Versuch einer Flora und Fauna von Britisch Guiana," in which Schomburgk had the assistance of several other botanists. This work has not yet been superseded, though its usefulness is unfortunately much limited by the publication of a large number of new names without descriptions. In 1876, Dr. Schomburgk supplemented this work by his "Botanical Reminiscences of British Guiana." But his most valuable literary work relates to the botany, to the agricultural and horticultural capabilities of his adopted country, and especially to the Botanic Garden, of which he was to a great extent the creator. His name will long be remembered in connection with this establishment, which is, it is asserted, the "most complete paradise of flowers in the southern hemisphere."

ACCORDING to the Calcutta correspondent of the *Times*, the Miranzai Expedition, under Sir W. Lockhart, has obtained much valuable geographical information about places which, although within a few miles of the frontier, have been hitherto unvisited by Europeans. The surveys effected by the Kuram field force during the Afghan war have been carried on to the Kurmana Valley.

A RUSSIAN scientific expedition, under the command of Captain Bartshevsky, has left Samarcand for the exploration of Southern Bokhara, the Pamir district, and Kafiristan.

ON Saturday, May 30, at the Royal Institution, Prof. A. H. Church, Professor of Chemistry in the Royal Academy of Arts, will begin a course of three lectures on the scientific study of decorative colour.

THE Rev. H. N. Hutchinson has undertaken to write for Messrs. Swan Sonnenschein and Co.'s "Introductory Science Text-books" a manual of physical geology. A second edition of Dr. Hatch's "Petrology" in the same series, reviewed in our columns last week, has already appeared.

MESSRS. WHITTAKER & Co. have in preparation a "Library of Popular Science." Among the works to be included in it are "Astronomy," by G. F. Chambers; "Light," by Sir H. Trueman Wood; "Chemistry," by T. Bolas; "Mineralogy," by Dr. F. H. Hatch; "Electricity and Magnetism," by S. Bottone; "Geology," by A. J. Jukes-Brown; "Botany," by G. Massee.

MR. J. ALLEN BROWN has expounded in the *West Middlesex Standard* an excellent scheme—now printed separately—for a technical institute and museum for the Ealing Parliamentary division of Middlesex. This division comprises Ealing, Acton, and Chiswick, and Mr. Brown's proposal is that a technical institute and museum should be established in whatever position may be most convenient for these localities. An essential part of his plan is that the instruction shall be imparted by specially qualified teachers and lecturers, and that their duties shall be "migratory or peripatetic," so that classes may be conducted or lectures given in any part of the division, and on any of the subjects contemplated under the Technical Instruction Acts. We commend Mr. Brown's scheme to the careful attention of the Middlesex County Council, which will soon have to decide as to the distribution of the funds placed at its disposal for technical instruction. There can be no doubt that the proposed institutions would be of immense advantage to the three districts, for Mr. Brown has a very enlightened conception of the true nature of technical instruction. What he wishes is that the young workman shall acquire "a knowledge of the scientific or artistic principles which are applicable to his trade or industry," and that by the development of his powers of observation and insight into the laws which govern all things "he may afterwards be enabled to effect improvements and excel to a greater extent than heretofore in the work he desires to accomplish."

THE Göttingen Society of Sciences has recently offered the following prize in physics for September 30, 1893:—From the researches of W. Röntgen and A. Kundt on variation of the optical properties of quartz in the electric field, there appears to be a close connection between the electro-optic phenomena and the elastic deformations which that piëzo-electric substance shows under the action of electrostatic forces. An extension of these inquiries to a series of piëzo-electric crystals with various properties of symmetry seems highly desirable. The investigation should also be directed to determining whether the electro-optic phenomena in piëzo-electric crystals are caused exclusively by the deformations occurring in the electric field or, besides, by a direct action of the electrostatic forces on the light-motion. Prize, £25.

THE German Society for the Encouragement of Industry offers the following (among other) prizes: (1) How far is the chemical composition of steel, and especially the amount of carbon present, a measure of the usefulness of cutting tools? Prize, a silver medal and £300; date, November 15, 1891. (2) A silver medal and £150 for the best chemical and physical investigation of the most common iron paints. Date, November 15, 1894. (3) A gold medal and £150 for the best work on the magnetism of iron. This should comprise a critical comparison of previous observations; also personal observations on steel and wrought iron bars of the most various chemical composition possible, examination being made both of the strength of temporary magnetization with absolutely measured and varying magnetizing force, and the strength of permanent magnetism and its durability with regard to temperature-changes and vibrations. Date, November 15, 1893. (4) Investigation of the trustworthiness of the usual methods of determining the carbon in iron. Prize, a silver medal and £150; date, November 15, 1892).

THE extraordinary collection of mummies, papyri, and other objects of antiquarian interest recovered last February at Deir-el-Bahari is now safely housed in the Ghizeh Museum. According to the Cairo correspondent of the *Times*, all the objects are in good condition, although some anxiety was caused by the protracted journey by boats from Luxor. The correspondent says that the mummies mostly belong to the 21st Dynasty, and, though styled Priests of Ammon, are supposed to be the corpses of generals and other official dignitaries who bore ecclesiastical besides other titles. The 163 mummies and the 75 papyri are not yet unrolled, and it is difficult to form an estimate of their archæological value, as many of the sarcophagi bear different names on the outer and inner casings, whilst others have the names usually inscribed on the outer casings intentionally effaced. M. Grébaut thinks that, owing to this circumstance and the magnitude of the collection, some time will be required before any important communications can be made to the scientific world.

A SERIES of experiments has been lately made by Herr Rubner (*Archiv für Hygiene*), with regard to the familiar fact that not only dry high temperatures are more easily borne than moist, but dry cold causes much less discomfort than moist cold. Dogs, fasting or fed, being observed in an air-calorimeter, it appeared that, in all cases, moist air increased the loss of heat by conduction and radiation. For every variation of the air-moisture 1 per cent., heat was parted with to the extent of 0·32 per cent. In a previous investigation, Herr Rubner demonstrated the lessened yield of water by evaporation from animals where the air-moisture is increased, involving lessened loss of heat. Here, then, are two antagonistic influences. He is disposed to regard the increased radiation and conduction in moist air as the primary action, and the diminished evaporation as secondary. The colder feeling of moist cold than dry is readily explained by the increased heat radiation. In moist heat, with the sense of oppression it brings, this factor passes rather into the background. The degree of temperature, and some other influences, of complex nature, also affect the amount of radiation.

THE Meteorological Council have issued a publication containing the hourly means obtained from the self-recording instruments at their observatories for the year 1887. This work constitutes a new departure in the use made of the records of the self-registering instruments, and one which we think will be of much practical use to meteorologists. The publication of the hourly observations *in extenso*, at the request of a number of scientific men, began with the year 1874, and was continued until 1880, in a lithographed form, and the daily means were added in 1879; from the year 1881 to 1886 they were issued in a printed form. The Council, after careful consideration, have now come to the conclusion that it is preferable, for a time at least, to publish mean values only; hitherto no hourly means had been published by the Office, but in the present work these have been grouped into five-day and other periods, in a convenient form for discussion, and the necessity for dealing with an excessive number of values has thereby been obviated, while many useful tables not included in the old series have been added. It is proposed to calculate the means similarly for earlier years, while the original records will be carefully preserved, and will be available, should they be needed, for any special research.

THE Annual Report of the Director of the Royal Alfred Observatory, Mauritius, for the year 1889 shows that the island has again enjoyed immunity from storms; the greatest hourly velocity of the wind was 31 miles. The almost total absence of tropical cyclones in the South Indian Ocean during the year is considered by Dr. Meldrum as another confirmation of the law that these cyclones are fewest in number and least intense in the years of least solar activity. The mean temperature was 0°·7 below the average for the last fifteen years, and below the average in every month except July and October. The maximum shade temperature was 93°·1 on March 27, and the minimum 52°·4 on June 18. The rainfall was 8·56 inches above the average; the greatest fall in one day was 3·88 inches on March 11, although this amount was much exceeded in other parts of the island. On January 1, a waterspout burst on the Pouce Mountain; Port Louis was flooded, and some persons were drowned. The collection of observations made at sea is actively carried on; 324 log-books were received, and the observations duly tabulated. The Report also contains observations made at the Seychelles and Rodriguez.

IN a paper recently published in the *Meteorologische Zeitschrift*, Prof. Hellmann, of Berlin, shows, from observations taken at different British, Continental, and American stations, at which barographs are used, that there exists a close coincidence in the daily range of the monthly extremes and in that of the hourly values of the barometer. He finds that the hours of occurrence of the highest and the lowest readings of the barometer during a month agree almost completely with the times in which the normal daily range has its maxima and minima, both curves being so similar in shape that it may be possible to judge of the general character of the daily range of the barometer from knowing only the hours at which the monthly extremes mostly occur. Hence, as the lowest readings of the barometer are accompanied by cloudy and stormy weather, during which the effect of the solar radiation upon the surface of the earth and the heating of the lower strata of the atmosphere are quite insignificant, Prof. Hellmann concludes that Prof. Hann and others are right in assuming that the normal daily range of the barometer is chiefly an effect of the *absorption of the solar rays in the upper strata* of our atmosphere. Prof. Hann has applied the harmonic analysis to the numbers furnished by Prof. Hellmann, and, by combining several stations in a group, has found the coefficients of the periodic formula to be practically the same as those for the normal daily range. We should, however, like to see a further confirmation with respect to the coincidence of the lowest readings and the diurnal minima, since the lowest readings occur so frequently during the passage of a severe storm, which can scarcely be said to have any agreement with the ordinary diurnal fluctuation.

The first paper in the last volume of Transactions of the Seismological Society of Japan is by Mr. Bertin, and describes the double oscillograph and its employment for the study of rolling and pitching. It traces curves automatically, showing the motion produced in a floating body by waves. The second paper is on the "Seiches" of lakes, by Dr. F. A. Forel, of Geneva, and discusses those variations in the level of the water of lakes with the investigation of which the author's name has been associated for some years past. Prof. John Milne describes the remarkable instrument invented by him for measuring and recording the oscillatory movements of railway trains. Mr. Mason contributes a paper, accompanied by carefully compiled tables, demonstrating the importance of elaborating some uniform system of timekeeping for the purposes of seismological observations. Prof. C. G. Knott, in his paper on earthquake frequency, explodes two of the time-honoured delusions of the popular mind in regard to earthquakes, viz. that they are more frequent during the night than the day, and that their periodicity is connected with lunar culminations. Mr. Otsuka gives an interesting account of the great earthquake that visited Kumamoto in July 1888; and Mr. Pereira contributes a carefully compiled record of all the earthquakes noted by him in Yokohama from March 1885 to December 1889. Mr. W. E. Forster writes on earthquakes of non-volcanic origin, caused, it is suggested, by the displacement of masses of land beneath the ocean. The volume concludes with various reports and papers by Prof. Milne, such as diagrams of earthquakes recorded in Tokio, a report on earthquake observations made in Japan during the year 1889, and an essay on the connection between earthquakes and electric and magnetic phenomena, which is full of matter of an interesting and suggestive kind.

According to the *Colonies and India*, Mr. Alexander McPhee, a West Australian bushman, who has steadily been earning fame lately by his explorations in the central regions of Australia, started inland from Roebourne in July last on another tour of discovery, taking back at the same time an albino aboriginal whom he found and brought to Melbourne a couple of years since. News has been received from which it appears that Mr. McPhee, with the albino, Jun Gun, and a "black fellow" named Timothy, went along the coast some 250 miles to a station called Yinadong, when the party turned inland in an easterly direction. After travelling about 350 miles, Mr. McPhee came upon another albino, a boy of fourteen years, whom he describes as the most extraordinary specimen of humanity he ever saw. One old man in this camp told Mr. McPhee that when he was a boy he heard of a party of whites and horses dying a long way inland. The old fellow could give no particulars about this party, but Mr. McPhee feels certain, owing to his acquaintance with the habits and customs of the blacks, and being thoroughly conversant with their dialect, that a party of white men perished about forty years ago somewhere in the interior. He heard of Warburton's party, and saw a native who told him that he guided them to water. He also heard of two parties of whites who had lately been in the desert, but turned back. From his turning point to the coast of La Grange Bay, Mr. McPhee reckons he was about 250 miles in a south-east direction from that bay. He found the natives very friendly, and on no occasion was it necessary to keep a watch. The country is described as very poor. The only birds observed during the journey were an odd crow and a few sparrows about the water; not a track of a kangaroo or emu was seen.

Some satisfactory statements as to the growth of collegiate education are made in the last official report on public instruction in the North-West Provinces and Oudh. Of individual colleges, Agra, at which the numbers in 1885 had fallen as low as 45, has increased within the last two years from 97 to 175, or by over 80 per cent., and the percentage of increase last year was in no case less than 20. The number of matriculated students, indeed, is rising so rapidly that the existing accommodation is said to be barely adequate; it will, the Government resolution says, become a question of urgent importance whether the increasing number of students should be provided for by additions to the staff and buildings at the colleges now in existence, or by the creation of new colleges, or by the strengthening of the college classes at high schools and adding to their number. "Government," it is added, "will necessarily be guided to a great extent by the nature and direction of the local demand, as indicated by the willingness of the residents of the principal towns to contribute to the increased burden of expenditure." On its present basis, at all events, the higher education of India has received a fair share of Government support. But if it is satisfactory, says the *Pioneer*, to find that collegiate education in its present form is making decided progress, and that it is becoming possible to throw the cost of the advance on private shoulders, it is a distinct disappointment that not a word is said, as not a step has been taken, in those new directions of educational activity where other provinces have not only started, but made appreciable progress. There may be two opinions as to the extent to which, or the means by which, it is possible to introduce technical education, but there can be no question that some movement is desirable. It may be hoped that the omission is due, not so much to a failure to estimate the importance of the subject, as to a desire to give it fuller treatment on a future occasion.

The amount of apparent flattening of the vault of the heavens Prof. Reimann has lately attempted to measure by noting the point which seems to bisect an arc extending from the zenith to the horizon. From 83 observations at Hirschberg he found that this point was 21°·47 ± 0·08 above the horizon. This indicates a ratio of the vertical axis to the horizontal of 1 : 3·66. This apparent flattening has an annual period, and is dependent on cloud. The highest position of the bisecting point was assigned in autumn (21°·98), the lowest in spring (20°·42). The vault seems flatter the more the cloud. It seems least flat with a misty horizon; and the flattening seems less by night than by day. Curiously, several other persons whom Prof. Reimann got to make the same determination all gave higher values for the angle.

The settlement of a purely philological question (that, namely, as to the position of the French accent), by a physical method, has been recently attempted by Dr. Pringsheim, of Berlin (*Naturw. Rdsch.*) The instrument used was König and Scott's phonautograph, into which a number of Frenchmen were required to speak; the measurement of the record being afterwards made by means of a tuning-fork curve running parallel with it. This instrument renders possible a determination of the duration, pitch, and intensity of each syllable, and Dr. Pringsheim discusses its indications. As a preliminary result, he finds that two-syllable words have the vowels pronounced with equal length and strength. Noteworthy differences appear in the curve of a word according as it occurs in the middle or at the end of a sentence. In the latter case, there is added to the characteristic word curve, a terminal curve with declining pitch and strength, which is nearly the same for different words, and corresponds to the sinking of the voice before a pause. The vowels and consonants show characteristic curves; and notably long wave-lengths occur with *n*, *l*, *b*, and *d*. The duration of syllables varies between 0·1 and 0·5 second; and between the syllables of a word there are often pauses of 0·03 to 0·2 second. The shortest syllable *é* in *été*, with rather slow pronunciation, consisted of 22 vibrations; yet the ear is capable of not only hearing the tone, but of detecting fine shades and differences in the mode of pronunciation. Further experiments in this direction, with an improved apparatus, are contemplated.

THE *Perak Government Gazette* states that a portion of an ethnographical collection formed by Signor G. B. Cerruti, in the island of Nias, has been recently acquired by the Government of Perak for the museum. Pulo Nias is one of a chain of islands bordering the south-western coast of Sumatra. The population is said to be numerous and of one race, though divided into many tribes under independent chiefs. Head-hunting is as common with them as it used to be in Borneo, and most of the houses have skulls hung up in them. Their weapons consist of iron-headed spears, mostly barbed, knives of two patterns, somewhat resembling the Kadubong Achi, with shields of two distinct types. No bows and arrows or blow-pipes seem to be known, nor are throwing sticks applied to their spears; boats also are not used by them, though rafts are sometimes made to cross the rivers on. The ironwork of their weapons is fashioned by themselves, and the upright double cylinder bellows is used to supply wind to their forges—the same in every respect as those used by the Semangs of Upper Perak, and the far away Malagasy. Helmets of black ijoh fibre are worn, somewhat similar to the cocoa-nut fibre ones of the Sandwich Islanders. Woven body armour is in use, in the shape of thick coats made of what appears to be the fibre of *Hibiscus tiliacens*. Buffalo hide armour is also said to be used, but is not represented in this collection. Attached to the sheaths of some of the knives are four or five animals' teeth, such as tigers, rhinoceros, &c., also a small carved wooden idol, and one or more bamboo boxes containing stones. In those examined there were twelve pebbles in each box. These stones are supposed to have been taken from the spot on which a man had been slain. All these charms are tied up into a bundle with red cloth, and bound with string on the upper front part of the sheath of the knife.

A COMPREHENSIVE study of the influence of forests on the daily variation of air-temperature has been recently made by Prof. Müttrich (*Met. Zeits.*), the data being from stations in Germany and Austria. *Inter alia*, this influence is greater in May to September or October than in the other months. In pine and fir woods it rises gradually from January to a maximum in August or September, then falls more quickly to a minimum in December; but in beech woods a minimum occurs in April, then there is quick rise, till the maximum is reached in July. The daily variation itself is greatest in May or June, both in forest and open country. The influence of the forest is to lower the maxima and raise the minima, and the former influence is in most months greater than the latter; in December and January, and occasionally in neighbouring months, it is less. The influence on the maxima in summer is greatest in beech woods, less in pine, and least in fir. The absolute value of the influence in woods of a given kind of tree is affected by the degree of density of the wood, being higher the denser the wood. The character of the climate (oceanic or continental) also affects the results. From daily observations in forest and open country, every two hours in the second half of June, it appears that, soon after 5 a.m. and 8 p.m., the air-temperature in the wood was equal to that in the open; that the maximum was about 0°·9 lower in the wood, and the minimum 0°·6 higher; that in May to September the difference sometimes reached 2°·7; that the maximum in the wood occurred about half an hour later, and the minimum a quarter of an hour earlier, than in the open; and that the daily mean air-temperature was about ⅛° less in the wood.

THE *Revue des Sciences Naturelles de l'Ouest* gives an account of the life of Mathurin Rouault, one of the pioneers in the geology of Brittany. Rouault was born in 1813, of a very poor family. At the age of ten, while engaged as a shepherd, he became interested in "stones" and "rocks," and began to make a collection. By the death of a relative he obtained possession of a small hairdresser's shop, where he worked on Saturdays and Sundays, spending the rest of his time in hunting for rocks. Although Geoffroy Saint-Hilaire visited his collection of specimens, and was much interested in them, nothing would have been done for the poor young geologist—who lived upon something like five centimes a day—if it had not been for General de Tournemine, who, stationed with the garrison in Rennes, had been attracted by him. It is said that one day he went into the shop, and, seeing an antique pistol which Rouault had bought for a few centimes to kill himself with, the general remarked, "That is just the pistol I am after: I want it for my collection." And without waiting for an answer he took the pistol, and gave the young man 100 francs. M. de Tournemine went still further. He revised a memoir which the illiterate geologist had written. This was read in the Academy of Sciences, and met with so much success that the author became well known. The town of Rennes gave him 800 francs a year to help him to live in Paris, and afterwards he was appointed Director of the Geological Museum of Rennes. But he was dismissed on account of quarrels with some unintelligent bureaucrat, and died in 1881. Before his time only five or six fossils were known in Brittany: afterwards they numbered 500 or 600. He spent two years or more in making up *Trinucleus Pongerardi* out of over 2000 fragments.

AN important paper upon the atomic weight and position in the periodic system of the rare element lanthanum is contributed by Dr. Brauner, of Prague, late of the Owens College, Manchester, to the current number of the *Berichte*. In his recent work upon the reduction of oxides by metallic magnesium Prof. Winkler advanced the view that lanthanum is a tetravalent element of atomic weight 180, instead of, as has hitherto been accepted, a trivalent element belonging to the boron vertical group of the periodic system, with an atomic weight of 138·5. If lanthanum were indeed tetravalent with atomic weight 180, it would probably be the missing element between ytterbium and tantalum on the one hand, and cerium and thorium on the other. Further, Prof. Winkler expresses the opinion that the old values of Rammelsberg, Zschiesche, and Erk, for the equivalent of lanthanum, are correct. These experimenters obtained the round number 45 for the equivalent, and this number multiplied by 4 gives Prof. Winkler's suggested atomic weight 180. If, however, multiplied by 3, the atomic weight 135 is arrived at, and Prof. Winkler argues that even if the element were trivalent its atomic weight would not be 138·5 but 135. Against these views Dr. Brauner brings forward the following experimental facts. In the first place, Hillebrand (working under Bunsen) found the specific heat of Bunsen's pure lanthanum to be 0·04475. No impeachment has ever been brought against this result, and Dr. Brauner sees no reason why it should not be accepted. Making use of Dulong and Petit's generalization and multiplying this number by 138, a normal atomic heat of 6·18 is arrived at, whereas if multiplied by 180 the abnormal value 8·07 is obtained. Again, an element of atomic weight 180 should possess a density of 8·2, whereas that of lanthanum is only 6·48, a specific gravity corresponding to an atomic weight of 138. Considering therefore the position of lanthanum in the trivalent boron vertical group assured, Dr. Brauner brings forward a redetermination of its atomic weight of his own in order to decide between 138·5 and 135. His experimental method consisted in converting known weights of the oxide into sulphate. The material employed was obtained by a lengthy process of fractionation with ammonium nitrate, the oxide eventually obtained containing the most positive of the cerite earths (lanthanum oxide) and showing no traces in the spectrum of any others. His value thus obtained is 138·2, a number closely agreeing with those of Cleve and Bettendorff. The earlier and lower values of Rammelsberg and others are shown to be probably due to the presence of yttria, which was not detected by these observers, inasmuch as the work of Thalén and Bunsen upon the spectrum of yttrium had not then been published. Hence lanthanum of atomic weight 138·2 retains the place in the trivalent group of the periodic system marked out for it by its well-known basic properties.

THE additions to the Zoological Society's Gardens during the past week include a Striped Hyæna (*Hyæna striata* ♀) from India, presented by Mr. B. T. Ffinch, C.M.Z.S.; two Hairy-rumped Agoutis (*Dasyprocta prymnolopha*) from British Guiana, presented by Mr. H. Barrington; two Brent Geese (*Bernicla brenta*), a Pintail (*Dafila acuta* ♂), two Wigeons (*Mareca penelope* ♂ ♀), a Common Sheldrake (*Tadorna vulpanser* ♀), two Golden Tench (*Tinca vulgaris*, var.), nine Golden Carp (*Carassius auratus*), British, presented by Mrs. Atkinson; eight European Tree Frogs (*Hyla arborea*) from the South of France, presented by Mr. Clifford D. Fothergill; a Crested Porcupine (*Hystrix cristata*) from India, a Tibetan Crossoptilon (*Crossoptilon tibetanum* ♀) from Western China, deposited; two Swinhoe's Pheasants (*Euplocamus swinhoii* ♂ ♀) from Formosa, two Japanese Pheasants (*Phasianus versicolor* ♀ ♀) from Japan, two Amherst's Pheasants (*Thaumalea amherstiæ* ♀ ♀) from Szechuen, China, a Black-necked Stilt Plover (*Himantopus nigricollis*), a Cayenne Lapwing (*Vanellus cayennensis*) from South America, purchased; a Wild Swine (*Sus scrofa* ♀) from Persia, received in exchange; two Indian Desert Foxes (*Canis leucopus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE PHOTOGRAPHY OF FAINT NEBULÆ.—In the Journal of the British Astronomical Association for February, Dr. Max Wolf, of Heidelberg Observatory, contributes a note on a nebula surrounding ζ Orionis, the third star in the belt, which he has discovered on photographs taken with a 4-inch portrait lens. Some reproductions submitted to the Association show a large amount of nebulosity south-west of ζ, also nebulous ground around ζ, and a nebulous star north of ζ. Dr. Wolf's note is important, inasmuch as it indicates that the 4-inch portrait lens used at Heidelberg gives results which compare favourably with those obtained at Harvard with a much larger instrument, viz. the Bache equatorial of 8 inches aperture and 44 inches focal length. With regard to the use of portrait lenses for celestial photography, Dr. Wolf makes a few succinct remarks. In photographing the stars, the intensity of the image depends only upon the area of the lens employed, and an instrument of 20 inches diameter therefore requires 25 times less exposure than one 4 inches in diameter having the same focal length, in order to obtain the same number of stellar images. But it is a different thing with comets, nebulæ, and the like—bodies having a finite area. The intensity of the image at the focus then varies as the fraction $\left(\frac{d}{f}\right)^2$, where d is the diameter of the object-glass, and f its focal length. If, therefore, the intensity of the light received with an aperture of 20 inches and focal length of 100 inches be expressed by 0·04, that of a portrait lens of 4 inches aperture and 12 inches focus is 0·11. This shows that in order to photograph the same faint nebula, the instrument of 20 inches aperture requires an exposure about three times as long as the 4-inch portrait lens.

Another paper having the same purport is contributed by Dr. Holden to vol. iii. No. 14, of the Publications of the Astronomical Society of the Pacific, from which it appears that from 80 to 100 minutes' exposure with the 33-inch Lick telescope will give about the same number of stars as 205 minutes' exposure with Mr. Roberts's 20-inch reflector. When, however, the amount of nebulosity depicted is considered, the advantage is considerably in favour of the short-focus reflector, a comparison of the results obtained with the two instruments indicating that 15 minutes' exposure with the reflector is about as effective in showing the nebulosity of Orion as 60 minutes' with the refractor.

VARIATIONS IN LATITUDE.—Prof. H. G. van de Sande Bakhuyzen extends our knowledge of this subject in a paper contained in the March number of the *Monthly Notices* of the Royal Astronomical Society. The conclusions deduced from the investigation of observations of Polaris made between 1851 and 1882, and the interesting researches of Mr. Thackeray (Memoirs R.A.S., vol. xlix. p. 239), may be summed up as follows:—(1) The monthly discordances in the zenith distances of Polaris are, for the greater part, not caused by a real variation of latitude, but chiefly by an effect of temperature. (2) It is not possible to explain those discordances by an error in the indications of the exterior thermometer, or by an influence depending only on the exterior temperature. (3) The discordances can be explained, for the greater part, by a cause depending on the difference of the exterior and interior temperatures. (4) Probably that cause is a refraction in the observing-room, and its effects are sensibly proportional to those differences of temperature. (5) The discordances corrected for that refraction are about the same for both culminations, and can be explained by a real variation of latitude.

An investigation of the mean North Polar distances of Polaris in both culminations observed at Greenwich between 1883 and 1889 leads to the conclusions: (1) that it is probable that the observations of Polaris at Greenwich confirm the variations of latitude observed elsewhere in 1884–1885 and 1889–1890; (2) that there is a very strong probability that the variations in these years had an exceptional character, and do not agree with the annual variations, deduced from the observations of Polaris at Greenwich during the period 1851–1882.

RE-DISCOVERY OF WOLF'S COMET (1884 III.).—*Astronomische Nachrichten*, No. 3033, contains the information that Wolf's periodical comet was observed on its return by Prof. Barnard, of Lick Observatory, on May 3·9792 G.M.T. The following ephemeris is from one given in *Edinburgh Circular* No. 15, by Prof. Berberich. The brightness of the comet at re-discovery has been taken as unity.

Ephemeris for Berlin Midnight.

1891.	Right Ascension.			Declination.		Brightness.
	h.	m.	s.	°	'	
May 23	23	16	31	+ 17	47·1	1·44
,, 27		25	42	18	42·8	1·54
,, 31		35	0	19	37·7	1·65
June 4		44	26	20	31·4	1·77
,, 8		53	59	21	23·9	1·90
,, 12	0	3	40	22	14·9	2·03
,, 16		13	30	23	4·0	2·18
,, 20		23	26	23	50·8	2·33
,, 24		33	32	24	35·2	2·50
,, 28		43	45	25	16·9	2·68
July 2		54	5	25	55·4	2·88
,, 6	1	4	34	26	30·3	3·08

The comet will pass perihelion on September 3·3199 Berlin mean time. It is near α Pegasi at the present time, and may therefore be seen just before sunrise. The motion is towards Andromeda.

THE PARIS OBSERVATORY.[1]

THIS report opens with the address delivered by the Director, Admiral Mouchez, before the Council of the Observatory on February 24 last; the following is a brief summary of the most important points touched upon.

After referring to the successful completion of the building for the large *equatorial coudé*, in which the instrument is now being erected, and to the formation of a special service for spectroscopy, over which M. Deslandres has been put in charge, he enters on the question of the formation of a branch establishment outside Paris. "The demands of modern science," he says, "the extreme smallness of the quantities on which the astronomy of position depends, and the extreme faintness of the objects that physical astronomy studies in order to penetrate more and more deeply into the knowledge of the universe, admit indeed of new processes of observation of such delicacy that they are altogether incompatible with the turmoil and disturbances of all kinds in a populous city. The instruments with large optical power lose nearly all their superiority, because they magnify the defects of an impure and disturbed atmosphere at least as rapidly as the images of the stars."

This is by no means the first time that this question of a branch establishment has been raised, but it looks very much as if it might now be taken up seriously. It seems that a proposal has been made to extend the railroad from Sceaux-Limours in the interior of Paris to Médicis and Cluny, where it would join the metropolitan; if this project was carried out, trains would run as close to the Observatory as 150 metres, thus affording the assistants at the Observatory an interesting amusement in calculating the distances of these trains by the vibrations set up in the various instruments.

A committee of inquiry, presided over by M. Chauchat, has been formed to inquire into the situation, and the unanimous opinion of all the astronomers questioned on the subject was that "the Observatory would be almost lost if this project was carried out according to the present conditions."

Of the other arguments put forward by Admiral Mouchez in favour of the branch establishment, the following may be mentioned. The lighting of the surrounding streets by means of the electric light. This, as he says, would obliterate all stars above the 12th magnitude, and perhaps even above the 11½th, to say nothing of the minor planets, nebulæ, and some comets. And with regard to photographing the heavens with moderate exposures, it would become nearly impossible owing to the fogging of the plates before the images are formed, the gas from the street lamps even now producing this effect on the sensitized plates. Referring to the opening and enlarging of the Rue Cassini, he points out, that at no remote date, houses will be constructed from 20 metres to 25 metres in height at a distance of 100 metres, and just in the direction of the meridian line of the instruments; these, besides completely blotting out from view many of the circumpolar stars at their lower culmination, will render the observation of those that remain difficult on account of the smoke from the chimneys.

Following Admiral Mouchez's address are the reports, from each of the heads of the various departments, of the work done during the past year. With the meridian circle no less than 14,374 stars have been observed, exclusive of the 432 observations of the planets made with the same instrument. Observations which were commenced in the month of April with the *equatorial coudé*, have been regularly pursued, and at present the results have been highly satisfactory. Not only "do we believe that we have settled in every detail the most precise rules for the application of the new method, but also we have obtained the constant of aberration with an exactness which surpasses all researches made up to the present time."

The three equatorials have been used by M. Bigourdan, Mdlle. Klumpke, and M. Boinot respectively, and with them observations have been made of comets, double stars, nebulæ, eclipses of Jupiter's satellites, occultations, planets, and double stars.

M. Paul Henry, who is chief of the photographic department, has been busily engaged among other things in making large *clichés* of different regions of the sky, several of which were prepared at the request of foreign astronomers.

The most important addition to the Observatory for the year was the special service for stellar spectroscopy, which, as we have mentioned before, is superintended by M. Deslandres. This branch, when in full working order, should be of the utmost value to science, and the results obtained will be looked forward to with interest. With regard to this branch Admiral Mouchez has given an extract from M. Deslandres' report on the installation of the apparatus and the results obtained.

After a short description of the meteorological work carried on, together with the various other reports usually inserted in this pamphlet, Admiral Mouchez concludes with a brief reference to the Observatory School at Montsouris, of which also he is Director. This school was organized under the patronage of the Bureau of Longitudes, in order to supply a want long felt in France of a school for practical astronomy, where "marine officers, explorers, professors of science, and others could come and accustom themselves to make observations." Since the year 1877 the Observatory has been freely opened to anyone, the only conditions being that those who go should have sufficient scientific knowledge to understand what is taught, and that their work should be regular. To give an idea of the range of the subjects that form the syllabus of instruction we cannot do better than condense the methods of organization as given in the report.

With regard to astronomy, both theoretical and practical lectures are given twice or three times a week. M. Boitel delivers a course on electricity and magnetism which extends over four months, during which time he conducts the officers over all the large electrical manufactories in Paris. Lectures on meteorology are delivered by M. Moureaux, who concludes them with practical instructions for the determination of the magnetic elements. M. Thoulet treats of ocean geography in a course that is of interest and use to sailors. The regulation of the compass, so important to-day on account of our iron ships, forms the subject of a number of lectures by M. Caspari, while photography is studied for two months under the superintendence of M. Guenaire.

From this syllabus it will be seen that a good, practical, and sound course is open to all those who wish to take advantage of it, and in the list of explorers who have figured in the principal missions during the last fifteen years the majority will be found to have served at any rate a short period at the Observatory School.

In concluding his remarks, Admiral Mouchez, after referring to the school that was started in 1879, and which was suppressed some years after for reasons of economy, points out the necessity of giving every encouragement to the one that is doing such good work at Montsouris.

W. J. L.

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[1] "Rapport Annuel sur l'État de l'Observatoire de Paris pour l'Année 1890." Présenté au Conseil par M. le Contre-Amiral Mouchez. (Paris: Gauthier-Villars et Fils, 1891.)

NOTE ON THE PHYSIOLOGICAL ACTION OF CARBON-MONOXIDE OF NICKEL [$Ni(CO)_4$].[1]

BY the kindness of Mr. Ludwig Mond, we have had the opportunity of examining the physiological action of carbon-monoxide of nickel, a substance of unique chemical composition, represented by the formula $Ni(CO)_4$. The general results of our investigation are as follows:—

(1) $Ni(CO)_4$ is a powerful poison when injected subcutaneously into a rabbit weighing 1·5 kilo. even with a dose of 1/30th c.cm.

(2) The vapour of $Ni(CO)_4$ in air, even to the extent of 0·5 per cent., is dangerous.

(3) The symptoms are those of a respiratory poison, and are similar to those caused by carbonic oxide.

(4) The spectrum of the blood of an animal poisoned by $Ni(CO)_4$ is that of carbonic-oxide-hæmoglobin, and it is not reduced by sulphide of ammonium.

(5) When the substance is injected subcutaneously it is probably in part dissociated in the tissues, as there is evidence of the existence of nickel in those tissues, but the nickel also finds its way into the blood, and is found there.

(6) The substance produces a remarkably prolonged fall of temperature even when given in small quantities. In several instances, with lethal doses, the fall was from 2° to 12° C. This may be accounted for by the hæmoglobin being prevented to a large extent from supplying the tissues with oxygen. Nico, as we may, for convenience, call this substance, makes it possible to give graduated doses of carbonic oxide, and thus reduce temperature

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[1] By John G. McKendrick, M.D., F.R.S., and William Snodgrass, M.A. M.B., Physiological Laboratory, University of Glasgow.

by directly interfering with the respiratory exchanges occurring in the tissues. The objections to its use as an antipyretic are that, owing to its poisonous properties, it is difficult to inject it subcutaneously in sufficiently small doses, while it is not easy to obtain a solution in any menstruum in which decomposition will not take place. If a convenient method of dissolving it could be devised, $Ni(CO)_4$ might become a valuable antipyretic, the *modus operandi* of which is intelligible.[1]

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, April 2.—Mr. W. Crookes, F.R.S., Vice-President, in the chair.—The following papers were read:—Citraconfluorescein, by J. T. Hewitt. Lunge and Burckhardt have shown that maleic anhydride is capable of yielding a fluorescein; the author has obtained the corresponding fluorescein from citraconic anhydride, by the action of resorcinol in the presence of sulphuric acid. Citraconfluorescein is easily soluble in alcohol and glacial acetic acid, fairly soluble in water; the aqueous solution is yellowish-brown and shows a green fluorescence.—Ethylic thiacetacetate, by Dr. C. T. Sprague. Hübner obtained ethylic thiacetacetate by the action of sulphur monochloride, S_2Cl_2, on ethylic acetacetate. It has since been obtained by Delisle by the action of sulphur dichloride, SCl_2, on ethylic acetacetate; by Schönbrodt by the action of sulphur on the copper derivative of ethylic acetacetate; and by Michaelis and Phillips from thionyl chloride and ethylic acetacetate. Buchka proposed the formula $S(CH.Ac.CO_2Et)_2$, but an alternative formula, $S(O.C:CH.CO_2Et)_2$, was suggested by Delisle. The author describes the preparation of the substance and the products of its interaction with hydrazines; and shows that it behaves towards phenylhydrazine in the same manner as ethylic acetacetate. The results are in accordance with the formula proposed by Buchka.—The function of chlorine in acid chlorides as exemplified by sulphuryl chloride, by H. E. Armstrong. A number of experiments carried out during recent years in the author's laboratory show that sulphuryl chloride, SO_2Cl_2, acts on benzenoid compounds simply as a chlorinating agent. Sulphuryl chloride is easily formed by the direct union of sulphur dioxide and chlorine in the presence of a catalyst, such as camphor, charcoal, or acetic acid; it is a highly mobile liquid of low boiling-point, and is acted on with extreme slowness by water and alkaline solutions. It is an inert substance possessed of properties by no means such as are usually regarded as characteristic of acid chlorides. The chlorine is apparently but loosely held, and is easily withdrawn by a compound having an affinity for chlorine, such as naphthalene. On warming a mixture of this hydrocarbon and sulphuryl chloride, SO_2 is evolved and naphthalene tetrachloride is produced. The author doubts whether the chlorine in acid chlorides is possessed of special activity, and is inclined to the view that the activity of acid chlorides is conditioned by the oxygen rather than the chlorine; this view being supported by the observations of Wagner and Saytzeff, and the later ones of Pawlow (*Annalen*, clxxxviii. 104). The author also discusses the action of SO_3HCl, and the analogous compound $SO_3.EtCl$, and points out that pyrosulphuryl chloride, $S_2O_5Cl_2$, behaves much as if it consisted of SO_3 and SO_2Cl_2.—The action of nitric acid on the ligno-celluloses, by C. F. Cross and E. J. Bevan. Dilute nitric acid attacks the ligno-celluloses when heated with them at 60°, with the formation of a bright yellow derivative of lignone and nitrous acid On further interaction, large quantities of nitrous oxide, N_2O, are evolved, together with carbonic anhydride and a small proportion of nitric oxide. A sensible quantity of hydrogen cyanide is also produced, the proportion being increased by increase of temperature. The observations point to the entrance of the NOH residue into the lignone molecule; its interaction with nitrous acid being finally the displacement of H_2 by O. The reaction is probably general for compounds containing the NOH residue, and the authors suggest that attention be paid to the gaseous products of the interaction of nitric acid and carbon compounds, as calculated to elucidate their mechanism.—The Chairman, Mr. Crookes, gave a short verbal account of observations on the volatilization of metals *in vacuo* under the influence of an electric discharge.

April 16.—Prof. A. Crum Brown, F.R.S., President, in the chair.—The following papers were read:—Studies on the formation of substitution derivatives, by H. Gordon. The following experiments were undertaken with the object of throwing further light on the laws which govern substitution in the case of benzenoid compounds. *The action of bromine on diorthonitrophenol.*—When bromine is added to an acetic acid solution of diorthonitrophenol at ordinary temperatures, the normal product, namely parabromdiorthonitrophenol, is obtained. However, if the mixture be heated at 100° for a short time, a mixture is obtained consisting of parabromdiorthonitrophenol and orthobromorthoparadinitrophenol. And if the heating be prolonged, and small quantities of bromine added, the mixed product is converted into orthobromorthoparadinitrophenol. Parabromdiorthonitrophenol is therefore completely converted by the action of heat, and bromine into the isomeric orthobromorthoparadinitrophenol. The same isomeric change takes place under the influence of nitric acid. An acetic acid solution of parabromdiorthonitrophenol, when heated with a few drops of nitric acid at 100°, is completely converted into the isomeric orthobromorthoparadinitrophenol. Experiments were then undertaken with the corresponding chloro-compounds. It was found that chlorine had no action on diorthonitrophenol when dissolved in acetic acid at 100°, even in the presence of iodine. Chlorination, however, takes place when chlorine is passed into a solution of diorthonitrophenol in antimony pentachloride at 105°, and only the normal product parachlorodinitrophenol is formed. *Action of bromine on parachlordiorthonitrophenol.*—Experiments to ascertain whether isomeric change could be effected by the action of bromine on parachlordinitrophenol only gave negative results, the normal product, parachlororthobromorthonitrophenol, being obtained in every case. The author considers that in the case of the chlorine compound isomeric change does not take place, because the chlorine is more firmly held than bromine. *Action of sulphuric acid on orthoparadichlorphenolorthosulphonic acid.*—The combined action of heat and sulphuric acid on orthoparadichlorphenolsulphonic acid gave no indication of any isomeric change taking place, although the reaction was investigated under a great variety of conditions of temperature, &c. The corresponding dibromphenol also gave negative results, but as several secondary reactions set in, such as the formation of tribromphenol, this reaction was not further investigated. *The chlorination and bromination of phenol.*—Phenol when chlorinated in the ordinary manner yields a mixture of para- and ortho- chlorphenol. The author finds that a similar mixture is obtained when SO_2Cl_2 is employed as the chlorinating agent. He has also investigated the action of bromine on phenol under the conditions described by Hübner and Brenken (*Ber.* vi. 170), and finds that the product is practically pure parabromphenol. *The sulphonation of the nitrophenols.*—Orthonitrophenol and paranitrophenol are, according to Armstrong, both readily acted upon by SO_3HCl; the former yields the well-known sulpho-acid; the latter yields a product which is decomposed by water, and was supposed by Armstrong to be the sulphate, and this the author finds to be the case. The author considers that the initial action in both cases is the same; but that the sulphate formed from orthonitrophenol at once undergoes isomeric change, whereas the sulphate from paranitrophenol is more stable. The author did not succeed in obtaining any sulpho-acid by heating the sulphate from the paranitrophenol at 100°. But he obtained a fair yield of sulpho-acid by heating the nitrophenol with two molecular proportions of SO_3HCl at 100°. Hence, there is little doubt that the paranitrophenolsulphonic acid is formed by the sulphonation of the sulphate. Metanitrophenol resembles the para-compound in being converted into sulphate, but not into the sulpho-acid even by the action of heat.—Compounds of dextrose with the oxides of nickel, chromium, and iron, by A. C. Chapman. The nickel compound is obtained by adding a solution of nickel hydrate in ammonia to a solution of dextrose in 90 per cent. alcohol. It is a green amorphous substance, insoluble in water and alcohol, of the composition $C_6H_{12}O_6.2NiO + 3H_2O$. The chromium compound, which appears to have the composition represented by the formula $C_6H_{12}O_6.Cr_2O_3 + 4H_2O$, is prepared by dissolving an excess of dextrose in an aqueous solution of chromic chloride, and pouring this solution into cold strong ammonia. The precipitated hydrate partly dissolves on standing, and on pouring the purple solution so obtained into 90 per cent. alcohol, the chromium dextrosate is obtained as a lilac-coloured precipitate. The iron compound, $2C_6H_{12}O_6.3Fe_2O_3$

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[1] This investigation was carried on during last winter. It appears that M. Hanriot made a communication of the subject to the Société Chimique on February 27. He found the substance to be more poisonous than CO, and that the blood gave the spectrum of carbon-monoxide-hæmoglobin.

$+ 3H_2O$, is obtained by adding a slight excess of ammonia to a solution of ferric chloride containing an excess of dextrose; on standing, a deep red solution is obtained, which when poured into 90 per cent. alcohol yields the dextrosate of iron as a red flocculent precipitate. The moist compound dissolves easily in water to a red solution, is decomposed on boiling, but is not decomposed by ammonia, potassic ferrocyanide, or potassic thiocyanate. The dry compound is insoluble in water.—A rapid method of estimating nitrates in potable waters, by Dr. G. Harrow. The method depends on the reduction of nitric to nitrous acid by means of zinc dust and hydrochloric acid, in a very dilute solution, in the presence of α-naphthylamine and sulphuric acid; the estimation is made by comparing the depths of the pink azo-coloration developed in the solution with that arising on similar treatment of standard nitrate solutions. When nitrites are present, the amount is estimated in a similar manner prior to the addition of zinc dust, and due allowance is subsequently made. A number of comparisons with the Crum method show that very satisfactory results are obtainable. —The "gravivolumeter," an instrument by means of which the observed volume of a single gas gives directly the weight of the gas: a preliminary note, by F. R. Japp, F.R.S. The author describes a method of constructing a gas apparatus, by means of which, with an ordinary graduation in cubic centimetres, any required single gas may, without observation of temperature or pressure and without calculation, be measured under such conditions that each cubic centimetre represents a milligram of the gas. The author describes the apparatus in detail and the method of using it, and he anticipates that it will, at least, give results sufficiently accurate for technical purposes.—Mr. de Mosenthal exhibited one of Lipmann's coloured photographic negatives.—The action of acetic acid on phenylthiocarbimide, by J. C. Cain and Dr. J. B. Cohen, Owens College. The authors show that the product of the action of pure glacial acetic acid on phenylthiocarbimide is not diacetanilide, as stated by Hofmann; but that two compounds are formed—namely, diphenylurea and acetanilide. At low temperatures diphenylurea is mainly formed, at higher temperatures acetanilide. The reactions may be expressed by the following equations:—

$$2C_6H_5NCS + 6C_2H_4O_2 = (C_6H_5NH)_2CO + 3(C_2H_3O)_2{\cdot}O + 2H_2S + CO_2$$

and

$$(C_6H_5NH)_2CO + 2C_2H_4O_2 = 2C_6H_5NH_2 + (C_2H_3O)_2O + CO_2.$$

—The action of aluminium chloride on benzenoid acid chlorides, by R. E. Hughes, Jesus College, Oxford. The author has examined the action of aluminium chloride on cinnamic and hydrocinnamic chlorides, in the expectation that pentamethylene derivatives might result. The experiments, however, afforded negative results. The chloride was either dissolved in or mixed with light petroleum, and aluminium chloride then added; action set in at 80–90° in the case of cinnamic, and at 50° and more briskly in the case of hydrocinnamic, chloride. The chief product in both cases was an ill-characterized substance, which has not been examined. The author also describes the following compounds: hydrocinnamic chloride, hydrocinnamide, and hydrocinnamanilide. It is noted that benzoic and cinnamic acids may be readily separated by treating the mixture with phosphorus pentachloride and distilling the product under reduced pressure; the portion passing over below 95° under 10 mm. contains the benzoic chloride.

Paris.

Academy of Sciences, May 11.—M. Duchartre in the chair.—Essay on graphical dynamics, with reference to the periods of motion of hydraulic motors, by M. H. Léauté.—On the lowering of the surface of water in a horizontal cylindrical vessel, by M. Haton de la Goupillière.—On the boundaries of the littoral zones, by M. Léon Vaillant.—Observations made at Marseilles Observatory of the asteroid (308) discovered on March 31, by M. Borrelly. The observations for position extend from April 6 to April 30.—Elements of the orbit of Borrelly's new asteroid (308), by M. Fabry.—Provisionary elements of Borrelly's asteroid deduced from observations made at Marseilles Observatory on March 30, April 8, 18, and 26, by M. Esmiol.—Solar observations made at the Royal Observatory of the Roman College during the first quarter of 1891, by M. Tacchini. —On the movement of the moon's perigee, by M. Perchot. —On limited permutations, by M. C. A. Laisant.—On a class of complex numbers, by M. Markoff.—On a registering manometer applicable to pieces of ordnance, by M. P. Vieille.—An "elastic" theory of plasticity and fragility of solid bodies, by M. Marcel Brillouin.—On the wave-surface in crystals, by M. C. Raveau.—On the determination of the dielectric constant of glass by means of very rapid electrical oscillations, by M. R. Blondlot. The author has made some experiments which support Prof. J. J. Thomson's conclusion that the specific inductive capacity of glass is very nearly equal to the square of the index of refraction, and has least value when a slow frequency of vibration is employed.—On a new compound of oxygen and tungsten, by M. E. Péchard.—Thermic study of bibasic organic acids with simple functions, by M. G. Massol.—Remark on the preceding note, by M. Berthelot.—On the fourth primary amyl-alcohol, by M. L. Tissier.—On the diffusion of fresh water into sea-water, by M. J. Thoulet.—On the theory of M. Tschermak's felspars, by M. K. de Kroustchoff. A description is given of a new triclinic felspar having a composition very similar to oligoclase, but distinguished from it by several peculiarities.—On the genital organs of some Tristomidæ, by M. G. Saint-Remy.—On the constitution of the sexual nuclei of plants, by M. Léon Guignard.—On the groups of the genus *Clusia*, by M. J. Vesque. —The parasitic fungus of the larva of the cockchafer, by MM. Prillieux and Delacroix.—The parasite of the cockchafer, by M. Le Moult.—On a remarkable inversion of strata termed *pli couché* observed near Toulon, by MM. Marcel Bertrand and Zurcher.—On the permanence of the orogenic effort in the Pyrenees during the geological periods, by M. Roussel.

Amsterdam.

Royal Academy of Sciences, April 24.—Prof. van de Sande Bakhuyzen in the chair.—Mr. van der Waals dealt with a formula for electrolytic dissociation, which may be deduced from his theory of a mixture. This formula accounts for the facts (1) that ions may combine with absorption of heat; (2) that the parameter of electrolytic dissociation varies with the medium which holds the salt-molecules in solution; (3) that the quantity of free ions may diminish when the quantity of salt-molecules increases.

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THURSDAY, MAY 28, 1891.

MEDICAL RESEARCH AT EDINBURGH.

Laboratory Reports of the Royal College of Physicians of Edinburgh. Vol. III. (Edinburgh and London: Young J. Pentland, 1891.)

NOW that for three years the laboratory of the Edinburgh Royal College of Physicians has shown steady advancement in every direction—in the number of workers engaged within it, in the volume of work accomplished, and more especially in the quality of that work—Dr. Grainger Stewart and his Council must congratulate themselves heartily that they were undeterred by any misgivings from entering upon a venture which has been so abundantly successful, and which has added so much to the renown of the College. It must be a source of very sincere satisfaction to them, and especially to Dr. Batty Tuke, the prime mover in its organization, to know that no laboratory in the Kingdom can show for the same space of time a record of so much good work in so many directions, of which a large part would never have been undertaken had this laboratory not been established.

In many respects the present volume exhibits marked improvement as compared with its predecessors. While composed of more than a dozen papers, these only represent but a portion of the investigations that have been completed, and all of them contain matter of permanent interest; others whose interest is of a more temporary nature have, I think wisely, been excluded. The value of the volume is further enhanced greatly by the fact that the majority of the reports appear here for the first time. Among these may be mentioned Dr. Helme's important contribution to the physiology of the uterus; Dr. Gulland's heterodox papers upon leucocytes and adenoid tissue; Noël Paton and Balfour's very full studies upon the composition and physiological action of the human bile; Woodhead and Cartwright Wood's observations upon bacterio-therapeutics; and a short but important communication by Cartwright Wood and Maxwell Ross on the influence which the process of inflammation exerts upon the course of infectious disease.

Taking these in order, Dr. Helme's paper is of especial value, not only clinically, from the light it throws on the mode by which certain drugs act upon the uterus, and from the consequent indications it affords as to the conditions under which they may wisely be administered, but also as a contribution to the physiology of non-striped voluntary muscle. Employing the *überlebende* organ—the organ removed with all precautions immediately after the death of the animal (a sheep)—and continuing the circulation through it artificially, Dr. Helme has been able to study its slow rhythmic contractions apart from the influence of the central nervous system and of the changes in the blood supply. From a physiological point of view, his most important observation is perhaps that which brings out the striking difference existing between striped and non-striped muscle as regards the relationship between contraction and blood supply. Whereas a striped muscle during contraction becomes hyperæmic, the uterus, the largest mass of unstriped muscle in the body, becomes during contraction relatively anæmic.

It is impossible to pass Dr. Gulland's articles upon the nature and varieties of leucocytes and upon the development of adenoid tissue without bestowing on them not a little adverse criticism, and this, while appreciating fully the long months spent in laborious preparation and examination of tissues, and in studying the literature of the subject, of which they bear ample witness. That Dr. Gulland bases his conclusions upon the view that the leucocytes are symbiotic, and shows at the outset that he totally misconceives the nature of symbiosis, is quite sufficient to render fuller criticism of his views unnecessary. Yet, that it may not be said that I misrepresent his views, it may be as well to quote his words upon this subject:—

> "There are still" (in the Metazoan) "many functions to be performed which can only be discharged by cells possessed of Protozoan characteristics. . . . To perform these functions it is necessary that a certain number of cells should continue to be practically Protozoa, and these cells are what we call 'leucocytes,' so that we may regard them *morphologically as representing those members of the primitive Metazoan colony which escaped differentiation, and have remained unaltered Protozoa through the whole series of Metazoa*" (the italics are mine).

Such inconsequent theorizing goes far to neutralize the minute and careful observations which Dr. Gulland has made into the histology of his subject.

That the formation of bile solids is more closely associated with the general metabolism than with the changes of digestion is the conclusion drawn by Dr. Noël Paton and Mr. Balfour, though somewhat unexpectedly they find that in fever, where the general metabolism is greatly increased and the digestive processes reduced, the amount of bile solids excreted is diminished. All studies of cases of biliary fistula in man are of value, and such full observations as those here described are rare. Of drugs they find calomel and salicylate of soda active in increasing the flow of bile. Whether they are right in looking upon the bile as an excretion, rather than as at the same time a secretion playing an essential part in digestion, is open to doubt. Even if with bile excluded from the intestine only 30 per cent. of the fats ingested pass out unused, that nevertheless is a proportion large enough to demand consideration, and to support the assumption that as a secretion, as well as an excretion, the bile is of definite importance. The ingenious method devised for the estimation of the bile pigments (p. 197) deserves a more extended trial.

At a time when Koch's endeavours to cure tuberculosis by means of injections of products of growth of the tubercle bacilli have brought the whole subject of bacterio-therapeutics prominently to the fore, the full discussion of this by Drs. Woodhead and Wood is very acceptable, based, as it is, upon their own important discovery that the invasion of the organism by the bacillus of anthrax may be prevented by injections of the sterilized fluid in which the *Bacillus pyocyaneus* has been grown. Space forbids that I should do more than indicate that those interested will here find a full account of our present knowledge of a subject which is occupying the energies of every leading bacteriologist.

Of allied interest is the communication by Dr. Wood and Mr. Ross. It has long been known that the advance of erysipelas can often be successfully combated by painting the skin immediately outside the erysipelatous area with some counter-irritant. The authors have studied the *rationale* of this treatment, and conclude that the irritant brings about the formation of a zone of inflammation, with dilatation of the vessels and diapedesis of the white corpuscles, which now, by destroying the micrococci, act as a barrier to the further progress of the disease. With the malignant pustule produced by the inoculation of the anthrax bacilli, similar counter-irritation was effectual in only three out of thirty cases—that is to say, with the more active virus the stimulus applied was not sufficient to produce an effectual barrier. J. GEORGE ADAMI.

THE CHEMICAL AND BACTERIOLOGICAL EXAMINATION OF POTABLE WATERS.

Examen Químico y Bacteriológico de las Aquas Potables. Por A. E. Salazar y C. Newman, con uno capitulo del Dr. Rafael Blanchard sobre "Los Animales Parásitos introducidos por el Aqua en el Organismo." (London: Burns and Oates, 1890.)

A PECULIAR interest attaches to this work at the present moment in consequence of the sad political events now going on in the country from which it has emanated; for, whilst almost each successive day brings news of the sacrifice of human life in one of the fiercest and most sanguinary civil contests of recent years, the object of this book is to show how the latest results of scientific research may be applied to combating on the same soil some of the ills which flesh is heir to. The publication of this treatise for Chilian students affords the strongest evidence of the rapidity with which scientific knowledge traverses the globe at the present day, and it must be a source of great satisfaction to all interested in the dissemination of the principles of hygiene that there should be a demand for a work of such an advanced character in a country so remote from what we are wont to regard as the centres of civilization.

The scope of this work is more comprehensive than that of perhaps any similar one in our own language; English treatises on water analysis being in general only short manuals giving instructions for the execution of analytical methods devised by their authors, who usually dismiss the rival methods of others with a few words, often not of a very complimentary kind. The pages under review, however, not only give an interesting account of the various methods employed by water-analysts, but subject their several claims to a fair and impartial criticism, whilst detailed information is supplied for carrying out those methods which the authors regard as, on the whole, the most serviceable. Again, a most exhaustive account is given of the bacteriological examination of water, including precise instructions for the cultivation of micro-organisms, the preparation of nutritive media, the sterilization of apparatus, the use of the microscope, and the performance of inoculation experiments on animals. But even this ample programme was inadequate for the ambition of the authors, who have associated with themselves a third colleague, who contributes a bulky appendix on "the animal parasites gaining access to the organism through water." The work is not only profusely illustrated with cuts, but contains also a number of original photographs representing both the microscopic and macroscopic appearance of some bacteria. Indeed, the bacteriological part is the real centre of gravity of the work. A decade will soon have elapsed since the bacteriological examination of waters began to attract much attention in consequence of the ingenious method o gelatin-plate cultivation devised by Koch. It was not, however, until some years later that the method yielded results of any practical importance, inasmuch as it was at first almost exclusively applied by bacteriologists whose previous information on questions of water-supply was of a somewhat limited order, whilst the value of the method for the solution of many hitherto unsolved problems connected with the hygiene of water is even now but imperfectly appreciated by chemists. When the method was first applied to the London water-supply, in the year 1885, it at once brought to light that in the process of sand-filtration, as practised on the large scale, a most astonishing proportion of the micro-organisms present in the unfiltered water were removed, whilst in the best of our deep-well waters the number of microbes found was so small that it seemed probable that the removal of these low forms of life in this process of natural filtration was really complete, and that the few actually found had very likely been imported into the wells from the surface. On the other hand, it was shown that the sand-filters did not wholly remove the organisms present in the unfiltered water, as, in the course of regular examinations carried on over a period of more than three years, a most unmistakable relationship between the number of microbes present in the unfiltered and filtered waters respectively was discernible. The scope of the bacteriological method of examination became very much narrowed when it was discovered that there are many micro-organisms which have the power of multiplying to an enormous extent in the purest waters, including distilled water itself, so that the number of microbes present in a given sample of water affords no indication *per se* of the purity or otherwise of the water. This disturbing element in the bacterioscopic examination of water is not sufficiently emphasized by the authors. But this extraordinary phenomenon of multiplication, although it invalidates the bacteriological process for the general purposes of water examination, does not at all interfere with its successful application to the investigation of the efficiency of filtration, either natural or artificial, provided that the filtered water is subjected to examination without delay after it has undergone the process of filtration.

It should be pointed out that there exists a very widespread misapprehension as to the ideal object of the bacteriological examination of waters, and the authors of this work fall into the same error to some extent also. It is very generally supposed that the main object of a bacteriological examination is to discover whether or not there are disease-producing organisms, *e.g.* those of typhoid, in the water. But this is a point really of very limited importance, and what should be kept in view in an examination of water is the endeavour to discover, not whether the water contains zymotic poison at the time of analysis, but firstly, whether it is exposed to influences which may at any time lead to the introduction of such zymotic poisons, *e.g.* through contamination with sewage;

and secondly, whether, if such organized poisons should gain access, there is any sufficient guarantee or not that they will be destroyed or removed before the water reaches the consumer. It is because the chemical analysis affords us at present a better clue than the bacteriological examination as to whether a water has received sewage or not that it is of more general applicability than the latter; but we must appeal to a bacteriological inquiry in order to ascertain whether, in the event of sewage gaining access to the water, there is a guarantee in the subsequent history of the water that the zymotic poisons, which may at any time accompany the sewage, would undergo removal. In short, the object of nearly all water examinations is obviously to ascertain whether the water may at any time be dangerous to health, and not, even if this could be with certainty determined, whether it contains a zymotic poison at the particular moment of examination. On the other hand, the fact that the microbe, which is now pretty generally accepted as the inducing cause of typhoid fever, has been on more than one occasion actually discovered in drinking-water which was under suspicion of producing an epidemic of that disease, affords most important evidence as to the manner of its distribution.

There is much need of a similar work to this in English, as each year an increasing number of younger medical men are coming forward for the degrees in Public Health which are now granted by several of our Universities, and to these a practical and critical treatise such as this would prove of great value. It is of great importance that such Public Health students should be impressed with a sense of the responsibility which attaches to the examination of waters for domestic purposes, and that most serious mischief may and often does result from such investigations being intrusted to incompetent persons. It is gratifying to see that the authors do not undertake to prescribe any of those artificial standards of purity for drinking-water which so frequently figure in books of this kind, and which are attended with the greatest danger, leading as they do the ignorant to believe that they can pronounce upon the fitness or otherwise of water for drinking purposes from the numbers which they have obtained in a few simple quantitative determinations. For it must never be forgotten that the sanitary examination of water is surrounded with such difficulties that it is only by bringing to bear on each particular case all the evidence that it is possible to obtain, and then interpreting this evidence by the light of an extended experience, that a sound judgment can be arrived at.

P. F. F.

OUR BOOK SHELF.

Botany: a Concise Manual for Students of Medicine and Science. By Alex. Johnstone, F.G.S. (Edinburgh and London: Young J. Pentland, 1891.)

DURING recent years many books on botany have been published, specially for the use of students preparing for examinations. In these a few types and phases of plant life have been described somewhat in detail. In the present case a much wider range has been taken, the result being an illustrated botanical note-book, condensed but not meagre. In the preface the author takes it for granted that every student nowadays attends lectures or demonstrations, and "therefore does not so much require a manual with diffuse explanations, but rather a kind of illustrated digest and general note-book, which will enable him to quickly arrange and make most effective use of the various facts and theories treated of by his teacher." A book on these lines Mr. Johnstone has been successful in producing. It consists of 260 pages and 226 illustrations. Some of the latter are the ones which seem by custom to be considered necessary for reproduction in every fresh botanical manual, while others appear to be new. The outline ones, such as those on p. 30, illustrating the branching of cells, give a much clearer idea than could be done by pages of letterpress. A short introductory chapter points out the position botany holds in science. The strictly botanical part of the work is treated of in four sections, viz. (1) morphology; (2) external morphology or organography; (3) physiology; and (4) taxonomy.

Under morphology, the structure, life-history, contents, and modifications of the cell as an individual are first treated of, after which the combinations of cells to form tissues are described, a special chapter being reserved for the consideration of systems of permanent tissues. The section on external morphology will be found very useful to beginners in systematic botany. It could be wished that the chapter on physiology, although containing much useful information in its 15 pages, had been more extended. The greater part of the remainder of the book is devoted to taxonomy, in which the leading characters of each class (arranged in ascending order) are given, followed by the names of some of the genera, which may be regarded as typical of their respective classes, and interspersed with illustrations. The orders of Angiosperms most frequently met with are represented by short diagnoses and floral diagrams. A useful glossary and index complete the book.

The arrangement throughout the book is good. The various headings, &c., printed in type differing according to their importance, have been very carefully set out, and give a good *résumé* of botany in a tabular form. As an illustrated note-book for a teacher, as well as a student, this work will be found of great use.

C. H. W.

Hand-book of the Ferns of Kaffraria. By T. R. Sim, Curator of the Botanic Garden, King Williamstown, South Africa. 66 pages, 63 plates. (Aberdeen: Taylor and Henderson, 1891.)

THIS little book contains popular descriptions and outline plates of the ferns of Kaffraria, with a chapter of definitions of the botanical terms used in describing ferns, and another giving directions how to cultivate them. The Cape, considering the general interest and remarkable individuality of its phanerogamic flora is very poor in ferns. Kaffraria yields only 68 species, about the same number as Great Britain. Amongst them are two tree ferns, a Cyathea and a Hemitelia, and several herbaceous species of a distinctly subtropical type, such as *Vittaria lineata* and *Marattia fraxinea.* Associated with these are several species with which we are familiar at home, such as *Aspidium aculeatum*, *Cystopteris fragilis*, and *Adiantum Capillus-Veneris.* No doubt by further exploration the list will be considerably increased. The author does not seem to have known anything about the Rev. R. Baur, a Moravian missionary who made large collections of ferns and other plants in Transkeian Kaffraria. The two new species which Mr. Sim claims to have added to the Cape flora cannot be admitted as novelties. *Blechnum remotum* is a variety of the American *B. hastatum*, which I do not think can stand as distinct specifically from the common Cape *B. australe.* The plant figured as *Lomaria lanceolata* on Plate 25 is no doubt *Lomaria inflexa* of Kunze, which was gathered long ago in the colony, by Gueinzius, and is beautifully figured by Kunze from specimens which he forwarded. By the aid of this book there can be no difficulty, even to an amateur, in recognizing any of the Kaffrarian species ; and perhaps at some future time Mr. Sim, who was trained at Kew, will extend his area so as to cover the whole colony, for which the total number of ferns known is between 130 and 140.

J. G. Baker.

Rider Papers on Euclid. Books I.–II. By Rupert Deakin, M.A. (London : Macmillan and Co., 1891.)

This little book consists of a series of graduated riders so arranged that the beginner may be able to thoroughly understand and grasp the principal propositions of the first two books of Euclid. One of the chief errors that the author endeavours to avoid is the great stress teachers lay on some of the propositions, which are treated as most important, while others are more or less overlooked.

The method he adopts is to treat each proposition first as a rider, and by giving the enunciation and drawing the figure, see if any of the class can show how it is proved. By this means the subject can be made interesting, as beginners can then look upon each rather as a puzzle than as a stiff piece of work.

The two books are divided into nine parts, each part consisting of six papers, and the riders in each paper, with the exception, of course, of the first, deal with all the preceding propositions. The student is advised in the first six papers only to draw the figures, in order to accustom himself to one of the chief difficulties which, as the author says, " experience shows me that all students feel more or less in solving riders."

At the end are printed the enunciations of the propositions of the two books, followed by several papers set at various examinations. Altogether, teachers will find this an admirable help for classes in which the subject is being treated for the first time.

Die Krystallanalyse oder die chemische Analyse durch Beobachtung der Krystallbildung mit Hülfe des Mikroskops mit theilweiser Benutzung seines Buches über Molekularphysik. Bearbeitet von Dr. O. Lehmann. (Leipzig : Engelmann, 1891.)

We have so recently noticed at length the splendid work of Dr. O. Lehmann on "Molecular Physics" (see Nature, vol. xlii. p. 1) that it is only necessary in this place to call attention to this pamphlet of 82 pages, illustrated by 73 woodcuts, in which the author gives the necessary directions for the work of micro-chemical analysis. The instruments used and methods employed are concisely stated, and all the essential details of the operations are supplied to the chemist in this little handbook. Dr. Lehmann claims, not unjustly, that the methods of micro-chemical analysis must play the same part in the laboratory of the organic chemist as spectral analysis does in the laboratory of the inorganic chemist.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

The University of London.

My friend, Mr. Thiselton Dyer, invites me, by his references to what I have written on this subject, to a discussion in your columns. I am very unwilling to accept the invitation, because I have already and often stated my views, and because I see by the length of Mr. Dyer's letter that I may be led into an interminable labyrinth of side-issues. The official report in which are published the minutes of the evidence given before the Royal Commission which sat on this subject in the year 1888, contains a more lengthy discussion of the subject by myself and others than it is possible to carry through in the columns of Nature ; and I could wish that for once those interested in a subject would rescue from proverbial oblivion the pages of careful statement entombed in a Blue-book. Since, however, my friend trails his coat, it would be doing violence to my old-established regard for him to refuse to tread on it—just a little.

The question raised by Mr. Dyer seems to be, why should not the examining board in Burlington Gardens undergo certain reforms and continue to be the so-called University of London? It has done good service to education, he says, and with the removal of more than half its members and their replacement by gentlemen who either really know or really care about University education it might do more. If it were, he suggests, to rise superior to all its most solemn obligations and falsify the pledges of its founders by undertaking to teach as well as to examine, it would really be as much of a "teaching University" as is either Oxford or Cambridge, and its non-collegiate supporters from all parts of Britain might enjoy the spectacle of the mother-college (University College) from which this examining board took birth, abandoning in favour of Burlington Gardens those traditions of scientific research which have made the College in some measure a realization of Fichte's ideal.

[Mr. Dyer seems to have forgotten the facts when he contends that such teaching as Fichte sketched in his plan for the University of Berlin, cannot be carried on in the same institution or by the same men who administer the teaching required by a University student at the commencement of his career. Fichte's plan was carried out in the University of Berlin, and has been followed by every other University in Germany. The very questions which we are now debating were debated in the early years of this century in Germany, and the Jesuits' plan of education by examination was rejected. University College was founded (except so far as it was a private enterprise) on the lines of a German University, and only required the *prestige* and independence conferred by the power of granting University degrees to enable it to fulfil in London Fichte's ideal. Its professors have never been (as Mr. Dyer well knows) mere instructors for examination purposes. The researches of Graham, Williamson, Sharpey, and of Michael Foster, Sanderson, Schäfer, Kennedy, and many others have been carried on in its laboratories. The proposal to detach such work from the London Colleges, and to associate it with the examining board in Burlington Gardens, on the ground that it is inconsistent with the teaching of University undergraduates, appears to me to involve an erroneous conception of what University education and University organization should be. This by way of parenthesis.]

The point which I wish to insist on is that, excepting the proposal to undertake higher professorial teaching, I have no objection whatever to the reforms of the examining body in Burlington Gardens advocated by Mr. Dyer.

What I desire (and I merely use the first person singular for the purpose of discussion, and not because I stand alone in my wishes, or undervalue the support of others) is that, without any interference with the Burlington Gardens board, the privilege of granting degrees should be conferred by the Crown upon a combined Senate consisting of the Professors of University and King's Colleges (the authority of the councils of the two Colleges being duly guarded).

The fact that Burlington Gardens are in London and that University and King's College are also in London, as well as the talk about a teaching University "in and for" London, have very little bearing upon the question as to whether it is or is not desirable to grant University privileges to the two Colleges. There is population enough and accommodation enough for a dozen Universities within the metropolitan area. As far as I am able to judge as to the principles which should guide the Crown in bestowing the privilege of incorporation as a University, the only questions to be asked are : "Does the body which asks for this privilege consist of learned men whose work will be facilitated by the granting to them of this ancient and honourable position? Do they give guarantees of material support, and of a public demand for their teaching, which will enable them to discharge the functions of a University with dignity and efficiency, now and hereafter? Will the concession to them of this privilege tend directly or indirectly or both to the public welfare?" I cannot imagine that anyone will undertake to give a negative response to these questions in reference to the combined Colleges, University and King's. Certain it is that during the acute discussion which has been carried on for the last four or five years, no one has ventured to do so. What has happened is simply this, that persons connected with Burlington Gardens have opposed the bestowal of University powers on the two Colleges, either for the reason that they consider the withdrawal of the Colleges from the sphere of the operations of the Burlington Gardens examining board a reflection upon that body, or because they are unwilling that a privilege should be conceded to Colleges, however well fitted to receive it, which their own local or provincial college is not yet important enough to claim. A further incident of the movement has been that the just demands of London medical students and their teachers for a University degree in medicine, as readily attainable by London students as are the medical degrees of Edinburgh, Glasgow, Dublin, Aberdeen, St. Andrews, Durham, and Cambridge, by the students of those places, have been formulated and generally approved.

Neither of these accompaniments of the request for University powers made by University and King's Colleges seems to me to touch the question as to whether it is right on grounds of public policy to accede to that request. Sir William Thomson, Sir George Stokes, and Mr. Weldon after an exhaustive inquiry were in favour of granting the privilege asked for. Three lawyers, namely Lord Selborne, Sir James Hannen, and Sir James Ball, were not persuaded. The commission composed of these six gentlemen agreed to ask the Burlington Gardens authorities to try to devise such alterations in their "University" as would satisfy the aspirations of University and King's Colleges. Burlington Gardens has absolutely and hopelessly failed in this attempt—as anyone conversant with the conditions of the problem could foresee must be the case. They have proposed a scheme which has not been accepted by the Colleges, and has also been rejected by their own provincial graduates. Why should more time be wasted about the attempt to put three pints into a quart bottle? Let the Burlington Gardens University continue to exercise its function of examining for schools and colleges which are not strong enough to examine for themselves, and let them continue so to do only until the colleges are fit to receive independent University powers; let the Senate reform itself if it can, and if the absurd dead-weight of graduates tied round its neck and called Convocation will permit it to do so. But do let us have in the meanwhile a genuine professorial University set on foot in London, not because it is London, but because University and King's Colleges are there, and respectfully petition Her Majesty to do for them what the monarch has done (not unwisely, it must be allowed) in past days for the Senatus Academicus of Edinburgh, of Aberdeen, of Leyden, of Berlin, Bonn, Leipzig, and other cities.

What the two Colleges ask for is a privilege—a special favour. To include other institutions as co-recipients of the privilege would destroy its character and its value. As Mr. Dyer points out, we do not want a federal University, such as are Cambridge and Oxford and the Victoria. We have seen enough of the friction and never-ending committees and schedules of such clumsily organized Universities. By limiting the charter to University and King's Colleges, a professorial University can be established in which the professors shall be—as in the Scotch and the German Universities—at once the teachers, the examiners, and the governing body. I cannot perceive what good can be attained by joining a series of rival teaching bodies together, calling them a University, and setting them to waste the lives of their lecturers in committees and boards and the drawing up of schedules. The only persons who gain by such wasteful arrangements are the busybodies and bureaucrats, who either acquire importance by their intermediation in the disputes of rival teachers, or gain a livelihood by pompously conducting the affairs of the committees and boards in which what is good and strong in each member is counteracted, whilst only what is feeble, worthless, and emasculate survives.

The professorial University formed by a union of King's and University would be of modest dimensions, and rightly so. It would in virtue of its charter be able to grow. This I regard as the most important feature in the proposal. Instead of hastily bringing together a variety of teaching bodies, we should leave it to the new University to assimilate them, make terms with them, in the course of time.

Though they are modest bodies compared with the Imperial centralizing institution, from the thraldom of which they seek to escape, yet King's and University Colleges can show figures stating the property and the number of students which they would bring to the new University, which are far larger than the corresponding figures for many other Universities both in the United Kingdom and abroad. Their buildings and land are worth half a million sterling. Their annual receipts exceed £30,000; their annual attendance of students is as great as that of the University of Oxford. This is an ample basis; with this start the new University would without any doubt be able to ensure a steady growth, increase of its property and of its teaching capacities, by a healthy and gradual development.

Mr. Dyer skilfully seeks to enlist support for the supremacy of Burlington Gardens by asking the following questions (to which he does not give the answers for obvious reasons): "Why should two out of many institutions be picked out for University honours? Why should Bedford College be left out? How can the Royal College of Science be ignored? Why ignore the City and Guilds Institute?"

These questions are excusable only when we admit that Mr. Dyer may for the nonce treat his defence of Burlington Gardens as a lawyer may treat a shady case entrusted to his advocacy in the courts.

The reason why the Crown should pick out the two Colleges for the University privilege is, firstly, that they and they alone have asked for it; secondly, that they and they alone possess the property, professoriate, status, and historical purpose which could warrant the privilege; and, lastly, that University powers are essentially a privilege fitted and intended to strengthen and build up the institution to which they are granted above others. Bedford College is cited by Mr. Dyer solely, I am afraid, with the purpose of rousing the jealousy of its members. They are, I hope and believe, too sensible to be led to imagine that their excellent institution is at all comparable in magnitude or importance to University and King's. As to the Royal College of Science, the answer is different. It is a Government institution under a special department founded and carried on with a special purpose. It grants its own certificates and fulfils its objects. I see no objection to its receiving the privilege of granting those certificates in the form of University degrees; but it could not be associated with University and King's Colleges to form one Senatus Academicus. To introduce it or the City and Guilds Institute into the new University would necessitate the formation of what I am persuaded would be a pernicious and futile organization—namely, a federal University. And, moreover, it appears that both the Royal College or Normal School of Science and the Guilds Institute were founded with public money and are carried on for other purposes than that of training University students, and that their managers do not seek the privilege of granting University degrees nor consider that their public utility would be increased by any such federation with the new University as Mr. Dyer suggests. There is plenty of room in London for non-University Colleges as well as for more than one University. The objectionable notion which Mr. Dyer and some others entertain is that these institutions can be made more useful by arbitrarily bringing them under the control of some central government—such as is now exercised by Burlington Gardens.

The fact appears to me to be that centralization in University matters is wasteful of time and energy, paralyzing and delusive. Two Colleges like University and King's can unite and settle their affairs together, and if granted such powers as other Universities possess they may in time take into their organization, partially or completely, other institutions, or arrange methods of co-operation with other institutions. Indeed they would, if incorporated as a University, be sure to do this, and to do it far more efficiently than could be the case were they abruptly associated with a variety of rival corporations, each with equal rights and equal voice, and left to compromise and to vote through endless committees, either as constituents of a reformed Burlington Gardens University or of a new piece of federal futility.

Mr. Dyer has wisely avoided the question of the demand for medical degrees. I confess that this is a very difficult problem on account of the attitude of the medical profession. If the medical profession is to be allowed to grant medical degrees, the present significance and a good deal of the value of the University privilege will be destroyed. It is, I believe, quite useless to attempt to *satisfy* the demands of the medical profession in this matter. The thing to be aimed at is to remedy an injustice; it is necessary to provide a degree as accessible as that of other Universities through whatever University or Universities may exist, hereafter, in London.

In my evidence to the Commissioners I made some suggestions on this matter. I am inclined to think that the following steps are necessary for a satisfactory solution of the problem: (*a*) the abolition of the medical faculties of University and

King's Colleges—excepting the Professorships of Anatomy, Physiology, Pathology, and Forensic Medicine—and the creation of independent clinical schools attached to the North London and Lincoln's Inn Hospitals; (*b*) the nomination of a medical professoriate for the new University by representatives of all the London medical schools, vacancies to be hereafter filled up on the recommendation of the Senate of the University; (*c*) the recognition, under conditions, by the new University, of the clinical teaching in each of the London hospitals, and the admission of students to its medical degrees on condition of having passed the prescribed examinations of the University and of having pursued not necessarily more than one-half of the entire curriculum under the professors of the University. The University might also be required to recognize (in exchange for a like concession) the examinations in certain subjects of the Conjoint Board as excusing candidates from like examination by the University.

This is undeniably a complex part of the subject. It would be simplest, and probably satisfactory in the end, to grant the power of giving medical degrees to the limited body (King's and University) and to leave it to make such arrangements as it might find expedient with the medical schools of London. The professional feeling of the medical faculties of University and King's Colleges would insure their making an equitable use of the privilege, such as their medical brethren would heartily approve.

E. RAY LANKESTER.

P.S.—There is one argument put forward by Mr. Dyer which I have omitted to notice in the foregoing, but should like to tread on. He quotes my opinion that the University may usefully examine scholars passing from the schools to the University as a test both of the work of the schoolboy and of the efficiency of the schoolmaster, and proceeds to maintain that in the same way an examining board may usefully check not only the work of University undergraduates, but of their teachers. This is advanced as an argument in favour of external or superior examining boards in University examinations as opposed to examinations conducted by University professors with associated external examiners. Mr. Dyer has, however, omitted to cite the reply which I had already given to his specious argument. It is this: the University is the highest term in the educational hierarchy. It may fittingly examine students who are about to pass from the school to continue their studies on a higher level, viz. its own. But who or what are the persons recognized as standing above the University professoriates? I do not know of any such body. It is precisely the arrogation of this position for the Senate of the University of London which renders it objectionable. There is necessarily a limit to the organization of authority in educational matters, and it is as absurd for the members of a central examining board to control the teaching of those who are *ex hypothesi* the most capable teachers in the country as it is for the Home Office to control the details of the work of the Senate of Burlington Gardens. Either University professors are worthy to occupy their positions or they are not—no higher branch of the educational profession exists. To coerce them by means of Senates composed of retired teachers and *dilettanti* educationists is clearly injurious: to set them to work to criticize and worry one another as "impartial examiners" is odious and a waste of their time. The only thing to do is to take such measures as are possible for insuring that no one who is not fit for the position shall hold office as a professor in a chartered University, and to so arrange that it shall be to the interest of the professor, and also to that of his University, for him to discharge his duties efficiently.

If we are to have an indefinite series of authorities one above the other, who, one would like to know, is to control the examining board which sits over the professors? And who again to control these controllers?

The bureaucratic machinery which seems to find favour with Mr. Dyer is, in my opinion, superfluous. The most efficient Universities (in two differing directions), those of Germany and of Scotland, have no authority in educational matters above that of the professoriate, and are not subject, like Oxford, Cambridge, and London, to the interference of graduates in the form of convocation.

MR. THISELTON DYER appears to think that Fichte's ideal of a University is unrealizable, unless, as he supposes, "some wealthy man gives, say, half a million to found such a University in some quiet country town in England, where professor and pupils might labour together, undisturbed by the life and movement of a big city, or the worry of the examination-room, for the advantage of knowledge." I venture to think that this supposition of Mr. Thiselton Dyer's conveys the unwelcome truth that the conception of the true nature of a University has not yet reached some even of that section of the British public who have earned well-merited distinction in science; and it is as one who has had experience of a Scottish and a German University, in the character of student and teacher, and of two English University Colleges as teacher, that I ask permission as shortly as I can to place before your readers what many minds aim at, in the hope that a teaching University in London, call it what you will, would ultimately provide it.

I reiterate the assertion which I lately made in a letter to the *Times*, that a University is primarily a place for the extension of the bounds of knowledge; this is to be achieved by the labours of the professors and teaching staff; by fellows, specially appointed for that purpose, if the system of fellowships is thought desirable, although, in my opinion and experience, much may be said against it; and by the *whole body of the students*. Of course it is not to be supposed that every student is capable of discovering new facts or of applying principles in an original manner; but almost every man is endowed with some share of inventive faculty, which must ultimately be developed, if he is to make his way in the world otherwise than as a day-labourer, or as a piece-worker in a factory, or as a copying-clerk; and the object of a University should be to cultivate this faculty to the utmost. An efficient medical man spends his life in clinical experimentation; a successful barrister exercises his ingenuity in applying old decisions to new cases; a competent engineer not merely studies how to improve his machinery, but also studies his fellow-creatures, and the chances of trade, so as to bring his manufactures into new fields. If the inventive faculty is not developed at the University, it will be developed later, in every man who fulfils his duty to his fellow-creatures and to himself.

Now I dare to contend that the degree-stamp of the English Universities, especially of the University of London, except in certain cases in its highest degrees, such as the D.Mus., D.Litt., M.D., and D.Sc. degrees (and these only as a result of recent modifications), is of no value whatever in the eyes of that portion of the public whose opinion carries with it a commercial reward. Speaking for myself, I have had assistants, graduates of Edinburgh, of London, and of German Universities, and I unhesitatingly state that the only degrees to which I should attach the least importance are those of Germany, and that because there is in them some evidence that the graduate has had at least an initiation into the methods of research. As this assertion may be applied personally, I should wish it to be clearly understood that I have no reason whatever to be in any way dissatisfied with graduates from Edinburgh or from London, but merely to state that the fact of their being graduates in no way influenced me in their appointment. And many manufacturers, in want of assistants, actually regard an English degree in the light of a disqualification; so that most of the posts of "works-chemists" are held by non-graduates. They prefer, in fact, to train their own men—that is, to give them such an education in research as bears on the particular problems which they themselves have to solve; or to take them from the laboratories of general analysts, where new problems present themselves from time to time.

It is impossible, under existing circumstances, to give undergraduates such training. They have examination on the brain. They judge from the standpoint of "Will this 'pay' at an examination?" not from the standpoint of "Is this worth knowing?" And they cannot be blamed. It is not the fault of the examiners; it is not the fault of the students; the professors, I believe, do not, except in a general way, follow the syllabuses; it is simply that the better students conscientiously aim at what is set before them—a degree that has no market value, except in the eyes of school teachers. Personally I cannot complain that I do not get research done by students; in actual fact a considerable number do stay after graduation, and some do not graduate at all; I merely hold the opinion that the method is on wholly wrong lines; that a degree, if given, should be the official testimony to a certain time spent with diligence and profit in gaining knowledge of how to attack problems—of how to acquire knowledge useful for the purpose in view.

It will be said that honours-degrees will find no place in such a system. Why should they? Does the desire to beat competitors stimulate a desire for knowledge? Does it stimulate originality? I for one would willingly see them non-existent.

Up to a certain point, the acquisition of knowledge of facts should be, as at present, tested by examination; but I am convinced that the system is at present pushed to an extreme, and that much better results would be gained by giving a degree for training, and that can be done only by the trainer—the teacher. He will, as a rule, be glad to share his responsibility with, and to benefit by the advice of, an outsider; but with him should ultimately rest the decision as to the merit or demerit of a candidate, as he is the only person able to judge. Under such a system, there would be little plucking; for the student would be advised not to present himself, unless he had sufficiently qualified.

It may also be said that undue advantage would be taken by the teacher in recommending unfit students for graduation. Teachers in such positions are, I believe, generally honourable men; they are chosen after the most careful inquiry into their past career. It is not held fitting in commercial circles to appoint a clerk or an accountant on good recommendations, and after sufficient apprenticeship, and then to surround him with safeguards, in case he turn out incapable or dishonest.

The objection may possibly be raised, that under such a system the standard of degrees would be very uneven; but what of that? As at present, anyone applying for a post of any kind would furnish a reference to his teachers; and a private letter from one well acquainted with the candidate turns the scale, for or against, in spite of every degree in the United Kingdom.

In plain English, degrees, as at present given, are not valued by that portion of the public qualified to judge; and we must face this fact, and endeavour to render a degree a real mark of merit.

I believe, with Mr. Dickins, that the examinations of the University of London have done much in disseminating knowledge, and they have therefore proved of great service, but except in the case of the higher degrees before mentioned, and of the degrees in the Faculty of Medicine where evidence of training is a *sine quâ non*, I greatly doubt whether they have contributed towards the creation of knowledge, or training in originality. And from the very nature of the constitution of the University of London, it is impossible that it should be otherwise. This very morning, I happened to ask a student attending my lectures on organic chemistry why he, a B.Sc. in chemistry, was attending my lectures. His reply was characteristic. "I scamped up enough of the subject privately, sir, to squeeze through; but now I wish to know it." In any right system, such a proceeding should be impossible.

It is therefore with the hope that the creation of a teaching University for London might tend to remedy such evils, that I, for one, would welcome it. I would urge that the distinguished names mentioned by Mr. Thiselton Dyer are surely guarantees that the London Colleges recently possessed men capable of imparting the highest standard of knowledge, and of stimulating true originality; yet I believe that it is by no means "cutting cheese with a razor" to employ just such men in watching over the development even of junior students; and it is not without advantage to the most able men of science and of letters to be obliged periodically to devote consideration to "elements" and to pass in review first principles. It counteracts the tendency towards specialization, which, however valuable, always limits the mental horizon. I will undertake to say that the quality of the most advanced teaching in biology and physiology in University College when the chairs were occupied by Burdon Sanderson, by Michael Foster, and by Lankester knew no limit; and I greatly doubt the wisdom of appointing teachers whose attention is to be devoted exclusively to research. As my predecessor, Prof. Williamson, often remarked, it is more difficult to teach junior than to teach senior students; and while the superintendence of exercise and laboratory work may well be shared by assistants, in order that the professor may have time to devote to research, and to superintendence of advanced students, it would be a serious calamity were the influence of such minds to be withdrawn wholly from the juniors.

It is precisely by such a federation of Colleges such as University and King's, and of other sufficiently qualified institutions which have the will and the power to join, that specialization may ultimately be effected. The future occupants of the chairs may be chosen so as to represent every side of a subject; and anyone wishing to pursue research in any special branch would have no difficulty in selecting that particular college where his specialty was also the specialty of the teacher.

WILLIAM RAMSAY.

No well-wisher of the University can feel otherwise than grateful to you for affording a portion of your valuable space for the letters of Mr. Thiselton Dyer and Mr. Dickins on this subject. No two men could be found to speak with greater authority from first-hand knowledge of the facts. The arguments on the subject have been too much of an *ex parte* character hitherto, not seldom based on insufficient information or erroneous impressions. Nothing, for example, could be further from the truth than the statement in the *Times* of May 13, by the writer of what was upon the whole a fair and comprehensive leading article, that "there is no reason why the highest honours of the University of London should not be obtained by a person who never set foot in London or even in England." Many, who like myself voted for the projected scheme of the Senate, must have felt, as I did, as a result of a wide and varied educational experience, that it was potential with great good in the future, and could be accepted as the working basis of the future development of the University, although we felt that the one serious blot in it was the abandonment of uniformity in the examinations for the pass degrees. I verily believe that this was the one thing fatal to its success in Convocation; that it was so far in excess of the recommendations of the Royal Commission as to be unwarrantable; and that it put a lever into the hands of the opposition, of which—as the event proved—a practised disputant like Mr. Bompas did not fail to make most effective and disastrous use.

Wellington College, Berks, May 25. A. IRVING.

Quaternions and the "Ausdehnungslehre."

THE year 1844 is memorable in the annals of mathematics on account of the first appearance on the printed page of Hamilton's "Quaternions" and Grassmann's "Ausdehnungslehre." The former appeared in the July, October, and supplementary numbers of the *Philosophical Magazine*, after a previous communication to the Royal Irish Academy, November 13, 1843. This communication was indeed announced to the Council of the Academy four weeks earlier, on the very day of Hamilton's discovery of quaternions, as we learn from one of his letters. The author of the "Ausdehnungslehre," although not unconscious of the value of his ideas, seems to have been in no haste to place himself on record, and published nothing until he was able to give the world the most characteristic and fundamental part of his system with considerable development in a treatise of more than 300 pages, which appeared in August 1844.

The doctrine of quaternions has won a conspicuous place among the various branches of mathematics, but the nature and scope of the "Ausdehnungslehre," and its relation to quaternions, seem to be still the subject of serious misapprehension in quarters where we naturally look for accurate information. Historical justice, and the interests of mathematical science, seem to require that the allusions to the "Ausdehnungslehre" in the article on "Quaternions," in the last edition of the "Encyclopædia Britannica," and in the third edition of Prof. Tait's "Treatise on Quaternions," should not be allowed to pass without protest.

It is principally as systems of geometrical algebra that quaternions and the "Ausdehnungslehre" come into comparison. To appreciate the relations of the two systems, I do not see how we can proceed better than if we ask first what they have in common, then what either system possesses which is peculiar to itself. The relative extent and importance of the three fields, that which is common to the two systems, and those which are peculiar to each, will determine the relative rank of the geometrical algebras. Questions of priority can only relate to the field common to both, and will be much simplified by having the limits of that field clearly drawn.

Geometrical addition in three dimensions is common to the two systems, and seems to have been discovered independently both by Hamilton and Grassmann, as well as by several other persons about the same time. It is not probable that any especial claim for priority with respect to this principle will be urged for either of the two with which we are now concerned.

The functions of two vectors which are represented in quaternions by $S\alpha\beta$ and $V\alpha\beta$ are common to both systems as published in 1844, but the quaternion is peculiar to Hamilton's. The linear vector function is common to both systems as ultimately developed, although mentioned only by Grassmann as early as 1844.

To those already acquainted with quaternions, the first question will naturally be: To what extent are the geometrical methods which are usually called quaternionic peculiar to Hamilton, and to what extent are they common to Grassmann? This is a question which anyone can easily decide for himself. It is only necessary to run one's eye over the equations used by quaternionic writers in the discussion of geometrical or physical subjects, and see how far they necessarily involve the idea of the quaternion, and how far they would be intelligible to one understanding the functions $S\alpha\beta$ and $V\alpha\beta$, but having no conception of the quaternion $\alpha\beta$, or at least could be made so by trifling changes of notation, as by writing S or V in places where they would not affect the value of the expressions. For such a test the examples and illustrations in treatises on quaternions would be manifestly inappropriate, so far as they are chosen to illustrate quaternionic principles, since the object may influence the form of presentation. But we may use any discussion of geometrical or physical subjects, where the writer is free to choose the form most suitable to the subject. I myself have used the chapters and sections in Prof. Tait's "Quaternions" on the following subjects: Geometry of the straight line and plane, the sphere and cyclic cone, surfaces of the second degree, geometry of curves and surfaces, kinematics, statics and kinetics of a rigid system, special kinetic problems, geometrical and physical optics, electrodynamics, general expressions for the action between linear elements, application of ∇ to certain physical analogies, pp. 160–371, except the examples (not worked out) at the close of the chapters.

Such an examination will show that for the most part the methods of representing spatial relations used by quaternionic writers are common to the systems of Hamilton and Grassmann. To an extent comparatively limited, cases will be found in which the quaternionic idea forms an essential element in the signification of the equations.

The question will then arise with respect to the comparatively limited field which is the peculiar property of Hamilton, How important are the advantages to be gained by the use of the quaternion? This question, unlike the preceding, is one into which a personal equation will necessarily enter. Everyone will naturally prefer the methods with which he is most familiar; but I think that it may be safely affirmed that in the majority of cases in this field the advantage derived from the use of the quaternion is either doubtful or very trifling. There remains a residuum of cases in which a substantial advantage is gained by the use of the quaternionic method. Such cases, however, so far as my own observation and experience extend, are very exceptional. If a more extended and careful inquiry should show that they are ten times as numerous as I have found them, they would still be exceptional.

We have now to inquire what we find in the "Ausdehnungslehre" in the way of a geometrical algebra, that is wanting in quaternions. In addition to an algebra of vectors, the "Ausdehnungslehre" affords a system of geometrical algebra in which the point is the fundamental element, and which for convenience I shall call Grassmann's algebra of points. In this algebra we have first the addition of points, or quantities located at points, which may be explained as follows. The equation

$$aA + bB + cC + \&c. = eE + fF + \&c.,$$

in which the capitals denote points, and the small letters scalars (or ordinary algebraic quantities), signifies that

$$a + b + c + \&c. = e + f + \&c.,$$

and also that the centre of gravity of the weights a, b, c, &c., at the points A, B, C, &c., is the same as that of the weights e, f, &c., at the points E, F, &c. (It will be understood that negative weights are allowed as well as positive.) The equation is thus equivalent to four equations of ordinary algebra. In this Grassmann was anticipated by Möbius ("Barycentrischer Calcul," 1827).

We have next the addition of finite straight lines, or quantities located in straight lines (*Liniengrössen*). The meaning of the equation

$$AB + CD + \&c. = EF + GH + \&c.$$

will perhaps be understood most readily, if we suppose that each member represents a system of forces acting on a rigid body. The equation then signifies that the two systems are equivalent. An equation of this form is therefore equivalent to six ordinary equations. It will be observed that the *Liniengrössen* AB and CD are not simply vectors; they have not merely length and direction, but they are also located each in a given line, although their position within those lines is immaterial. In Clifford's terminology, AB is a *rotor*, AB + CD a *motor*. In the language of Prof. Ball's "Theory of Screws," AB + CD represents either a *twist* or a *wrench*.

We have next the addition of plane surfaces (*Plangrössen*). The equation

$$ABC + DEF + GHI = JKL$$

signifies that the plane JKL passes through the point common to the planes ABC, DEF, and GHI, and that the projection by parallel lines of the triangle JKL on any plane is equal to the sum of the projections of ABC, DEF, and GHI on the same plane, the areas being taken positively or negatively according to the cyclic order of the projected points. This makes the equation equivalent to four ordinary equations.

Finally, we have the addition of volumes, as in the equation

$$ABCD + EFGH = IJKL,$$

where there is nothing peculiar, except that each term represents the six-fold volume of the tetrahedron, and is to be taken positively or negatively according to the relative position of the points.

We have also multiplications as follows:—The line (*Liniengrösse*) AB is regarded as the product of the points A and B. The *Plangrösse* ABC, which represents the double area of the triangle, is regarded as the product of the three points A, B, and C, or as the product of the line AB and the point C, or of BC and A, or indeed of BA and C. The volume ABCD, which represents six times the tetrahedron, is regarded as the product of the points A, B, C, and D, or as the product of the point A and the *Plangrösse* BCD, or as the product of the lines AB and BC, &c., &c.

This does not exhaust the wealth of multiplicative relations which Grassmann has found in the very elements of geometry. The following products are called *regressive*, as distinguished from the *progressive*, which have been described. The product of the *Plangrössen* ABC and DEF is a part of the line in which the planes ABC and DEF intersect, which is equal in numerical value to the product of the double areas of the triangles ABC and DEF multiplied by the sine of the angle made by the planes. The product of the *Liniengrösse* AB and the *Plangrösse* CDE is the point of intersection of the line and the plane with a numerical coefficient representing the product of the length of the line and the double area of the triangle multiplied by the sine of the angle made by the line and the plane. The product of three *Plangrössen* is consequently the point common to the three planes with a certain numerical coefficient. In plane geometry we have a regressive product of two *Liniengrösse*, which gives the point of intersection of the lines with a certain numerical coefficient.

The fundamental operations relating to the point, line, and plane are thus translated into analysis by multiplications. The immense flexibility and power of such an analysis will be appreciated by anyone who considers what generalized multiplication in connection with additive relations has done in other fields, as in quaternions, or in the theory of matrices, or in the algebra of logic. For a single example, if we multiply the equation

$$AB + CD + \&c. = EF + GH + \&c.$$

by PQ (P and Q being any two points), we have

$$ABPQ + CDPQ + \&c. = EFPQ + GHPQ + \&c.,$$

which will be recognized as expressing an important theorem of statics.

The field in which Grassmann's algebra of points, as distinguished from his algebra of vectors, finds its especial application and utility, is nearly coincident with that in which, when we use the methods of ordinary algebra, tetrahedral or anharmonic co-ordinates are more appropriate than rectilinear. In fact, Grassmann's algebra of points may be regarded as the application of the methods of multiple algebra to the notions connected with tetrahedral co-ordinates, just as his or Hamilton's algebra of vectors may be regarded as the application of the methods of multiple algebra to the notions connected with rectilinear co-ordinates. These methods, however, enrich the field to which they are applied with new notions. Thus the notion of the co-ordinates of a line in space, subsequently introduced by Plücker, was first given in the "Ausdehnungslehre" of 1844. It should also be observed that the utility of a multiple algebra, when it takes the place of an ordinary algebra of four co-ordinates, is very much greater than when it takes the place of three co-ordinates, for the same reason that a multiple algebra taking the place of three co-ordinates is very much more useful than one taking the place of two. Grassmann's algebra of points will always command the admiration of geometers and analysts, and furnishes an instrument of marvellous power to the former, and in its general form, as applicable to space of any number of dimensions, to the latter. To the physicist an algebra of points is by no means so indispensable an instrument as an algebra of vectors.

Grassmann's algebra of vectors, which we have described as coincident with a part of Hamilton's system, is not really anything separate from his algebra of points, but constitutes a part of it, the vector arising when one point is subtracted from another. Yet it constitutes a whole, complete in itself, and we may separate it from the larger system to facilitate comparison with the methods of Hamilton.

We have, then, as geometrical algebras published in 1844, an algebra of vectors common to Hamilton and Grassmann, augmented on Hamilton's side by the quaternion, and on Grassmann's by his algebra of points. This statement should be made with the reservation that the *addition* both of vectors and of points had been given by earlier writers.

In both systems as finally developed we have the linear vector function, the theory of which is identical with that of strains and rotations. In Hamilton's system we have also the linear quaternion function, and in Grassmann's the linear function applied to the quantities of his algebra of points. This application given those transformations in which projective properties are preserved, the doctrine of reciprocal figures or principle of duality, &c. (Grassmann's theory of the linear function is, indeed, broader than this, being co-extensive with the theory of matrices; but we are here considering only the geometrical side of the theory.)

In his earliest writings on quaternions, Hamilton does not discuss the linear function. In his "Lectures on Quaternions" (1853), he treats of the inversion of the linear vector function, as also of the linear quaternion function, and shows how to find the latent roots of the vector function, with the corresponding axes for the case of real and unequal roots. He also gives a remarkable equation, *the symbolic cubic*, which the functional symbol must satisfy. This equation is a particular case of that which is given in Prof. Cayley's classical "Memoir on the Theory of Matrices" (1858), and which is called by Prof. Sylvester the Hamilton-Cayley equation. In his "Elements of Quaternions" (1866), Hamilton extends the symbolic equation to the quaternion function.

In Grassmann, although the linear function is mentioned in the first "Ausdehnungslehre," we do not find so full a discussion of the subject until the second "Ausdehnungslehre" (1862), where he discusses the latent roots and axes, or what corresponds to axes in the general theory, the whole discussion relating to matrices of any order. The more difficult cases are included, as that of a strain in which all the roots are real, but there is only one axis or unchanged direction. On the formal side he shows how a linear function may be represented by a quotient or sum of quotients, and by a sum of products, *Lückenausdruck*.

More important, perhaps, than the question when this or that theorem was first published is the question where we first find those notions and notations which give the key to the algebra of linear functions, or the algebra of matrices, as it is now generally called. In vol. xxxi. p. 35, of this journal, Prof. Sylvester speaks of Cayley's "ever-memorable" "Memoir on Matrices" as constituting "a second birth of Algebra, its *avatar* in a new and glorified form," and refers to a passage in his "Lectures on Universal Algebra" from which, I think, we are justified in inferring that this characterization of the memoir is largely due to the fact that it is there shown how matrices may be treated as extensive quantities, capable of addition as well as of multiplication. This idea, however, is older than the memoir of 1858. The *Lückenausdruck*, by which the matrix is expressed as a sum of a kind of products (*lückenhaltig*, or open), is described in a note at the end of the first "Ausdehnungslehre." There we have the matrix given not only as a sum, but as a sum of products, introducing a multiplicative relation entirely different from the ordinary multiplication of matrices, and hardly less fruitful, but not lying nearly so near the surface as the relations to which Prof. Sylvester refers. The key to the theory of matrices is certainly given in the first "Ausdehnungslehre," and if we call the birth of matricular analysis the second birth of algebra, we can give no later date to this event than the memorable year of 1844.

The immediate occasion of this communication is the following passage in the preface to the third edition of Prof. Tait's "Quaternions":—

"Hamilton not only published his theory complete, the year before the first (and extremely imperfect) sketch of the 'Ausdehnungslehre' appeared; but had given ten years before, in his protracted study of Sets, the very processes of external and internal multiplication (corresponding to the Vector and Scalar parts of a product of two vectors) which have been put forward as specially the property of Grassmann."

For additional information we are referred to art. "Quaternions," "Encyc. Brit.," where we read respecting the first "Ausdehnungslehre":—

"In particular two species of multiplication ('inner' and 'outer') of directed lines in one plane were given. The results of these two kinds of multiplication correspond respectively to the numerical and the directed parts of Hamilton's quaternion product. But Grassmann distinctly states in his preface that he had not had leisure to extend his method to angles in space. . . . . But his claims, however great they may be, can in no way conflict with those of Hamilton, whose mode of multiplying *couples* (in which the 'inner' and 'outer' multiplication are essentially involved) was produced in 1833, and whose quaternion system was completed and published before Grassmann had elaborated for press even the rudimentary portions of his own system, in which the veritable difficulty of the whole subject, the application to angles in space, had not even been attacked."

I shall leave the reader to judge of the accuracy of the general terms used in these passages in comparing the first "Ausdehnungslehre" with Hamilton's system as published in 1843 or 1844. The specific statements respecting Hamilton and Grassmann require an answer.

It must be Hamilton's "Theory of Conjugate Functions or Algebraic Couples," (read to the Royal Irish Academy 1833 and 1835, and published in vol. xvii. of the Transactions), to which reference is made in the statements concerning his "protracted study of Sets" and "mode of multiplying *couples*." But I cannot find anything like Grassmann's external or internal multiplication in this memoir, which is concerned, as the title pretty clearly indicates, with the theory of the complex quantities of ordinary algebra.

It is difficult to understand the statements respecting the "Ausdehnungslehre," which seem to imply that Grassmann's two kinds of multiplication were subject to some kind of limitation to a plane. The external product is not limited in the first "Ausdehnungslehre" even to three dimensions. The internal, which is a comparatively simple matter, is mentioned in the first "Ausdehnungslehre" only in the preface, where it is defined, and placed beside the external product as relating to directed lines. There is not the least suggestion of any difference in the products in respect to the generality of their application to vectors.

The misunderstanding seems to have arisen from the following sentence in Grassmann's preface: "And in general, in the consideration of angles in space, difficulties present themselves, for the complete (*allseitig*) solution of which I have not yet had sufficient leisure." It is not surprising that Grassmann should have required more time for the development of some parts of his system, when we consider that Hamilton, on his discovery of quaternions, estimated the time which he should wish to devote to them at ten or fifteen years (see his letter to Prof. Tait in the *North British Review* for September 1866), and actually took several years to prepare for the press as many pages as Grassmann had printed in 1844. But any speculation as to the questions which Grassmann may have had principally in mind in the sentence quoted, and the particular nature of the difficulties which he found in them, however interesting from other points of view, seems a very precarious foundation for a comparison of the systems of Hamilton and Grassmann as published in the years 1843–44. Such a comparison should be based on the positive evidence of doctrines and methods actually published.

Such a comparison I have endeavoured to make, or rather to indicate the basis on which it may be made, so far as systems of geometrical algebra are concerned. As a contribution to analysis in general, I suppose that there is no question that Grassmann's system is of indefinitely greater extension, having no limitation to any particular number of dimensions.

J. WILLARD GIBBS.

The Flying to Pieces of a Whirling Ring.

IN NATURE of May 14 (p. 31) I notice a letter by Mr. C. A. Carus-Wilson on the rotation of a hollow steel flask, composed apparently of a spherical shell mounted on an axis constituting a diameter. Mr. Carus-Wilson speaks of this body as being under a "tension" of "31·5 tons per square inch" at a certain speed of rotation. He does not, however, specify what is the tension to which he refers, nor where it is found, neither does he give the density and elastic constants of the material nor indicate the method by which he arrived at his result.

So far as I know, the only problem of the kind which has yet been solved is that of an isotropic spherical shell [1] rotating about an imaginary axis through its centre at speeds at which the strains follow Hooke's law. This differs from the case Mr. Carus-Wilson speaks of, inasmuch as the existence of a real material axis must introduce conditions somewhat different from those assumed by the mathematical theory, and further the results obtained by this theory cannot legitimately be applied to speeds exceeding that where bulging becomes sensible, if indeed so far.

This solution is probably, however, the nearest to the practical problem at present attainable.

According to it the strains and stresses vary throughout the shell with the distance from the centre, and the angular distance from the axis of rotation. They also depend on the density and on the elastic properties of the material. There are also at every point three principal stresses, whereof one it is true vanishes over the surfaces. Thus such a statement as Mr. Carus-Wilson's requires further explanation.

According to the two theories most commonly entertained, the quantity which determines the limiting safe speed is the maximum value of either the *greatest strain* or the *maximum stress-difference*,—*i.e.* the algebraical difference between the greatest and least principal stresses at a point. Over the surfaces of the shell the absolutely greatest values of both these quantities are found, for shells of all degrees of thickness, in the equatorial plane—or plane through the centre perpendicular to the axis of rotation.

Denoting the angular velocity by ω, the radii of the outer and inner surfaces respectively by a and a', the density by ρ, Young's modulus by E, the greatest strain by s, the maximum stress-difference by S, and the stress at right angles to the meridian plane by Φ, the three last quantities being measured in the equator, the following are some of the results I found for materials in which Poisson's ratio is 1/4:—

	$Es/\omega^2\rho a^2$		$S/\omega^2\rho a^2$		$\Phi/\omega^2\rho a^2$	
	Inner surface.	Outer surface.	Inner surface.	Outer surface.	Inner surface.	Outer surface.
$a'/a = 0·9$	0·950	0·833	1·064	0·866	0·912	0·866
$\frac{a-a'}{a}$ negligible	1·0	1·0	1·0	1·0	1·0	1·0

Apparently in the case mentioned by Mr. Carus-Wilson, $a'/a = 15/16 = 0·9375$. Supposing the material to have Poisson's ratio = 1/4, which seems to accord fairly with experiments on steel, the approximate values of s, S, and Φ, for this case could be obtained by interpolation from those I give above. The differences between the values of corresponding strains and stresses at the two surfaces are less, of course, for $a'/a = 15/16$ than for $a'/a = 0·9$, but still are far from negligible. Mr. Carus-Wilson's numerical result rather suggests that the tension he refers to is the stress Φ, measured as above in the equator, and that he employed the formula $\Phi = \omega^2\rho a^2$. This formula (see Cambridge Philosophical Transactions, vol. xiv. p. 300), is correct for the value of Φ in the *equator* in an *infinitely thin* shell, but it does not strictly apply to any shell whose thickness is comparable with its radius. In the paper in the Cambridge Transactions first referred to, there are given tables of the numerical measures of the strains and stresses over the surfaces for a series of values of a'/a for materials in which Poisson's ratio is 1/4. These give by interpolation fairly accurate values for all values of a'/a. For other values of Poisson's ratio, recourse must be had to the general formulæ given in the paper, unless $\epsilon, \equiv 1 - a'/a$, is very small, when the greatest values of s and S are given approximately by $Es/\omega^2\rho a^2 = 1 - \frac{1}{2}\epsilon(1 - \eta)$, $S/\omega^2\rho a^2 = 1 + \epsilon/(1 + \eta)$, where η is Poisson's ratio (see Camb. Trans., vol. xiv. p. 304).

May 16. C. CHREE.

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[1] Cambridge Philosophical Society's Transactions, vol. xiv. pp. 467–483.

A Comet observed from Sunrise to Noon.

A SHORT time ago I got the loan of an old number of *Harper's Monthly* (March 1889), good reading matter being very acceptable, however old, in this outlandish place, in which I read an article, on the origin of celestial species, by J. Norman Lockyer, F.R.S., Cor. Inst. France, that set me thinking of what I observed of the great comet of 1882, when it made its tremendous plunge round the sun, on September 18. At that time I was master of a small vessel, trading in the Society Islands; and on the day mentioned—in latitude 16° 25′ S., longitude 151° 57′ W. of Greenwich, a position about midway between the two islands Bolabola and Maupiti (the Maurua of Cook)—I saw, with the naked eye, the comet travel about 90° of the circle of the sun's disk, between sunrise and noon; but what made it most remarkable to us was that it should be possible for us, in a perfectly clear sky, to be able to watch it all, from sunrise to noon, with very little more distress to the eye than if in a clear night looking at a full moon.

Now, Sir, may it not be that this is partly a *proof* of the theory set forth by Norman Lockyer in the article above mentioned, viz. that comets are swarms of meteorites in collision, travelling through space, and that the outer invisible part of the swarm that formed this comet's nucleus had partially eclipsed the sun, like a veil over it? I am not aware if it was noticed by any competent astronomer or not, but the chances are that none had the splendid opportunity that we had to see the phenomena; so, Sir, knowing that men of science are always glad to get facts from observers in all parts of the world is my excuse for writing this to you, not knowing Mr. Lockyer's address. Thinking this, although late, may probably be of some interest to the scientific world, I leave you to do what you may think proper with it.

WM. ELLACOTT.

Raiatea, January 30.

Graphic Daily Record of the Magnetic Declination or Variation of the Compass at Washington.

I BEG to call your attention to the enclosed reprint from the May Pilot Chart of curves of magnetic declination as recorded at the United States Naval Observatory at Washington. This reprint admits of reproduction more readily than the curves as shown on the Pilot Chart, being in black and white, and only reduced to two-fifths of true size (the reduction on the Pilot Chart itself being one-quarter). It will be interesting to this Office to elicit expressions of opinion relative to the advantages of the prompt publication of these curves. The experiment is to be tried for three months, but it is not likely to be continued longer unless certain decided advantages develop. It may be of sufficient interest to NATURE to republish these curves, and thus assist us in giving them wide publicity.

RICHARDSON CLOOER,
Washington, D.C., May 6. Hydrographer.

[We are unable to print the curves, but we may note that they are issued with the following explanation:—"These curves indicate graphically the true direction in which the magnetic needle at the Naval Observatory pointed during each instant from noon, March 29, to noon, April 30. The base-line shows a slight break at the end of each two hours, 75th meridian time, and the amount of westerly variation at any time is 4° *plus* the number of minutes represented by the height of the curve above the base line at that time, measured by the scale at the right or left margin of the diagram. The slight breaks in the curve itself occur when the chronograph sheets are changed. Although the daily change of variation at any one place, even in magnetic storms such as those that have occurred during the past month, is too small to be of any importance in practical navigation, yet it is thought that the prompt publication of these curves cannot fail to interest masters of vessels, as well as scientific men. The mean daily curve, which can be drawn by taking the average of many such curves, shows that there is a regular, though slight, daily change in the variation, somewhat analogous to the daily range of the barometer, although the daily minimum of variation at Washington occurs at about 8 a.m., and the maximum between 1 and 2 p.m. It is proposed to continue the publication of these curves on this Chart for at least three months, and any questions regarding them will receive immediate consideration and reply. The attention of masters of vessels is called to the form issued by this Office for the record of observations of variation at sea, and to the general importance of the subject in connection with vessels' compasses and the variation curves plotted on our charts."]

The Alpine Flora.

IN connection with this subject (see NATURE, vol. xliii. p. 581) it may be well to draw the attention of botanists to the fact that a young vigorous strawberry plant, in an exposed garden, will, during the winter season, place all its leaves in a perfectly horizontal position, some even close to and resting on the ground, in striking contrast to its summer habit of erect growth, whereby it is often damaged by strong winds.

Whether direct climatal conditions be the sole cause of this peculiarity, or whether inherited, I cannot determine; presumably, in its natural surroundings, the continual crowding and consequent struggle would not necessitate the adoption of dwarfing as a means of survival. J. LOVEL.

May 13.

Magnetic Anomalies in Russia.

THE magnetic disturbances in England and Wales as communicated to NATURE, vol. xliii. p. 617, by M. Mascart and A. W. Rücker, are of great interest, but the size of the disturbances between Charkov and Kursk in Russia is of much higher value. More than 150 stations with magnetic elements have proved that in the above region there are points where the declination differs by 86°, the inclination by 29°, and the magnetic total force by 0·39 el. un. The principal centres are distant from each other not more than 12 kilometres. The m. elements are :—

Principal centres of disturbance.	Decl. °	Incl. °	Total force, e. u.
Nepchaevo	+ 48	+ 81	0·84
Visloe	− 33	+ 52	0·65
Kisselevo	− 38	+ 63	0·72
Sobinino	+ 30	+ 60	0·75
Petropavlovka	− 20	+ 76	0·80
Belgorod	− 36	+ 71	0·64

The normal values are − 1° Decl. ; + 64° Incl. ; 0·48 total force. The districts are covered by sedimentary rocks.

St. Petersburg, April 30. A. DE TILLO.

THE REJUVENESCENCE OF CRYSTALS.[1]

VERY soon after the invention of the microscope, the value of that instrument in investigating the phenomena of crystallization began to be recognized.

The study of crystal-morphology and crystallogenesis was initiated in this country by the observations of Robert Boyle; and since his day a host of investigators—among whom may be especially mentioned Leeuwenhoek and Vogelsang in Holland, Link and Frankenheim in Germany, and Pasteur and Senarmont in France—have added largely to our knowledge of the origin and development of crystalline structures. Nor can it be said with justice that this field of investigation, opened up by English pioneers, has been ignobly abandoned to others; for the credit of British science has been fully maintained by the numerous and brilliant discoveries in this department of knowledge by Brewster and Sorby.

There is no branch of science which is more dependent for its progress on a knowledge of the phenomena of crystallization than geology. In seeking to explain the complicated phenomena exhibited by the crystalline masses composing the earth's crust, the geologist is constantly compelled to appeal to the physicist and chemist; from them alone can he hope to obtain the light of experiment and the leading of analogy, whereby he may hope to solve the problems which confront him.

But if geology owes much to the researches of those physicists and chemists who have devoted their studies to the phenomena of crystallization, the debt has been more than repaid through the new light which has been thrown on these questions by the investigation of naturally-formed crystals by mineralogists and geologists.

In no class of physical operations is *time* such an important factor as in crystallization; and Nature, in producing her inimitable examples of crystalline bodies, has been unsparing in her expenditure of time. Hence it is not surprising to find that some of the most wonderful phenomena of crystallization can best be studied—some, indeed, can only be studied—in those exquisite specimens of Nature's handiwork which have been slowly elaborated by her during periods which must be measured in millions of years.

I propose to-night to direct your attention to a very curious case in which a strikingly complicated group of phenomena is presented in a crystalline mass: and these phenomena, which have been revealed to the student of natural crystals, are of such a kind that we can scarcely hope to reproduce them in our test-tubes and crucibles.

But if we cannot expect to imitate all the effects which have in this case been slowly wrought out in Nature's laboratory, we can, at least, investigate and analyze them; and, in this way, it may be possible to show that phenomena like those in question must result from the possession by crystals of certain definite properties. Each of these properties, we shall see, may be severally illustrated and experimentally investigated, not only in natural products, but in the artificially-formed crystals of our laboratories.

In order to lead up to the explanation of the curious phenomena exhibited by the rock-mass in question, the first property of crystals to which I have to refer may be enunciated as follows :-

Crystals possess the power of resuming their growth after interruption; and there appears to be no limit to the time after which this resumption of growth may take place.

It is a familiar observation that if a crystal be taken from a solution and put aside, it will, if restored after a longer or shorter interval to the same or a similar solution, continue to increase as before. But geology affords innumerable instances in which this renewal of growth in crystals has taken place after millions of years must have elapsed. Still more curious is the fact, of which abundant proof can be given, that a crystal formed by one method may, after a prolonged interval, continue its growth under totally different conditions and by a very different method. Thus, crystals of quartz, which have clearly been formed in a molten magma, and certain inclosures of glass, may continue their growth when brought in contact with solutions of silica at ordinary temperatures. In the same way, crystals of felspar, which have been formed in a mass of incandescent lava, may increase in size, when solvent agents bring to them the necessary materials from an enveloping mass of glass, even after the whole mass has become cold and solid.

It is this power of resuming growth after interruption, which leads to the formation of zoned crystals, like the fine specimen of amethyst enclosed in colourless quartz, which was presented to the Royal Institution seventy years ago by Mr. Snodgrass.

The growth of crystals, like that of plants and animals, is determined by their environment; the chief conditions affecting their development being temperature, rate of growth, the supply of materials (which may vary in quality as well as in quantity), and the presence of certain foreign bodies.

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[1] The Friday Evening Discourse, delivered at the Royal Institution on January 30, 1891, by Prof. John W. Judd, F.R.S.

It is a very curious circumstance that the form assumed by a crystal may be completely altered by the presence of infinitesimal traces of certain foreign substances—foreign substances, be it remarked, which do not enter in any way into the composition of the crystallizing mass. Thus there are certain crystals which can only be formed in the presence of water, fluorides, or other salts. Such foreign bodies, which exercise an influence on a crystallizing substance without entering into its composition, have been called by the French geologists "mineralizers." Their action seems to curiously resemble that of diastase, and of the bodies known to chemists as "ferments," so many of which are now proved to be of organic origin.

Studied according to their mode of formation, zoned crystals fall naturally into several different classes.

In the first place, we have the cases in which the successive shells or zones differ only in colour or some other accidental character. Sometimes such differently coloured shells of the crystal are sharply cut off from one another, while in other instances they graduate imperceptibly one into the other.

A second class of zoned crystals includes those in which we find clear evidence that there have been pauses, or, at all events, changes in the rate of their growth. The interruption in growth may be indicated in several different ways. One of the commonest of these is the formation of cavities filled with gaseous, liquid, or vitreous material, according to the way the crystal has been formed—by volatilization, by solution, or by fusion; the production of these cavities indicating rapid or irregular growth. Not unfrequently it is clear that the crystal, after growing to a certain size, has been corroded or partially resorbed in the mass in which it is being formed, before its increase was resumed. In other cases, a pause in the growth of the crystal is indicated by the formation of minute foreign crystals, or the deposition of uncrystallized material along certain zonal planes in the crystal.

Some very interesting varieties of minerals, like the Cotterite of Ireland, the red quartz of Cumberland, and the spotted amethyst of Lake Superior, can be shown to owe their peculiarities to thin bands of foreign matter zonally included in them during their growth.

A curious class of zoned crystals arises when there is a change in the habit of a crystal during its growth. Thus, as Lavalle showed in 1851 (*Bull. Géol. Soc. Paris*, 2me. sér., vol. viii. pp. 610–13), if an octahedron of alum be allowed to grow to a certain size in a solution of that substance, and then a quantity of alkaline carbonate be added to the liquid, the octahedral crystal, without change in the length of its axes, will be gradually transformed into a cube. In the same way, a scalenohedron of calcite may be found inclosed in a prismatic crystal of the same mineral, the length of the vertical axis being the same in both crystals.

By far the most numerous and important class of zoned crystals is that which includes the forms where the successive zones are of different, though analogous, chemical composition. In the case of the alums and garnets, we may have various *isomorphous* compounds forming the successive zones in the same crystal; while, in substances crystallizing in other systems than the cubic, we find *plesiomorphous* compounds forming the different enclosing shells.

Such cases are illustrated by many artificial crystals, and by the tourmalines, the epidotes, and the felspars among minerals. The zones, consisting of different materials, are sometimes separated by well-marked planes; but in other cases they shade imperceptibly into one another.

In connection with this subject it may be well to point out that zoned crystals may be formed of two substances which do not crystallize in the same system. Thus, crystals of the monoclinic augite may be found surrounded by a zone of the rhombic enstatite; and crystals of a triclinic felspar may be found enlarged by a monoclinic felspar.

Still more curious is the fact that, where there is a similarity in crystalline form and an approximation in the dominant angles (plesiomorphism), we may have zoning and intergrowth in the crystals of substances which possess no chemical analogy whatever. Thus, as Senarmont showed in 1856, a cleavage-rhomb of the natural calcic carbonate (calcite), when placed in a solution of the sodic nitrate, becomes enveloped in a zone of this latter substance; and Tschermak has proved that the compound crystal thus formed behaves like a homogeneous one, if tested by its cleavage, by its susceptibility to twin lamellation, or by the figures produced by etching. In the same way, zircons, which are composed of the two oxides of silicon and zirconium, are found grown in composite crystals with xenotime, a phosphate of the metals of the cerium and yttrium groups.

These facts, and many similar ones which might be adduced, point to the conclusion that the beautiful theory of isomorphism, as originally propounded by Mitscherlich, stands in need of much revision as to many important details, if not, indeed, of complete reconstruction, in the light of modern observation and experiment.

The second property of crystals to which I must direct your attention is the following:—

If a crystal be broken, or mutilated in any way whatever, it possesses the power of repairing its injuries during subsequent growth.

As long ago as 1836, Frankenheim showed that, if a drop of a saturated solution be allowed to evaporate on the stage of a microscope, the following interesting observations may be made upon the growing crystals. When they are broken up by a rod, each fragment tends to re-form as a perfect crystal; and if the crystals be caused to be partially re-dissolved by the addition of a minute drop of the mother liquor, further evaporation causes them to resume their original development (*Pogg. Ann.*, Bd. xxxvii., 1836).

In 1842, Hermann Jordan showed that crystals taken from a solution and mutilated gradually became repaired or healed when replaced in the solution (*Müller Archiv. für* 1842, pp. 46–56). Jordan's observations, which were published in a medical journal, do not, however, seem to have attracted much attention from the physicists and chemists of the day.

Lavalle, between the years 1850 and 1853,[1] and Kopp, in the year 1855, made a number of valuable observations bearing on this interesting property of crystals (*Liebig Ann.*, xciv., 1855, pp. 118–25). In 1856 the subject was more thoroughly studied by three investigators who published their results almost simultaneously: these were Marbach (*Compt. rend.*, xliii., 1856, pp. 705–706, 800–802), Pasteur (*ibid.*, pp. 795–800), and Senarmont (*ibid.*, p. 799). They showed that crystals taken from a solution and mutilated in various ways, upon being restored to the liquid became completely repaired during subsequent growth.

As long ago as 1851, Lavalle had asserted that, when one solid angle of an octahedron of alum is removed, the crystal tends to reproduce the same mutilation on the opposite angle, when its growth is resumed! This remarkable and anomalous result has, however, by some subsequent writers been explained in another way to that suggested by the author of the experiment.

In the same way the curious experiments performed at a subsequent date by Karl von Hauer, experiments which led him to conclude that hemihedrism and other peculiarities in crystal growth might be induced by mutilation,[1] have been asserted by other physicists and chemists not to justify the startling conclusions drawn from them at the time. It must be admitted that new experiments bearing on this interesting question are, at the present time greatly needed.

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[1] *Bull. Géol. Soc. Paris*, 2me sér., vol. viii. pp 610–13, 1851; Moigno, *Cosmos*, ii, 1853, pp. 454–56; *Compt. rend.*, xxxvi., 1853, pp. 493–95.

In 1881, Loir demonstrated two very important facts with regard to growing crystals of alum (*Compt. rend.*, Bd. xcii. p. 1166). *First*, that if the injuries in such a crystal be not too deep, it does not resume growth over its general surface until those injuries have been repaired. *Secondly*, that the injured surfaces of crystals grow more rapidly than natural faces. This was proved by placing artificially-cut octahedra and natural crystals of the same size in a solution, and comparing their weight after a certain time had elapsed.

The important results of this capacity of crystals for undergoing healing and enlargement, and their application to the explanation of interesting geological phenomena has been pointed out by many authors. Sorby has shown that, in the so-called crystalline sand-grains, we have broken and worn crystals of quartz, which, after many vicissitudes and the lapse of millions of years, have grown again and been enveloped in a newly formed quartz-crystal. Bonney has shown how the same phenomena are exhibited in the case of mica, Becke and Whitman Cross in the case of hornblende, and Merrill in the case of augite. In the felspars of certain rocks it has been proved that crystals that have been rounded, cracked, corroded, and internally altered—which have, in short, suffered both mechanical and chemical injuries—may be repaired and enlarged with material that differs considerably in chemical composition from the original crystal.

It is impossible to avoid a comparison between these phenomena of the inorganic world and those so familiar to the biologist. It is only in the lowest forms of animal life that we find an unlimited power of repairing injuries: in the Rhizopods and some other groups a small fragment may grow into a perfect organism. In plants the same phenomenon is exhibited much more commonly, and in forms belonging to groups high up in the vegetable series. Thus, parts of a plant, such as buds, bulbs, slips, and grafts, may—sometimes after a long interval—be made to grow up into new and perfect individuals. But in the mineral kingdom we find the same principle carried to a much farther extent. We know, in fact, no limit to the minuteness of fragments which may, under favourable conditions, grow into perfect crystals—no bounds as to the time during which the crystalline growth may be suspended in the case of any particular individual.

The next property of crystals which I must illustrate, in order to explain the particular case to which I am calling your attention to-night, is the following:—

Two crystals of totally different substances may be developed within the space bounded by certain planes, becoming almost inextricably intergrown, though each retains its distinct individuality.

This property is a consequence of the fact that the substance of a crystal is not necessarily continuous within the space inclosed by its bounding planes. Crystals often exhibit cavities filled with air and other foreign substances. In the calcite crystals found in the Fontainebleau sandstone, less than 40 per cent. of their mass consists of calcic carbonate, while more than 60 per cent. is made up of grains of quartz-sand, caught up during crystallization.

In the rock called "graphic granite," we have the minerals orthoclase and quartz intergrown in such a way that the more or less isolated parts of each can be shown, by their optical characters, to be parts of great mutually interpenetrant crystals. Similar relations are shown in the so-called micro-graphic or micro-pegmatitic intergrowths of the same minerals which are so beautifully exhibited in the rock under our consideration this evening.

There is still another property of crystals that must be kept in mind, if we would explain the phenomena exhibited by this interesting rock:—

A crystal may undergo the most profound internal changes, and these may lead to great modifications of the optical and other physical properties of the mineral; yet, so long as a small—often a very small—proportion of its molecules remain intact, the crystal may retain, not only its outward form, but its capacity for growing and repairing injuries.

Crystals, like ourselves, grow old. Not only do they suffer from external injuries, mechanical fractures, and chemical corrosion, but from actions which affect the whole of their internal structure. Under the influence of the great pressures in the earth's crust, the minerals of deep-seated rocks are completely permeated by fluids which chemically react upon them. In this way, negative crystals are formed in their substance (similar to the beautiful "ice-flowers" which are formed when a block of ice is traversed by a beam from the sun or an electric lamp), and these become filled with secondary products. As the result of this action, minerals, once perfectly clear and translucent, have acquired cloudy, opalescent, iridescent, avanturine, and "schiller" characters; and minerals, thus modified, abound in the rocks that have at any period of their history been deep-seated. As the destruction of their internal structure goes on, the crystals gradually lose more and more of their distinctive optical and their physical properties, retaining, however, their external form; till at last, when the last of the original molecules is transformed or replaced by others, they pass into those mineral corpses known to us as "pseudomorphs."

But while crystals resemble ourselves in "growing old," and, at last, undergoing dissolution, they exhibit the remarkable power of growing young again, which we, alas! never do. This is in consequence of the following remarkable attribute of crystalline structures:—

It does not matter how far internal change and disintegration may have gone on in a crystal—if only a certain small proportion of the unaltered molecules remain, the crystal may renew its youth and resume its growth.

When old and much-altered crystals begin to grow again, the newly-formed material exhibits none of those marks of "senility" to which I have referred. The sand-grains that have been battered and worn into microscopic pebbles, and have been rendered cloudy by the development of millions of secondary fluid cavities, may have clear and fresh quartz deposited upon them to form crystals with exquisitely perfect faces and angles. The white, clouded, and altered felspar-crystals may be enveloped by a zone of clear and transparent material, which has been added millions of years after the first formation and the subsequent alteration of the original crystal.

We are now in a position to explain the particular case which I have thought of sufficient interest to claim your attention to-night.

In the Island of Mull, in the Inner Hebrides, there exist masses of granite of Tertiary age, which are of very great interest to the geologist and mineralogist. In many places this granite exhibits beautiful illustrations of the curious intergrowths of quartz and felspar, of which I have

[1] *Wien. Sitz. Ber.*, xxxix., 1860, pp. 611–22; Erdmann, *Journ. Prakt. Chem.*, lxxxi. pp. 356–62; *Wien. Geol. Verhandl.*, xii. pp. 212–13, &c.; Frankenheim, *Pogg. Ann.*, cxiii. 1861. Compare Fr. Scharff, *Pogg. Ann.*, cix., 1860, pp. 529–38; *Neues Jahrb. für Min.*, &c., 1876, p. 24; and W. Sauber, *Liebig Ann.*, cxxiv., 1862, pp. 78–82; also W. Ostwald, "Lehrbuch d. Allg. Chem.," 1885, Bd. i. p. 738; and O. Lehmann, "Molekular Physik.," 1888, Bd. i. p. 312.

already spoken. Such parts of the rock often abound with cavities (druses), which I believe are not of original but of secondary origin. At all events, it can be shown that these cavities have been localities in which crystal growth has gone on—they constitute indeed veritable laboratories of synthetic mineralogy.

Now, in such cavities the interpenetrant crystals of quartz and felspar in this rock have found a space where they may grow and complete their outward form; and it is curious to see how sometimes the quartz has prevailed over the felspar and a pure quartz-crystal has been produced; while at other times the opposite effect has resulted, and a pure felspar individual has grown up. In these last cases, however much the original felspar may have been altered (kaolinized and rendered opaque), it is found to be completed by a zone of absolutely clear and unaltered felspar-substance. The result is that the cavities of the granite are lined with a series of projecting crystals of fresh quartz and clear felspar, the relations of which to the older materials in an altered condition composing the substance of the solid rock, are worthy of the most careful observation and reflection.

Those relations can be fully made out when thin sections of the rock are examined under the microscope by the aid of polarized light, and they speak eloquently of the possession by the crystals of all those curious peculiarities of which I have reminded you this evening.

By problems such as those which we have endeavoured to solve to-night, the geologist is beset at every step. The crust of our globe is built up of crystals and crystal fragments—of crystals in every stage of development, of growth, and of variation—of crystals undergoing change, decay, and dissolution. Hence the study of the natural history of crystals must always constitute one of the main foundations of geological science; and the future progress of that science must depend on how far the experiments carried on in laboratories can be made to illustrate and explain our observations in the field.

BRITISH INSTITUTE OF PREVENTIVE MEDICINE.

A VIGOROUS attempt is being made by ignorant and prejudiced persons to prevent the establishment of a National Hygienic Institute worthy of the United Kingdom. A deputation will wait upon Sir Michael Hicks-Beach, President of the Board of Trade, on Friday, June 5, to submit to him an exact statement of the facts relating to the matter. Meanwhile, the Executive Committee has issued the following circular:—

On Monday afternoon, July 1, 1889, a meeting was held at the Mansion House, under the Presidency of Sir James Whitehead, Bart., then Lord Mayor of London, "for the purpose of hearing statements from Sir James Paget, and other representatives of scientific and medical opinion, with regard to the recent increase of rabies in this country, and the efficacy of the treatment discovered by M. Pasteur for the prevention of hydrophobia."

Although convinced of the advantages likely to accrue to the community at large by the founding of a Bacteriological Institute in England, the Committee felt that the time was not then come for establishing in England an institute similar to the "Institut Pasteur" in Paris, or the "Hygienische Institut" in Berlin. The idea, however, was not abandoned, and on December 5, 1889, an Executive Committee was appointed to take measures for the purpose of establishing in England a British Institute of Preventive Medicine.

Acting on the advice of their solicitors, Messrs. Hunters and Haynes, the Executive Committee decided to incorporate the Institute as a limited liability company, with the omission of the word "Limited," in order to impress the public with the fact that the Institute was not established for purposes of gain, but for purely charitable and scientific objects.

The application was lodged at the Board of Trade on February 13, 1891, and, shortly afterwards, a number of petitions were sent in asking the Board of Trade to withhold its license, as the objects of the Institute "clearly pointed to experiments on living animals." As Chairman of the Committee, Sir Joseph Lister then wrote to the President of the Board of Trade, showing why, in the opinion of the Committee, their opponents should not gain their point. In the first place, he pointed out that the granting of a vivisection license is not within the province of the Board of Trade, but under the control of the Secretary of State for the Home Department. In the second place, he clearly proved that it is absolutely necessary that the Institute should be licensed in the manner described, for it could not be registered under the Companies Act, 1862, without most seriously interfering with its prospects. From counsel's opinion it is evident that, should the Institute be registered as an ordinary limited liability company under the Act, it would at any time be possible for the members to wind up the company and divide the funds of the Institute; whereas the Board of Trade, in granting the license asked for, would make it a condition that all the property of the Institute should be applied to the advancement of science and kindred subjects only, and not be distributed among the members. In this way only could security be given that the funds would be applied for the purposes intended.

This letter was posted by one of the secretaries on May 12, 1891; but on the same day the solicitors to the Executive Committee received a letter from the President of the Board of Trade, who, without giving any reason whatever for his decision, declined to grant the application. On the next day, however, Sir Joseph Lister received a letter in answer to that posted on May 12, in which the President of the Board of Trade intimated his willingness to receive a deputation on June 5 at 11 a.m.

Workers in bacteriological science are now labouring under considerable difficulties, as there is no place in the United Kingdom specially fitted for such research. By the establishing of this Institute, they would be placed in the best possible conditions for carrying out original investigations. Moreover, a central Institute for the systematic teaching of bacteriology would be provided, not only for medical men, but also for veterinary surgeons, chemists, agriculturists, &c.

At present, in spite of the efforts made in this direction by several medical schools, most of the English workers who wish to gain special knowledge in bacteriology, are compelled to go to the Continental laboratories for their instruction. The question, therefore, which the Board of Trade will have to decide is, whether such a state of things should continue, or whether England should have its own national bacteriological Institute. Similar Institutes have been endowed by the State in other countries; and the Board of Trade, by refusing to grant their application, would prevent a body of private gentlemen from doing what has been done at great expense by the Governments of other nations.

NOTES.

We are informed that Kew has recently acquired by purchase from Mr. F. Curtis, a descendant of William Curtis, the founder of the *Botanical Magazine*, about 1650 original drawings, chiefly of figures which appeared in that publication. They belong partly to the first series and partly to the second, from 1800 to 1826—that is to say, during the period that the magazine was edited by Dr. Sims. Many of these drawings are very beautiful, and very carefully coloured, especially those done by James

Sowerby and Sydenham Edwards; but some of the finest of their work was not reproduced in the plates. The collection also includes some of the poorest work that ever appeared in the magazine. In 1815 Sydenham Edwards seceded, and worked for the rival *Botanical Register;* Sowerby had ceased contributing, and there seems to have been a lack of novelties for illustration. Towards the end of Dr. Sims's editorship, in 1826, the *Botanical Magazine* was doubtless supplanted in a great measure by the *Botanical Register* then conducted by the vigorous Lindley. Its circulation greatly decreased, and the impression was small; hence this series is very rare. The following year, however, Sir William Hooker became editor and speedily raised both the artistic and botanical character of the magazine. Many of the plates published during the latter half of Dr. Sims's editorship are not signed, but all the drawings are, and we learn that William Hooker, the artist of the *Paradisus Londinensis*, was an occasional contributor. The collection also contains a number of unpublished drawings.

A LETTER lately received from Emin Pasha by one of his ornithological correspondents in Europe is dated from one of the larger islands on Lake Victoria Nyanza in November last. It is full of details about birds, in which, as is well known, the Pasha takes the keenest interest, and alludes especially to an apparently new *Gralline* form, with three toes, met with in that district. Emin was on the point of starting southwards into the territory near the north end of Lake Tanganyika, and is now probably somewhere in that little-known country. He had been joined by Dr. Stuhlman, a young naturalist of Hamburg. Dr. G. Hartlaub, of Bremen, has just published a memoir on the birds collected by Emin during his return to the coast with the Stanley Expedition, and his subsequent sojourn at Bagamoyo. The specimens are referred to 140 species, of which eight are described as new to science.

THE Council of the Institution of Naval Architects has resolved to award the gold medal of the Institution to Prof. V. B. Lewes for his paper on boiler deposits, read at the Institution's recent annual general meeting.

THE President of the Royal Society, who is Chairman of the Board of Visitors, will hold the annual visitation of the Royal Observatory at Greenwich on Saturday, June 6 next. The Observatory will be open for inspection at 3 p.m.

MR. JAMES E. KEELER, the Astronomer of the Lick Observatory, has lately been appointed Director of the Alleghany Observatory, in succession to Mr. S. P. Langley, Secretary of the Smithsonian Institution.

A CZECH Academy of Sciences was opened at Prague on the 18th inst., by the Archduke Charles Louis. The Latin title of the Academy is Bohemica Scientiarum, litterarum et artium Academia Imperatoris Francisci Josephi; the President is Josef Hlávka, and the General Secretary Dr. F. J. Studnička.

AN extra evening meeting of the Royal Institution will be held on Tuesday, June 2, at nine o'clock, when Dr. Charles Waldstein will give a discourse on the discovery of "The Tomb of Aristotle."

AMERICAN papers announce the death of Prof. J. E. Hilgard, late superintendent of the U.S. Coast Survey. He was born at Zweibrücken in 1825, went to America with his father in 1835, and entered the service of the U.S. Coast Survey in 1845. "His work," says the New York *Nation*, "lay directly in the line of his profession, in the improvement of methods, the determination of weights and measures, and the novel method of ascertaining the differences of longitude by telegraph. His publications on these subjects are to be found chiefly in the Coast Survey Reports. One of the most noteworthy relates to the telegraphic determination of the differences of longitude between Greenwich, Paris, and Washington. He was a delegate to the International Metric Commission in 1872, and a member of the International Bureau of Weights and Measures, of which he declined the directorship. He was an original member of the National Academy of Sciences, and for some years its Home Secretary. In 1874 he was elected President of the American Association for the Advancement of Science. He succeeded to the work of Bache in connection with the work of the Bureau of Weights and Measures, and took a leading part in preparing exact metric standards for distribution to the States and Territories."

THE recent botanical mission of Mr. D. Morris to the West Indies forms the subject of the *Kew Bulletin* for May and June. The *Bulletin* publishes the official correspondence recording the circumstances under which the Imperial Government assented to Mr. Morris's mission, and reproduces his report to the Secretary of State for the Colonies.

THE *Kew Bulletin* does good service by publishing lists of garden plants annually described in botanical and horticultural publications, both English and foreign. In Appendix II., 1891, there is a list which comprises all the new introductions recorded during 1890. "These lists," says the *Bulletin*, "are indispensable to the maintenance of a correct nomenclature, especially in the smaller botanical establishments in correspondence with Kew, which are, as a rule, only scantily provided with horticultural periodicals. Such a list will also afford information respecting new plants under cultivation at this establishment, many of which will be distributed from it in the regular course of exchange with other botanic gardens."

ON the 13th inst. the Council of the county borough of Bootle decided to appropriate and set aside for the purpose of technical education the whole of the portion of the Exchequer contribution account which may so be used under the provision of the Local Taxation (Customs and Excise) Act, 1890. The Free Library and Museum Committee were entrusted with the carrying out of a scheme submitted by them to the Council; and they have appointed Mr. John J. Ogle to the office of Organizing Secretary to the Bootle Technical School. Mr. C. H. Hunt was also appointed Registrar. The sum available is estimated at £1936 per annum.

THE following is an extract from the *Times* of last week which may interest many of our readers:—Some months ago a company, which had been formed at Wheeling, West Virginia, for the purpose of "developing" that city, began to drill a well in search of petroleum or natural gas. A depth of over 4100 feet was reached, and in this distance a dozen thick veins of coal are said to have been passed, while petroleum and gas have both been struck—though not in paying quantities—and gold quartz, iron ore, and many other minerals have been brought to the surface. The officers of the Geological Survey at Washington, according to a Wheeling despatch, have become very interested in the proceeding, and "the hole will be drilled to a depth of one mile." After this the Government will take up the work under the direction of two expert officers of the Survey, and the drilling will be continued into the earth as far as human skill can penetrate. The object is to ascertain the thermometric and magnetic conditions as far as possible.

THE Transandine Railway across the Andes, connecting the Argentine railway system with that of Chili, has been the subject of an interesting article in *Engineering*. Our contemporary in its issue of this week again deals with this fine piece of engineering, and describes the tunnelling plant used, as well as the distribution by electrical means of the power available and necessary for driving the air compressors for the Ferroux rock drills used. This line across the Andes consists of a series of tunnels and other heavy works; the tunnels had to be bored in most inaccessible regions, where the means of transport are meagre in the extreme. The whole of the plant therefore had to be designed with great care and with special reference to the unusual requirements. Weight had to be minimized, and strength and simplicity had to be carefully obtained. Water-power was available at some distance from the scene of operations; the water-power was brought to the primary stations by means of 20-inch steel pipes. On the Chilian side the primary station contained ten dynamos and two spare ones, each being of 80 horse-power, and each coupled direct to, and driven by, a Girard turbine. The electric power generated is transmitted through a cable to secondary stations, where, by means of motors, the air-compressors are operated. A similar arrangement is in use on the Argentine side, only the dynamos are of 40 horse-power, because they had to be transported over mountains on mules' backs, which made it necessary to minimize the weight. This use of the electrical transmission of power is highly interesting, the circumstances being such that, without it, the boring of the tunnels would have been a work of great expense and magnitude.

Globus has received information from Japan to the effect that there is an increasing reaction in the country against foreign influences. This is said to be especially visible in schools where European instruction is given. Two such schools, one of which formerly had 300 pupils, the other 150, have been obliged to combine their forces, having no more than 150 pupils between them. At the University of Tokio the number of native lecturers increases, while that of the foreign staff decreases.

IN the New York *Sun*, Mr. G. F. Kunz, the well-known expert in gems, has recently called attention to a property of the diamond which may serve as a means of distinguishing it from other substances. Referring to the paper of Robert Boyle "On a Remarkable Diamond that Shines in the Dark," published in the Transactions of the Royal Society in 1663, Mr. Kunz remarks that this paper has been indirectly alluded to by a number of authors, but never read. Among a quantity of facts Boyle mentions one diamond that phosphoresced simply by the heat of the hand, absorbed light by being held near a candle, and emitted light on being rubbed. He stated that many diamonds emitted light by being rubbed in the dark. The experiments made by Mr. Kunz show conclusively not only that Boyle's statement that some diamonds phosphoresce in the dark after exposure to the sunlight or an arc of electric light is true, but also that all diamonds emit light by rubbing them on wood, cloth, or metal, a property which will probably prove of great value in distinguishing between the diamond and other hard stones, as well as paste, none of which exhibit this phenomenon, and will be welcomed by the general public who do not possess the experience of the dealer in diamonds. The property is evidently not electric, or it would not be visible on being rubbed on metal.

WE learn from the *American Meteorological Journal* for April that the appropriation for the new Weather Service of the United States is 879,753 dollars, being an increase of 62,797 dollars on the amount for the current year. This is accounted for by the addition of 50,000 dollars for the proposed extension of the service in agricultural regions, and by the fact that, under the present arrangement, five of the leading officials were assigned from the army, and their salaries must henceforward be provided for from the appropriation for the new Weather Service. The Chief of the Service is to receive 4500 dollars a year. No appointment has yet been made to this position. It is quite possible that the present Chief Signal Officer will be detailed from the army for this duty, and Prof. Abbe, Prof. W. M. Davis, Prof. Nipher, and Dr. Hinrichs are some of the other prominent meteorologists mentioned as possible candidates. The same *Journal* also reports that Dr. Baker, Secretary of the Michigan State Board of Health, has investigated the cause of influenza. He stated that the germs are at all times present, but that there must be certain coincident meteorological conditions to irritate the throat and air passages sufficiently to let the germ gain an entrance to the body. These conditions were, in this instance, the excessive prevalence of north and north-east winds, and the excessive amount of ozone during the past three months.

MR. C. L. WRAGGE has issued a circular, dated February 3 last, stating that "in consequence of the rapid extension of the Meteorological Service of Australasia in connection with the Queensland Government—an extension which now embraces a large portion of the Western Pacific Ocean, New Guinea, and the Malay Archipelago—it has been determined to stop the issue of the large charts which have hitherto dealt with the meteorology of Australasia only, and to issue, instead, in the early future, a weather chart as complete as possible, embracing not only Australasia, but also the regions above indicated." Some charts have already been issued giving the isobaric lines for the region referred to, and extending southwards and eastwards to New Zealand and the New Hebrides. Isobars drawn for 20° to 30° to the eastward of Brisbane must be to a great extent problematical, and in fact this is admitted by the broken lines extending over the ocean. The information, to say the least, seems at present insufficient for the purpose, and over large tracts it is absolutely wanting; but the establishment of stations in remote islands is, of itself, very desirable.

THE other day Prof. Vambéry delivered in Edinburgh, under the auspices of the Royal Scottish Geographical Society, an interesting lecture on British civilization and influence in Asia. He had many pleasant things to say about England, but did not quite overlook her shortcomings. He said he was immensely struck by the indifference shown by the public at large to everything that concerned Asia. He had lectured in more than 20 towns in this country, and found, even amongst the middle classes, great ignorance in regard to Asiatic geography and ethnography. Asiatic languages, moreover, were greatly neglected. Germany, which had not got any territory in Asia, bestowed far greater attention upon the old world than this country. He opined that if the interest in Asia would increase in this country commensurately with its political power and influence over the various races in Asia, Britain would decidedly remain there a permanent Power which could never be ousted by any rival. He thought that there ought to be more schools for Oriental languages in this country. There was a general supposition that Britons in general could not learn foreign languages, but that was not true. The greatest linguists of our age had been British, as, for example, Lord Strangford for Turkish, and the late Sir Richard Burton and the late Prof. Palmer for Arabic. Then there were scholars like Sir James Redhouse, Sir Henry Rawlinson, Sir William White, and many others bearing evidence of the brilliant linguistic capacity of the British. He believed that nothing could be easier than to recruit in this island a goodly number of Oriental linguists for employment in various Asiatic countries.

A PAPER by Messrs. G. F. Harris and H. W. Burrows, on the Eocene and Oligocene beds of the Paris Basin, is to be issued as a separate publication by the Geologists' Association. It will be illustrated by a map and sections. The paper is the result of several years' careful study of the Parisian Tertiaries, and close communication with many eminent French geologists. The authors give an elaborate appendix, consisting of a list of the fossil Mollusca, some 3500 species, showing the range in time; the nomenclature of each species has been critically revised and brought up to date. Careful attention has also been paid to the relationship between recent and Tertiary forms. The generic names under which most of the shells are still known in this country are added as an assistance to the student.

THE Physical Society of London has published the first part of the eleventh volume of its Proceedings. Among the contents are notes on photographs of rapidly moving objects, and on the oscillating electric spark, by Mr. C. V. Boys; a formula for calculating approximately the self-induction of a coil, by Prof. John Perry; a lecture experiment illustrating the effect of heat upon the magnetic susceptibility of nickel, by Mr. Shelford Bidwell; and experiments in photo-electricity, by Prof. G. M. Minchin.

A LECTURE by Prof. A. Macalister, delivered on January 29, on the opening of the new anatomical lecture-room at Cambridge, has been published by the Cambridge University Press. The subject is "The History of the Study of Anatomy in Cambridge."

MESSRS. CHARLES GRIFFIN AND CO. have published the eighth annual issue of the "Year-book of the Scientific and Learned Societies of Great Britain and Ireland." The work is compiled from official sources, and comprises lists of the papers read during 1890 before Societies engaged in fourteen departments of research, with the names of their authors.

THE Engineering Company, publishers, New York, are issuing a new monthly magazine, entitled *Engineering*, which is to be wholly devoted to the record of industrial progress. The first two numbers have been published.

THERE are some valuable morphological notes in the Johns Hopkins University Circulars for May. Among other papers we may mention one on the structure and development of the gonophores of a certain Siphonophore belonging to the order Auronectæ (Haeckel), by W. K. Brooks and E. G. Conklin. Other papers are: preliminary notes on some new species of Squilla, by R. P. Bigelow, and a preliminary note on the anatomy and transformation of Tornaria, by T. H. Morgan.

THE "Bibliothèque Évolutioniste" is the general title of a new scientific series which is being published in Paris. The editor is M. Henry de Varigny. The first volumes are mostly translations, Wallace's "Darwinism" opening the list; but French authors are also to contribute, and works are being prepared by Messrs. A. Sabatier, of Montpellier; J. Deniker, the well-known anthropologist; Prof. Giard, and others.

IN NATURE for May 14, p. 36, line 5 from top, or "1887" read "1889."

A NEW and very beautiful silver mineral is described by Mr. F. A. Genth in the May number of the *American Journal of Science*. It was discovered by Señor Aguilar, of the San Carlos Silver Mine at Guanajuato, Mexico, and has been named after him, aguilarite. It is a sulpho-selenide of silver, of the composition $Ag_2S + Ag_2Se$, the analyses of pure crystals agreeing exactly with this formula. The crystals are iron-black in colour, and possess a most brilliant lustre. They belong to the cubic system, and consist of curious skeleton dodecahedrons, the edges of which are perfect, while the centres of the faces are more or less worn or imperfectly developed. These dodecahedrons are frequently elongated in such a manner as to resemble either tetragonal prisms terminated by pyramids of the opposite order, or hexagonal prisms terminated by rhombohedral planes. They generally occur in interlaced and closely aggregated groups, the individual crystals of which attain a size of a centimetre or more in diameter. They are for the most part embedded in colourless calcite, which may readily be removed from them by means of dilute acetic acid; frequently a little quartz is associated with them. The crystals are readily sectile and malleable, and their hardness is only 2·5. Their specific gravity is 7·586. When heated in an open tube to low redness, gradually increasing to bright redness, they yield metallic silver, together with a slight sublimate of selenium, and slender needles of selenious and sulphuric oxides, which latter forms, with a little of the silver, silver sulphate. In many of the specimens of aguilarite examined, the crystals were observed to be penetrated in a remarkable manner by round holes, and they also frequently exhibited deposits of pure metallic silver upon their faces.

SEVERAL of the simpler sulphides of the organic radicles have been found to occur naturally in the crude petroleum oil of Ohio by Messrs. Mabery and Smith, who describe the mode adopted for their isolation in the current number of the *American Chemical Journal*. As far as they are aware, these alkyl sulphides have never previously been found in nature. When the higher boiling fractions of the distilled oil are agitated with oil of vitriol, these sulphur compounds are taken up by the sulphuric acid, and, upon subsequently neutralizing the acid solution with slaked lime, unstable calcium salts are obtained, which are readily decomposed by distillation in steam, which carries over the sulphides without decomposition. By employing these reactions upon a large scale, and afterwards subjecting the mixed sulphides to a rigorous fractional distillation under reduced pressure (150 mm. being the most convenient working pressure), the following sulphides have been isolated: methyl sulphide, $(CH_3)_2S$; ethyl sulphide, $(C_2H_5)_2S$; normal propyl sulphide, $(C_3H_7)_2S$; normal and iso-butyl sulphides, $(C_4H_9)_2S$; amyl sulphide, $(C_5H_{11})_2S$; hexyl sulphide, $(C_6H_{13})_2S$; and a few other sulphides of mixed radicles. Most of these sulphides were obtained in the pure state by treating the products of the fractionation with mercuric chloride, and thus obtaining crystals of the addition compounds of the type $(CH_3)_2S \cdot HgCl_2$, and subsequently decomposing these crystals of the mercury compounds with sulphuretted hydrogen.

THE additions to the Zoological Society's Gardens during the past week include a Barbary Ape (*Macacus inuus* ♂) from North Africa, presented by the Rev. G. H. Watkins; a Diuca Finch (*Diuca grisea*), two Gay's Finches (*Phrygilus gayi*) from Chili, two De Filippi's Meadow Starlings (*Sturnella defilippi*) from La Plata, presented by Mr. Charles G. Sharpe; two Bankiva Jungle Fowls (*Gallus bankiva* ♂ ♀) from India, presented by Captain George James; a Common Rhea (*Rhea americana*) from South America, presented by Mr. R. P. Houston; an Algerian Tortoise (*Testudo mauritanica*) from North Africa, presented by Mrs. Margaret Clarke; a Black-eared Marmoset (*Hapale penicillata*) from South-east Brazil, presented by Mr. Aubrey Lace; a Capybara (*Hydrochœrus capybara*), a Brown Milvago (*Milvago chimango*), a Violaceous Night Heron (*Nycticorax violaceus*) from South America, two Blue-bearded Jays (*Cyanocorax cyanopogon*) from Para, four Crested Screamers (*Chauna chavaria*) from Buenos Ayres, deposited; two Variegated Sheldrakes (*Tadorna variegata*) from New Zealand, two Larger Tree Ducks (*Dendrocygna major*) from India, purchased; two Japanese Deer (*Cervus sika* ♂ ♀), a Chinchilla (*Chinchilla lanigera*), an African Wild Ass (*Equus tæniopus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE DRAPER CATALOGUE OF STELLAR SPECTRA.—Vol. xxvii. of the *Annals of the Astronomical Observatory of Harvard College* contains a catalogue of the spectra of 10,351 stars, nearly all of them north of the parallel of declination − 25°, photographed with the 8-inch Bache telescope. As the work forms a part of the Henry Draper Memorial, it is suggested that it be designated as the Draper Catalogue. In order to produce the spectra, a prism 8 inches square and having a refracting angle of 13°, was fastened in front of the object-glass, with its refracting angle placed perpendicular to the earth's axis. The spectra obtained have been conveniently arranged in classes indicated by the letters A to Q. Of these, A, B, C, and D indicate varieties of Secchi's first type, E to L varieties of the second type, M the third type, and N the fourth type. The letter O is used for stars whose spectra consist mainly of bright lines, and the letter P is reserved for planetary nebulæ. The classes O and P closely resemble each other, and are regarded by Prof. Pickering as a fifth type of spectrum. All spectra not included in these classes are indicated by the letter Q. Viewed as the result of a preliminary survey of the types of the photographic spectra of stars, the catalogue is of the highest importance. But it is to the discussion of individual lines, which is to follow in another volume, that we have to look for detailed information which may improve our knowledge of stellar constitution.

SOLAR OBSERVATIONS FROM JANUARY TO MARCH 1891.—In *Comptes rendus*, No. 19 (May 11, 1891), Prof. Tacchini gives the following account of solar activity during the first three months of this year.

Observations of spots and faculæ have been made on 64 days, viz. 16 in January, 26 in February, and 22 in March. The results obtained are :—

1891.	Relative frequency		Relative magnitude		Number of groups per day.
	of spots.	of days without spots.	of spots.	of faculæ.	
January	1·56	0·30	18″·50	16″·88	1·38
February	2·31	0·15	24·04	89·62	2·38
March	1·27	0·14	11·91	41·82	1·45

The following are the results of observations of hydrogen prominences :—

1891.	Number of days of observation.	Prominences		
		Mean number.	Mean height.	Mean extension.
January	13	4·62	36″·9	1°·3
February	22	7·55	44·1	1·8
March	17	6·12	40·1	1·5

When these numbers are compared with those obtained for the last three months of 1890, a marked increase is apparent. In addition to this the results obtained for spots, faculæ, and prominences indicate that a secondary maximum of solar activity occurred during the month of February.

THE CONSTANT OF ABERRATION.—A short time ago MM. Lœwy and Puiseux described the principle of their new method of studying annual aberration and the general conclusions deduced from the observations made last year (see NATURE, vol. xliii. p. 498). In *Comptes rendus*, No. 20 (May 19) they give a detailed account of the *modus operandi*, and the numerical values obtained by the observation of two groups of four stars. The mean of all the observations gives for the constant of aberration the value 20″·447 ± 0″·024.

ANIMAL LIFE ON A CORAL REEF.[1]

IN nearly all the shallow waters of the tropical seas there is an abundant fauna, but nowhere is there such a crowd of marine animals of all kinds as there is in the region that extends from the growing edge of the coral reef to a depth of some 10 or 15 fathoms beyond it. This may be due to the fact that in this region there is plenty of light and heat, no great or sudden changes of temperature, or of the chemical composition of the water, and there is an abundant food supply brought by tidal currents from the surface of the ocean. Here it is, then, that we find the richest fauna. Here it is that the struggle for existence is most severe, and here it is that the animals are protected and concealed by the most pronounced marks and colours, and provided by Nature with various forms of armour, stings and spines to defend them in the battles with their enemies.

One of the most interesting results of this severe struggle for existence, or perhaps it would be more correct to say of the large number of species competing for existence, is the important faunistic difference that may be observed between one reef and another—nay, indeed, between one part of a reef and another part of the same reef.

Darwin long ago pointed out that in the struggle for existence a very slight advantage gained by any one of the competing species may entirely alter the whole aspect of the field ; and it follows that a very slight though constant difference in the physical conditions, such, for example, in the case of coral reefs, as rapidity of tidal currents, amount of surf or character of the shore rocks, may completely change the characteristics of the fauna. There are, it is true, some genera and species that are apparently found on all the reefs, such as Tubipora and Madrepora, but every reef has its own peculiar characters, and a naturalist never feels when he is examining one that he has seen something exactly like it on any previous occasion.

The majority of the corals that are found on the reefs of North Celebes belong to two great orders—the Zoantharia and Alcyonaria. The prevailing colour of the living Zoantharia is dull greeny-brown. The tentacles and the oral disks, and in some cases the growing or younger branches as a whole, may be very brightly coloured. White, pink, emerald green, violet, and blue, are colours frequently met with in different parts of the Zoantharian colony. The colours of the Alcyonarians may be due to the bright red, yellow, or purple spicules, or to the rich brown or green colour of the soft parts. There is very considerable variation in the colour of the soft parts of the Alcyonaria. The tentacles of the polyps of Tubipora, for example, may be any shade between bright green and pinkish-brown. A species of Sarcophytum, again, common on the shores of Celebes, showed green and greenish-yellow and yellow examples within the same half-mile of reef. All of these coral colours, with the exception of the colour of the spicules mentioned above, are soluble in spirit, the soft parts becoming, after prolonged immersion in this fluid, pale brown. The alcohol extracts of a considerable number of corals have now been submitted to spectrum analysis, and the bands they exhibit show close affinities with vegetable chlorophyll.

There is no experimental evidence at present that proves that the colours of the corals, nor, indeed, of the sponges, are either protective or warning in function. It seems much more probable that these brilliant colours represent different stages in the building up or breaking down of some complex chemical substance that is always present in marine zoophytes, and performs some important physiological function.

Besides the numerous sponges, corals, holothurians, mollusks, &c., that are attached to the bottom or creep but slowly from place to place, the numerous species of swimming animals that are capable of active movements in pursuit of prey, or escaping from their enemies, must be considered as part of the fauna of the coral reef. These include fishes, cephalopods, and crustacea, and those of them that seem to live habitually among the corals of the reef are characterized by the possession of very curious spots or stripes and very brilliant colours.

Soon after my arrival in Talisse a large lobster was brought to me marked by broad transverse bands of blue and white ; a large Squilla is not uncommon marked with similar bands of white and deep purple, and the little prawn *Stenopus hispidus*, that I found in a tidal pool close to a reef, has bands of red and white. The cephalopods have also peculiar markings. One specimen that I found, *Octopus lunulatus*, had large blue spots over its body and arms. The fishes, again, are marked with spots and stripes of various kinds and many brilliant colours.

Without going too deeply into the argument, we are justified in saying that these animals are so marked and coloured because they live among the brilliant surroundings of the coral reef ; or, to put it in another way, animals similarly organized and of similar habits would be at a disadvantage on the coral reefs if they were not so marked and coloured. The other fishes of the tropics do not possess these curious and beautiful characters ; the sharks, bonitos, flying fishes, herrings, and others that do not live habitually on the coral reefs are not unlike in general colour and ornamentation the fish of temperate seas. Again, the crustacea and fish of the tropical rivers and lakes are not as a rule characterized by any peculiar colouring or marking. These peculiarities, then, are not directly due to the high temperature and bright light of the tropics, but they are due to the character of the surroundings.

Most of the colours must be considered to be concealment colours. *Stenopus hispidus*, though so very conspicuous when taken out of the water, was extremely difficult to see in the pool where I found it. I should, in all probability, have failed to notice it, had I not quite unintentionally and blindly touched it with my stick. Like all animals protected by concealment colours, it remained perfectly motionless when alarmed. When looking down on to the growing edge of a reef from a boat on a calm day, it is very difficult at first to see anything but the corals and sponges. After a time, when the eyes become more accustomed to the light, the fish may be distinguished. Those that are coloured blue are much less readily seen than the gold, yellow, and red varieties; but an examination of the fish that I caught myself, and were caught for me by the natives, showed that the fish in which blue is the prevailing colour are much more frequent in the very shallow water, while those that were caught in water from 15 to 20 fathoms were more frequently red or yellow. The blue colour seems to be a protection for the fish from air-breathing enemies—the eagles, ospreys, and hawks—and as these enemies can only approach them from above, the colours are frequently confined to the dorsal sides. The red and yellow colours of the fish seem to be a protection from animals, such as the sharks, perch, and other carnivorous fish, that approach them from the deeper waters beyond the reefs. Thus red and yellow fishes rarely have these colours confined to the upper sides, and many of the blue fishes are coloured red or yellow ventrally.

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[1] Abstract of Lecture by Dr. S. J. Hickson, delivered at the London Institution, January 22, 1891.

It is difficult to frame any general rule to account for the curious distribution of the colours of these animals in spots and stripes. Speaking in very general terms, for there are many exceptions, the fish that browse on the corals, possessing small mouths and chisel-shaped teeth (such as the Chætodons, Trigger fish, and Surgeons), are striped; those that feed on other fish, and have large mouths armed with carnivorous teeth, such as the Serranidæ, are spotted.

The only example of what appears to be a warning colour that I have noticed occurs in connection with the spines on the tails of certain Surgeons and Trigger fish. *Acanthurus achillis*, for example, has a uniform purple colour, but there is a bright red patch surrounding the formidable tail spines that give these fish the name of Surgeons. Similar warning colours are very pronounced also in *Naseus unicornis* and *Naseus lituratus*, and in some of the Balistidæ.

WASHINGTON MAGNETIC OBSERVATIONS, 1886.[1]

THIS volume contains the results that have been obtained from the magnetic observations taken at the Naval Observatory during the years 1888 and 1889. The instruments with which they were made were, in the year 1887, placed in their respective buildings that had been erected for that purpose by the Bureau of Navigation. In the construction of these buildings the greatest care was taken to insure the complete elimination of local disturbances. No iron or any magnetic material was used at all, and the fastenings, &c., were entirely of copper, brass, and wood; even the stoves, in which only wood was burnt, were of soap-stone, with copper pipes.

The instruments that were employed consisted of a declinometer, theodolite, portable magnetometer, dip-circle, a set of self-recording magnetographs, a seismoscope, and seismograph; each of them, with the exception of the last two mentioned, being set on piers based on concrete, and in no way connected with the floors of the buildings. To complete the equipment, a compass-testing stand is placed on a pier north of the theodolite, and is capable of motion in an east and west direction. By means of an arm carrying two prisms that have adjusting screws, the opposite marks on the compass card can be placed in the field of view of the theodolite when the latter is directed on the prisms. All the observations, which are represented in tabular form, denote the results that have been obtained after applying all necessary corrections. The tables include, among others, the mean hourly values of the horizontal and vertical force for each month of 1889, and of the declination for each month of 1888 and 1889, the last of which are taken from the monthly curves; declination ordinates for each hour, in minutes of arc taken from daily declination traces; hourly values of horizontal and vertical force in absolute measure with all corrections; observations of horizontal intensity and dip, with a summary of disturbances in declination which differed two minutes or more from the mean monthly curve.

No less important is the series of the fourteen large plates at the end of the volume. The first shows the way that the daily photographic traces of declination, horizontal and vertical force are recorded; while the second illustrates the mean diurnal variation of the magnetic elements for the year 1889. In this latter plate the curve that gives the integration of these elements—that is, that gives the mean diurnal total force—brings out the fact that in every twenty-four hours there are two maxima and two minima, these latter two occurring between midnight and noon (75th meridian mean time).

Plates iii. to vi. inclusive show the traces of the monthly composite curves of declination for the two years.

In Plates vii. to xiv. most interesting comparison is made of the disturbed days of declination taken from observations at Washington, Los Angeles (California), Toronto (Canada), and Pawlowsk (Russia): the curves are all computed for the same time (*i.e.* for the 75th meridian west of Greenwich), and reduced to the same length of base line. Although on the whole the curves show a more or less equal variation, yet there are some cases in which a decided local variation has taken place. For instance, on January 20, between the hours of noon and four o'clock (75th meridian time), the magnetic declination at Washington, Los Angeles, and Toronto, shows only slight variations, while at Pawlowsk the disturbance is in comparison quite large. Another very interesting case happens on March 17, when the curves traced at Washington and Toronto are quite similar to each other, but different from those traced at the other two places: the curve showing the magnetic disturbances in declination at Pawlowsk being very similar to that indicating the horizontal force at Washington.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The subject of the Rede Lecture, to be given by Sir Alfred Lyall on June 17, is "Natural Religion in India."

The General Board of Studies have again brought forward proposals for the increase of the stipends paid to University Lecturers and Demonstrators in Natural Science, which had to be postponed last year owing to want of funds.

Mr. A. Hutchinson, Demonstrator of Chemistry in Caius College, has been recognized as a Teacher of Chemistry with reference to the regulations for medical degrees.

A Syndicate is proposed by the Council of the Senate for the purpose of considering whether any alternative for Greek should be permitted in the Previous Examination. This is sure to rouse much agitation, but it may be hoped that the long-vexed question will at length be settled in a liberal sense.

Another Syndicate is to consider the office of Superintendent of the Museums of Zoology and Comparative Anatomy, about to be vacated by Mr. J. W. Clark, Registrary. Some rearrangement of the duties, &c., is considered desirable.

SCIENTIFIC SERIALS.

American Journal of Science, May.—On the relationship of the Pleistocene to the pre-Pleistocene formations of the Mississippi basin, south of the limit of glaciation, by T. C. Chamberlain and R. D. Salisbury.—On certain measures of the intensity of solar radiation, by William Ferrel. The author shows that many measures of the intensity of solar radiation are of uncertain value. He specially discusses M. Crova's curves of the relative intensities of solar radiation, obtained at Montpellier with a modified form of the thermopile, called the registering actinometer.—Geological age of the Saganaga syenite, by Horace V. Winchell.—On a self-feeding Sprengel pump, by H. L. Wells.—Contributions to mineralogy, No. 50, by F. A. Genth; with crystallographic notes by S. L. Penfield and L. V. Pirsson. The composition and habits of the following minerals are given: three new varieties of axinite, eudialyte, and monticellite, and titanite from Magnet Cove, Arkansas.—

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[1] Appendix I.—"Magnetic Observations." By Ensign J. A. Hoogewerff, U.S. Navy. (Washington: Government Printing Office, 1890.)

Contributions to mineralogy, No. 51, by F. A. Genth. A new species, which has been named aguilarite, is described. It appears to be a cupriferous stephanite with an admixture of metallic silver.—Columbite of the Black Hills, South Dakota, by W. P. Blake.—The raised reefs of Fernando de Noronha, by Henry N. Ridley.—The cause of active compressive stress in rocks and recent rock flexures, by T. Mellard Reade.—A new phosphate from the Black Hills of South Dakota, by W. P. Headden.—Note on certain peculiarities in the behaviour of a galvanometer when used with the thermopile, by Ernest Merritt.—Supplementary notice on the polycrase of North and South Carolina, by W. E. Hidden and J. B. Mackintosh.

THE *American Meteorological Journal* for March contains:—An article by S. M. Ballou, on Prof. Russell's theory of cold waves, published in the Report of the Chief Signal Officer for 1889. This article is a reprint of a paper read at the meeting of the New England Meteorological Society on January 20 last. According to Prof. Russell's theory, the cause of the cold area from which the cold wave is drawn is held to be a preliminary strong upward diminution of temperature in the air, a subsequent overturning, bringing the cold air to the surface and producing uniform temperature upwards, and a further cooling above, producing high pressure. The author points out that each of these assumptions would probably be questioned, and he considers each of them in detail, quoting from the works of various authorities upon the subject.—Temperature in high and low areas. This is a translation of the substance of a reply by Dr. Hann, in the *Meteorologische Zeitschrift* of September 1890, to the criticisms of Prof. Hazen. These papers have already been noticed at length in NATURE.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, April 30.—"The Passive State of Iron and Steel, Part III." By Thos. Andrews, F.R.S., M.Inst.C.E.

Series V., Set 1. *Relative Passivity of Wrought-iron and various Steel Bars, and the Influence of Chemical Composition and Physical Structure on their Passive State in Cold Nitric Acid.*—The passive state of iron or steel may have hitherto been regarded by many as a sort of fixed property pertaining to iron and steel alike, when immersed in cold strong nitric acid. The following experiments were made to investigate if the passivity was of a universally static character, or whether it varied with the chemical composition and general physical structure of the metal, and, if so, to what extent.

The experiments of Series V., Set 1, were made on bars of the various steels selected from the author's standard samples. The bars were cold drawn through a wortle, and were therefore different in physical structure to the rolled plates used in the second series of the experiments. An idea of their general properties will be obtained on reference to Part II., Tables IV. and V. A polished bar, 8¼ inches long, 0·310 inch diameter, of the steel to be tested was placed in the wooden stand W, along with a polished wrought-iron bar of equal size, and the pair were then immersed in 1¼ fluid ounce of nitric acid, 1·42 sp. gr., contained in the U-tube, the bars being in circuit with the galvanometer. The immersion was continued for the periods stated, and with the electro-chemical results given on Table VI.

The wrought-iron bars used in each experiment were cut from one longer polished rod, so as to afford a fair comparison of the relative passivity of the various steels, compared with the wrought-iron and also with each other. The results are the average of numerous experiments in each case.

The experiments of Series V., Set 1, on the relative passivity of wrought-iron, soft cast-steel, hard cast-steel, soft Bessemer steel, and tungsten steel, showed that wrought-iron was electro-positive to the steels with a considerable E.M.F., the wrought-iron being thus shown to be less passive than the steels.

Series V., Set 2. *Relative Passivity of Wrought-iron and various Steel Plates in Cold Nitric Acid*, sp. gr. 1·42.—In the following series of observations, the metals experimented upon consisted of plates of rolled wrought-iron, rolled steels made by the Bessemer, Siemens-Martin, or crucible cast steel processes, such as soft cast-steel, hard cast-steel, soft Bessemer steel, hard Bessemer steel, soft Siemens steel, hard Siemens steel, and they are of the chemical composition given on Table VII. The terms "soft" and "hard" relate only to difference of percentage of combined carbon, and not to their having undergone annealing or hardening processes. Each plate was 3 inches square, by ⅛ inch thick = total area of exposure 19·5 square inches including edges, brightly polished all over, and had a long thin strip left on the top side, for convenience of attaching to the galvanometer connections. The whole of the wrought-iron plates, used as elements with the various steel plates, were cut from one larger wrought-iron plate, and were thus practically of uniform composition, thus ensuring an accurate comparison of the relative passivity of the wrought-iron compared with the different types of steels, and at the same time indicating relatively the influence of varied composition and structure on the passivity of the different classes of steels under observation. In each experiment, a polished wrought-iron plate and a polished steel plate were firmly placed in two small holes drilled through a thick plate-glass cover; the cover holding the two plates was then carefully placed closely over a porcelain vessel containing 15 fluid ounces of nitric acid, sp. gr. 1·42, the plates being fully immersed in the acid, and the protruding shanks of the bars connected in circuit with the galvanometer. The electro-chemical effects observed were then taken in the usual manner, and the results are given in detail on Table VIII., and indicated that wrought-iron was less passive than the steels, and further demonstrated that steels of a higher percentage of combined carbon are more passive than those of a lower percentage of combined carbon.

General Remarks.—It has been necessary to give in modified detail the effects observed during the periods of experimentation recorded on the Tables, Parts I., II., and III., so as to convey an accurate intimation of the method and nature of the research; and a brief *résumé* of some of the principal results and conclusions arrived at by the author up to the present time may now be given.

(1) The experimental observations of Part I., Series I., indicate that the influence of magnetization on the passive state of steel rods in cold nitric acid, sp. gr. 1·42, is not very great, but it was detectable with the delicate galvanometer and by the sensitive electro-chemical method pursued by the author in the investigation.

The effect of magnetization is more marked in warm nitric acid and when the iron is in a powdered state, as shown in the independent and separate experiments of Messrs. Nichols and Franklin on passive powdered iron in warm nitric acid, previously alluded to in Part I., by whom it was shown that the temperature of transition from the passive to the active state was very materially lowered by powerful magnetism; their experiments also indicate that the passive state of powdered iron cannot be fully overcome, even under strong magnetic influence, until a temperature of about 51° C. is reached.

(2) The author's experiments of Part I., Series II., at higher temperatures, confirm those of Part I., Series I., and further tend to demonstrate the influence of magnetization in somewhat lessening the passivity of steel, showing that even previous to the critical temperature point of transition from the passive to the active state, magnetized steel bars were rather less passive in warm nitric acid than unmagnetized ones.

(3) The results in Part II., Series III., show that the passivity of both unmagnetized wrought-iron and unmagnetized steel in nitric acid, sp. gr. 1·42, is considerably and proportionately reduced as the temperature of the acid increases, until the temperature point of transition from the passive to the active state is reached at a temperature of about 195° F., and it was also found that the wrought-iron was less passive in the warm nitric acid than cast-steel. (See also remarks at foot of Diagram I. in Part II.)

(4) The results of the observations of Part II., Series IV., indicate that Scheurer-Kestner was, to some extent, in error in regarding the passivity of iron as not dependent on the greater or less degree of saturation of the acid. The author's experiments herein recorded have shown that the passivity of the metals employed, viz. wrought-iron, soft cast-steel, hard cast-steel, soft Bessemer steel, and tungsten steel, was very materially increased with the concentration of the nitric acid; and it was also observed that wrought-iron was much less passive in the nitric acid of less concentration than most of the steels, the soft Bessemer steel being found about equal in passivity to the wrought-iron under the conditions of experimentation. A re-

ference to Table III. shows that a considerable amount of E.M.F. was developed between the various metals in every instance, which is a circumstance of much interest in connection with the passive state of iron and steel.

(5) The results obtained in Part III., Series V. and VI., on the relative passivity of wrought-iron and the various steels—soft cast-steel, hard cast-steel, soft Bessemer steel, hard Bessemer steel, soft Siemens steel, and hard Siemens steel—are of an important character, showing, by the delicate electro-chemical method employed, the sensitive influence of difference in chemical composition and physical structure, &c., on the passive state of the metals. Generally throughout this series of experiments it will be observed that the wrought-iron was electro-positive to the steels with a considerable E.M.F., amounting, in some cases, to as high as one-tenth to one-seventh of a volt, the wrought-iron being thus shown to be less passive than the steels.

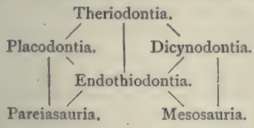
A reference to the experiments on the wrought-iron and various steel plates, on Table VIII., shows that the E.M.F. between the passive wrought-iron and the various soft steels, which contained less percentage of combined carbon, in circuit in cold nitric acid, sp. gr. 1·42, was very considerably less than the E.M.F. under similar conditions between the wrought-iron plates and the different hard steels having a higher percentage of combined carbon. The latter results, therefore, demonstrate the interesting circumstance that steels of a higher percentage of combined carbon are more passive than those of a lower percentage of combined carbon. It will be observed that the wrought-iron was also electro-positive to most of the steels, whether of a higher or lower percentage of combined carbon, which shows that wrought-iron may be regarded as generally less passive than steels.

May 14.—"Researches on the Structure, Organization, and Classification of the Fossil Reptilia. VII. Further Observations on *Pareiasaurus*." By H. G. Seeley, F.R.S., Professor of Geography in King's College, London.

All the affinities hitherto attributed to *Pareiasaurus* with Labyrinthodonts, Anomodonts, *Procolophon*, and Mammals are shown more strongly in the several parts of the skeleton, by the new evidence. The shoulder-girdle is more Labyrinthodont than was previously supposed, the skull is more Reptilian, and the pelvis and limbs are more Mammalian, though with some resemblance to Dinosaurs.

From furthur evidence of the structure of the skeleton in *Procolophon*, the author regards that type as a member of the Pareiasauria, rather than as forming a distinct sub-order. It also has four sacral vertebræ.

The divisions of the Anomodontia are grouped as—



Physical Society, May 9.—The Society varied its ordinary procedure by paying a visit to the ancient seat of learning situated on the banks of the Cam. Assembling at Liverpool Street Station, members and visitors to the number of about one hundred were conveyed in saloon carriages by the 11 o'clock express direct to their destination, the whole journey being accomplished in about seventy-five minutes. Amongst those present were Dr. E. Atkinson, Prof. Ayrton and Mrs. Ayrton, Mr. Walter Baily, Mr. Shelford Bidwell and Mrs. Bidwell, Mr. D. J. Blaikley, Mr. T. H. Blakesley and Mrs. Blakesley, Mr. J. T. Bottomley, Mr. C. V. Boys, Prof. Carey Foster, Mr. Conrad W. Cooke, Prof. Fitzgerald, Dr. E. Frankland and Mrs. Frankland, Dr. W. R. Hodgkinson, Prof. O. J. Lodge, Prof. Meldola, Prof. Perry and Mrs. Perry, Prof. Rücker, Dr. Sumpner, Prof. S. P. Thompson and Mrs. Thompson, Mr. A. P. Trotter and Mrs. Trotter, and Mr. G. M. Whipple. On arriving at the historic town the party became the guests of the Cambridge members, and proceeded to Emmanuel College, where they were received by Mr. W. N. Shaw. Various groups visited the cloisters, chapel, and gardens, and at one o'clock lunch was provided in the College Hall. At 2.30, a meeting of the Society was held in the Lecture Room of the Cavendish Laboratory. The papers read were all by authors resident in Cambridge, and the abstracts given below will sufficiently indicate the variety of the subjects brought before the Society. After the meeting the visitors inspected the Cavendish Laboratory. Amongst the many interesting instruments and apparatus to be seen, specially noticeable were Prof. J. J. Thomson's 50-feet vacuum tube, glowing from end to end with a luminous discharge; Mr. Shaw's pneumatic bridge, by which the pneumatic resistance or conductivity of various shaped orifices and channels can be compared; and the new air condensers to be used by Mr. Glazebrook as standards. The Cambridge Scientific Instrument Company had an interesting exhibit, including a dividing engine, Boys's radio-micrometer, electrically driven tuning-forks, and various recording instruments, amongst which was Galton's apparatus for registering the growth of plants. Other things which attracted attention were Glazebrook's spectrophotometer; Lord Rayleigh's coils and apparatus used in his determination of the ohm; a collection of models, medals, and instruments, formerly belonging to Prof. Maxwell; the resistance standards of the British Association, together with the historic rotating coils and electrodynamometer used in the determination of the B.A. unit. Tea was served in the Combination Room of Trinity College, and a majority of the visitors returned to town by the 8 o'clock express, greatly pleased with the day's outing. Others, however, prolonged their visit until Monday, and had opportunities of discussing important physical problems with the Cambridge members. The meeting was in every sense a great success, and will long be remembered as a red-letter day in the history of the Society.—At the science meeting, held in the Cavendish Laboratory, Prof. Ayrton, F.R.S., President, in the chair, the following communications were made:—Some experiments on the electric discharge in vacuum tubes, by Prof. J. J. Thomson, F.R.S. The phenomena of vacuum discharges were, he said, greatly simplified when their path was wholly gaseous, the complication of the dark space surrounding the negative electrode and the stratifications so commonly observed in ordinary vacuum tubes being absent. To produce discharges in tubes devoid of electrodes was, however, not easy to accomplish, for the only available means of producing an electromotive force in the discharge circuit was by electro-magnetic induction. Ordinary methods of producing variable induction were valueless, and recourse was had to the oscillatory discharge of a Leyden jar, which combines the two essentials of a current whose maximum value is enormous, and whose rapidity of alternation is immensely great. The discharge circuits, which may take the shape of bulbs or of tubes bent in the form of coils, were placed in close proximity to glass tubes filled with mercury, which formed the path of the oscillatory discharge. The parts thus corresponded to the windings of an induction coil, the vacuum tubes being the secondary and the tubes filled with mercury the primary. In such an apparatus the Leyden jar need not be large, and neither primary or secondary need have many turns, for this would increase the self-induction of the former and lengthen the discharge path in the latter. Increasing the self-induction of the primary reduces the E.M.F. induced in the secondary, whilst lengthening the secondary does not increase the E.M.F. per unit length. Two or three turns in each were found to be quite sufficient, and on discharging the Leyden jar between two highly polished knobs in the primary circuit a plain uniform band of light was seen to pass round the secondary. An exhausted bulb containing traces of oxygen was placed within a primary spiral of three turns, and on passing the jar discharge a circle of light was seen within the bulb in close proximity to the primary circuit, accompanied by a purplish glow which lasted for a second or more. On heating the bulb, the duration of the glow was greatly diminished, and it could be instantly extinguished by the presence of an electro-magnet. Another exhausted bulb surrounded by a primary spiral was contained in a bell jar, and when the pressure of air in the jar was about that of the atmosphere, the secondary discharge occurred in the bulb, as is ordinarily the case. On exhausting the jar, however, the luminous discharge grew fainter, and a point was reached at which no secondary discharge was visible. Further exhaustion of the jar caused the secondary discharge to appear outside the bulb. The fact of obtaining no luminous discharge either in the bulb or jar the author could only explain on two suppositions, viz. that under the conditions then existing the specific inductive capacity of the gas was very great, or that a discharge could pass without being luminous. The author had also observed that the conductivity of a vacuum tube without electrodes increased as the pressure diminished, until a certain point was reached, and afterwards diminished again, thus showing that the high resistance of a nearly perfect vacuum is in no way due to the presence of the electrodes. One peculiarity of the discharges was their local nature, the rings of light being much more sharply defined than was to be expected. They were also found to be most easily produced when the chain of molecules in the discharge were all of the same kind. For example, a discharge could be easily sent through a tube many feet long, but the introduction of a small pellet of mercury in the tube stopped the discharge, although the conductivity of the mercury was much greater than that of the vacuum. In some cases he had noticed that a very fine wire placed within a tube on the side remote from the primary circuit would present a luminous discharge in that tube.—Some experiments on the velocities of the ions, by Mr. W. C. D. Whetham. In studying electrolysis the question as to whether there is any transference of solvent when a porous wall is absent presented itself to the author. The ordinary methods of testing for transference, such as by increase of pressure, or by overflow, not being available, when there is no diaphragm, the author used different coloured solutions of the same salt, such as cobalt chloride in water and in alcohol, the former of which is red and the latter blue. By putting the solutions into a kind of **U**-shaped tube any change in the position of the line of junction of the two liquids could be measured. Two aqueous solutions in which the anion was the same were also tried, one combination being cupric chloride and common salt, and in this case the line of demarcation traversed about 7 inches in three hours. The results hitherto obtained by this method agreed fairly with those found by Kohlrausch.—On the resistance of some mercury standards, by Mr. R. T. Glazebrook, F.R.S. In 1885, M. Benoit, of Paris, supplied the author with three mercury standards, nominally representing the Paris Congress ohm, now commonly known as the legal ohm. Tests of these standards were described in a paper read before the Physical Society in 1885 by the present author. Recently he had occasion to compare two of the standards with the British Association coils. The mean of many concordant results gave the resistance of one of the mercury standards (No. 37) as 1·01106 B.A.U., whilst that of the other (No. 39) was 1·01032 B.A.U. Expressing them in legal ohms the present resistances are (No. 37) 0·99986 and (No. 39) 0·99913, whilst in 1885 the values obtained were (No. 37) 0·99990 and (No. 39) 0·99917. This shows that within the limits of experimental error the ratios of the mercury standards to the B.A. coils have remained practically unchanged during six years. The numbers given above are based on Lord Rayleigh's determination of the specific resistance of mercury, which differs appreciably from that found by Mascart and other observers. Taking the mean of the later concordant determinations, the values of the mercury standards expressed in legal ohms become (No. 37) 1·00033 and (No. 39) 0·99959. The values given by the maker were 1·00045 and 0·99954 respectively, showing a very close agreement. The author also found that refilling No. 37 from the same sample of mercury produced no appreciable change in its resistance, whilst No. 39 was somewhat affected by a similar operation. Experiments on the co-efficient of increase of resistance of mercury with temperature gave the value 0·000872 as the mean coefficient between 0° and 10° C., a number rather less than that obtained by Kohlrausch.—On an apparatus for measuring the compressibility of liquids, by Mr. S. Skinner. The apparatus consisted of a large spherical flask, with a long narrow neck containing the liquid to be experimented upon, the lower part of which was in communication through a stopcock and flexible tube with an adjustable reservoir. By raising or lowering the latter the flask could be easily filled or emptied or the quantity of liquid adjusted. The flask was inclosed in a bell jar, whose interior was in communication with a pump and barometer gauge. So sensitive was the arrangement that the compression of water produced by blowing into the jar caused the liquid to descend about 1 centimetre in the neck of the flask. This movement corresponded with a change of volume of about half a millionth. The coefficient of compressibility had been tested at different temperatures, and the results were not very different from those obtained by Tait and others. The influence of salts in solution in changing the compressibility had also been tested, and a great difference in this respect found between electrolytes and non-electrolytes.—Some measurements with the pneumatic bridge, by Mr. W. N. Shaw. The action of the apparatus is analogous in many respects to the Wheatstone's bridge, and its object is to compare the pneumatic resistances or conductivities of various orifices, channels, tubes, &c. The proportional arms are represented by two circular holes in thin plates of mica, the third arm by an aperture provided with a sliding shutter adjustable by a screw, and the fourth might consist of any aperture or tube whose conductivity was to be determined. The several apertures are pneumatically connected by large wooden boxes. The battery takes the form of a Bunsen burner with a long chimney, whilst the galvanometer is represented by a glass tube connecting opposite chambers, and containing a vane which sets itself at right angles to the tube when no air current is passing. The apparatus is remarkably sensitive to movements of the shutter, and on starting or stopping the draught after balance had been obtained, effects analogous to those produced by self-induction are observed. By its use it has been found that bevelling off one side of a hole in a thin plate increases the pneumatic conductivity of the aperture very considerably, particularly when the bevel is on the egress side. Another interesting result is that for square-ended tubes of given size the conductivity first increases as the length is made greater, and afterwards diminishes with further increase of length. Putting a flange on the outlet end reduces the anomalous effect, whilst a bevelled mouthpiece similarly placed causes it to disappear. In the discussion on Prof. Thomson's paper, Prof. Fitzgerald said the beautiful experiments were likely to lead to very important results. He did not quite understand how placing a fine wire in a vacuum tube could prevent the luminous discharge, for if the wire was on the side remote from the primary, and if there was any great increase in specific inductive capacity, he would have expected the air to screen the wire. Prof. Lodge asked for further information as to the action of the magnet in preventing the after-glow, and in some cases precipitating a luminous discharge. The experiment with the exhausted bulb within the bell jar was also difficult to understand, and he did not see why one of Prof. Thomson's two suppositions must necessarily be true. The President inquired whether Prof. Thomson had tried Mr. Crookes's experiment, in which the electric pressure necessary to produce a discharge was greatly lessened by putting a phosphorescent material in the tube. Prof. Thomson, in reply, said he had not tried the experiment, but the phosphorescence he had observed was of quite a different character from that produced in Mr. Crookes's tubes. To Prof. Fitzgerald he said the action of the wire was probably a question of time, and thought the whole field was in some way thrown on the wire and thus discharged. In reply to Prof. Lodge, he had not ascertained the true nature of the effect of a magnet on the glow, but he believed the glow to be due to a combination which might be prevented or facilitated by the action of the magnet causing the density to be different in different parts of the bulb. M. Guillaume, in discussing Mr. Skinner's paper, described the methods used by Sabine, Jamin, and others, in determining the compressibility of liquids, and pointed out their defects. The chief difficulty in such experiments was in finding the compressibility of the reservoir. Numbers expressing the compressibility of mercury obtained by different observers were given, the best values varying between 0·0000039 and 0·0000040.—On the motion of Prof. Ayrton, seconded by Prof. Rücker, a hearty vote of thanks was accorded to the authors for their valuable and interesting communications, and for the kind manner in which the Society had been received and entertained by the Cambridge members. Prof. Thomson and Mr. Glazebrook acknowledged the vote.

Geological Society, May 6.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read:—On a Rhætic section at Pylle Hill or Totter Down Bristol, by E. Wilson. In a deep railway-cutting at Pylle Hill, the Rhætic beds, having a thickness of not more than seventeen feet, are exposed between the Tea-Green Marls and the Lower Lias. There is no doubt as to the division between the Rhætic and Keuper beds in this section, but the line of demarcation between the Rhætic and the Lias has always been a matter of uncertainty in the West of England. In connection with this subject the term "White Lias," as applied to beds some of which are Rhætic and others Liassic, is held to be unsatisfactory. The author takes a limestone which is the equivalent of the Cotham Marble as the highest Rhætic bed in the section described. He divides the Rhætic beds of the cutting into an Upper Rhætic series and *Avicula contorta* Shales. The intimate connection betwixt the Tea-Green Marls and the Red Marls of the Upper Keuper is well displayed, whilst there is a sharp line of demarcation between the former and the *Avicula contorta* Shales. Most of the characteristic fossils of the British Rhætic are met with at Pylle Hill, together with a few forms which are new to England, and some of these possibly to science. A detailed section of the subdivisions of the Rhætic and adjacent beds, and a list of Rhætic fossils found in the section are given by the author. After the reading of the paper some remarks were made by Mr. Etheridge, Mr. H. B. Woodward, the Rev. H. Winwood, and Prof. T. Rupert Jones.—A microscopic study of the Inferior Oolite of the Cotteswold Hills, including the residues insoluble in hydrochloric acid, by Edward Wethered. The author gives the following main divisions of the Inferior Oolite of the Cotteswold Hills in descending order :—

Ragstones.
Upper Freestones.
Oolitic Marl.
Lower Freestones.
Pea Grit.
Transition Beds resting on Upper Lias.

The strata are described, and the results of microscopic examination of the different beds given. These latter confirm the author's views as to the important part which *Girvanella* have taken in the formation of oolitic granules ; whilst an examination of the borings referred to by Prof. Judd in the discussion of Mr. Strahan's paper "On a Phosphatic Chalk," convinces the author that these have no connection with the genus *Girvanella*. In the second part of the paper the insoluble residues left after treating the various deposits with acid are considered. They contain chiefly detrital quartz, felspars, zircons, tourmaline, chips of garnet, and occasionally rutile. In the argillaceous beds silicate of aluminia was found to occur plentifully. The detrital material is considered to be due to denudation of crystalline felspathic rocks, and not of stratified ones. This view seems to be supported by the quantity of felspar and its good state of preservation. The paper concludes with a consideration of the quantity of residue and the size of the quartz-grains in the different deposits, which are summarized in the following table :—

	Percentage of residue.	Size of quartz-grains, in millim.
Ragstones	2·8 ...	·17
Upper Freestones ...	1·1 ...	·12
Oolitic Marl	3·2 ...	·09
Lower Freestones ...	1·8 ...	·13
Pea Grit Series	5·0 ...	·14
Transition Beds	38·3 ...	·13

This shows a great falling off in the percentage of residue above the Transition Beds. That of the Freestones is remarkably low, and it would appear that these rocks were formed under conditions which allowed of very little sediment being deposited. The paper gave rise to a discussion, in which Prof. Hull, Mr. Etheridge, Mr. H. B. Woodward, the Rev. H. Winwood, and the author took part.

Royal Meteorological Society, May 20.—Mr. Baldwin Latham, President, in the chair.—The following papers were read :—On the vertical circulation of the atmosphere in relation to the formation of storms, by Mr. W. H. Dines. After giving an outline of the circulation of the atmosphere, the author refers to the two theories which have been suggested to account for the formation of storms, viz. (1) the convection theory, which is that the central air rises in consequence of its greater relative warmth, this warmth being produced by the latent heat set free by condensation ; and (2) the theory that the storms are circular eddies produced by the general motion of the atmosphere as a whole, just as small water eddies are formed in a flowing stream of water. The author is of opinion that the convection theory is the more probable of the two, but more information about the temperature of the upper air is greatly needed.—On Brocken spectres in a London fog, by Mr. A. W. Clayden. During the dense fogs in February last, the author made a number of experiments with the view of raising his own spectre. This he ultimately succeeded in accomplishing by placing a steady limelight a few feet behind his head, when his shadow was projected on the fog. He then made some careful measurements of the size and distance of the spectre, and also succeeded in taking some photographs of the phenomenon.—An account of the "Leste," or hot wind of Madeira, by Dr. H. Coupland Taylor. The "Leste" is a very dry and parching wind, sometimes very hot, blowing over the island from the E.N.E. or E.S.E., and corresponds to the sirocco of Algeria, or the hot north winds from the deserts of the interior experienced in Southern Australia. During its prevalence a thin haze extends over the land, and gradually thickens out at sea until the horizon is completely hidden. It is most frequent during the months of July, August, and September, and usually lasts for about three days.—Mr. Shelford Bidwell, F.R.S., exhibited an experiment showing the effect of an electrical discharge upon the condensation of steam. The shadow of a small jet of steam cast upon a white wall is, under ordinary conditions, of feeble intensity and of a neutral tint. But if the steam is electrified, the density of the shadow is at once greatly increased, and it assumes a peculiar orange-brown hue. The electrical discharge appears to promote coalescence of the exceedingly minute particles of water contained in the jet, thus forming drops large enough to obstruct the more refrangible rays of light. It is suggested that this experiment may help to explain the intense darkness, often tempered by a lurid yellow glow, which is characteristic of thunderclouds.

Linnean Society, April 16.—Prof. Stewart, President, in the chair.—A paper by the Rev. F. R. Wilson, was read, on lichens from Victoria, in which several new species were described, specimens of which were exhibited.—A paper by Surgeon-Major A. Barclay followed, on the life-history of two species of *Puccinia*, viz. *P. coronata*, Corda, and a new species which the author proposed to name *P. Jasmini-chrysopogonis*. A feature of peculiar interest noted in the latter species was the extraordinary abundance and wide distribution of the teleutosporic stage as compared with the comparative scarcity of the æcidial stage, and this disproportion in the distribution of the two stages had been remarked by the author long before he had ascertained that they were related.—A discussion followed, in which several of the botanists present took part.

May 7.—Prof. Stewart, President, in the chair.—Prof. R. J. Anderson exhibited a panoramic arrangement for displaying drawings at biological lectures.—Mr. John Young exhibited a nest of the Bearded Titmouse (*Calamophilus biarmicus*), which had been built in his aviary. Several eggs were laid, but none of them were hatched.—The Rev. E. S. Marshall exhibited several specimens of a Cochlearia from Ben More, believed to be undescribed.—Mr. Robert Deane forwarded for exhibition a plant of the Rayless Daisy, found growing abundantly in the neighbourhood of Cardiff ; and an undetermined Sponge, dredged in about 40 fathoms, off the coast of South Wales.—Mr. D. Morris drew attention to a Jamaica drift fruit recently found on the coast of Devonshire. Although figured so long ago as 1640 by Clusius, and subsequently noticed by other observers, the plant yielding it had only lately been identified by Mr. J. H. Hart, of Trinidad, as *Sacoglottis amazonica*. Mr. Morris likewise exhibited specimens of the fruit of *Catostemma fragrans*, received for the first time, from St. Vincent, showing its true position to be amongst the *Malvaceæ*, tribe *Bombaceæ*.—Mr. Thomas Christy exhibited some Kola nuts, and made remarks on the properties attributed to their medicinal use.—A paper was then read by Mr. Malcolm Lawrie, on the anatomy of the genera *Pterygotus* and *Slimonia*, and their relationship to recent *Arachnida*. An interesting discussion followed, in which the President, Prof. Howes, Dr. H. Woodward, and others took part.

Entomological Society, May 6.—Mr. Frederick DuCane Godman, F.R.S., President, in the chair.—Dr. D. Sharp exhibited a number of eggs of *Dytiscus marginalis* laid on the sheath of a species of reed, and commented on the manner of their oviposition, which he said had been fully described by Dr. Régimbart.—The Rev. A. E. Eaton exhibited a collection of *Psychodidæ* from Somersetshire, including six species of *Psychoda*, eleven species of *Pericoma*, and one species of *Ulomyia*. Mr. McLachlan commented on the interesting nature of the exhibition.—Mr. P. Crowley exhibited a specimen of *Prothoë caledonia*, a very handsome butterfly from Perak ; and a specimen of another equally handsome species of the same genus from Tonghou, Burmah, which was said to be undescribed.—Mr. H. Goss, the Secretary, read a letter from Mr. Merrifield, pointing out that the statement made by Mr. Fenn, at the meeting of the Society on April 1 last, of his views on the effects of temperature in causing variation in Lepidoptera, was incorrect ; he (Mr. Merrifield) had never suggested what might happen to *Tæniocampa instabilis*, and had expressly stated that he had found a reduction of the temperature below 57° to produce no effect, whereas in Mr. Fenn's experiments the temperature must have been below 40°.—The Secretary also read a letter which Lord Walsingham had received from Sir Arthur Blackwood, the Secretary of the Post Office, in answer to the memorial which, on behalf of the Society, had been submitted to the Postmaster-General, asking that small parcels containing scientific specimens might be sent to places abroad at the reduced rates of postage applicable to packets of *bonâ fide* trade patterns and samples. The letter intimated that, so far as the English Post Office was concerned, scientific specimens sent by sample post to places abroad would not be stopped in future.

Mathematical Society, May 14.—Prof. Greenhill, F.R.S., President, in the chair.—The following communications were made:—Relations between the divisors of the first n numbers, by Dr. Glaisher, F.R.S.—Wave motion in a heterogeneous heavy liquid, by Mr. Love.—Disturbance produced by an element of a plane wave of sound or light, by Mr. Basset, F.R.S.—On functions determined from their discontinuities and a certain form of boundary condition, and on a certain Riemann's surface, by Prof. W. Burnside.—Messrs. MacMahon, Larmor, Bryan, and the President took part in the discussions on the papers.

CAMBRIDGE.

Philosophical Society, May 4.—Prof. G. H. Darwin, President, in the chair.—The following communications were made:—The most general type of electrical waves in dielectric media that is consistent with ascertained laws, by Mr. J. Larmor.—A mechanical representation of a vibrating electrical system and its radiation, by Mr. J. Larmor.—The theory of discontinuous fluid motion in two dimensions, by Mr. A. E. H. Love. The paper contains an account of a modification of Mr. Michell's method. It is shown that, in all problems where the fixed boundaries consist of parts of *straight* lines, a figure can be constructed whose conformable representation upon a half plane gives rise to the equation of transformation which contains in itself the solution of the problem. The relation by which the representation is effected can in each problem be determined by known methods. The whole subject is thus reduced to integral calculus. Several new cases of the resistance offered by obstacles to the motion of fluids are solved. These include the determination of the mean pressure on a disk with an elevated rim, and of the mean pressure on a pier or other obstruction in a canal of finite breadth.—On thin rotating isotropic disks, by Mr. C. Chree. The subject treated is that of the rotation about their axes of thin disks whose section parallel to the plane faces consists of a circle or the area between two concentric circles. The paper aims at providing a solution which is not open to the objections recently urged by Prof. Pearson in NATURE against previous solutions.

PARIS.

Academy of Sciences, May 19.—M. Duchartre in the chair.—Determination of the constant of aberration; numerical values deduced from two groups of four stars, by MM. Lœwy and Puiseux.—On the transit of Mercury, by M. J. Janssen. It is remarked that a conclusive confirmation of the solar origin of the corona would be obtained if Mercury were photographed when at a short distance from the edge of the sun, and appeared in the negative projected upon a luminous background.—On the physical explanation of fluidity, by M. Boussinesq.—The heat of combustion and formation of some chlorine compounds, by MM. Berthelot and Matignon. The experiments indicate that for each equivalent of hydrogen replaced by chlorine in a series of compounds from 30 to 32 calories is disengaged. Cl_n substituted for H_n thus disengages about $30n$ calories.—On a double halo with parhelia observed on May 15, 1891, by M. A. Cornu.—On a memoir, by Herr W. von Bezold, relative to the theory of cyclones, by M. Faye.—Remarks on the employment of carbon bisulphide in the treatment of phylloxerous vines, by M. A. F. Marion and G. Gastine.—On the intermediate integrals of equations from derived partials of the second order, by M. E. Goursat.—On an elementary method of establishing differential equations of which θ functions form the integral, by M. F. Caspary.—On a class of complex numbers, by M. André Markoff.—Quantitative studies of the chemical action of light; Part iii. influence of dilution, by M. Georges Lemoine. Experiments with mixtures of oxalic acid and ferric chloride taken in equivalent proportions but with different quantities of water indicate that the chemical action of light upon them increases with the excess of water. The action of heat upon the mixtures appears to follow the same laws as that of light.—Calculation of the temperatures of fusion and ebullition of normal paraffins, by M. G. Hinrichs. A comparison is given of the observed and calculated melting and boiling points of the normal paraffins. The method of calculation is contained in *Comptes rendus*, May 4, 1891.—On the action exercised by alkaline bases on the solubility of alkaline salts, by M. Engel.—On the detection of silica in the presence of iron, by M. Leclere.—On the constitution and heat of formation of bibasic erythrates, by M. de Forcrand.—Thermal data relative to propionic acid and the propionates of potash and soda, by M. G. Massol. Facts are stated which prove that propionic acid, in combining with potash or soda, disengages as much heat as its superior and inferior homologues, acetic and butyric acids.—On the heat of dissolution and the solubility of some organic acids in methyl-, ethyl- and propyl-alcohols, by M. Timofeiew. The results indicate that there is a relation between the molecular solubility and heat of dissolution, the variation of molecular solubility carrying with it a variation, in the opposite sense, of the heat of dissolution.—Action of chlorides of bibasic acids on cyanacetic ethers, by M. P. T. Muller.—On the formation of nitrates in the earth, by M. A. Muntz.—Considerations of abysmal waters, by M. J. Thoulet.—On the genus Royena of the family Ebenaceæ, by M. Paul Parmentier.—On an inferior Basidiomycete parasite of grapes, by MM. Pierre Viala and G. Boyer.—On a particular appearance of the Cretaceous formation in the Bou-Thaleb group, Algeria, by M. E. Ficheur.—A bed of nephritis found in China, in the Nan Chan mountain-chain, by M. Martin.—Correction to a note on a recently described fossil, by M. Stanislas Meunier.—Discovery of a human skeleton contemporary with the Quaternary volcanic eruptions of Gravenoire (Puy-de-Dôme), by MM. Paul Girod and Paul Gautier.—Chemical and physiological researches on microbic secretions; transformation and elimination of organic matter by the pyocyanic bacillus, by MM. A. Arnaud and A. Charrin.

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THURSDAY, JUNE 4, 1891.

THE BRITISH INSTITUTE OF PREVENTIVE MEDICINE.

THE progress of bacteriological science, and the amount of exact information which it has shed upon the problems of disease during the last fifteen years, have led several of the Governments of the Continent and America to establish institutes providing for original research, as well as technical instruction, in preventive medicine.

This country, on the other hand, which pioneered sanitary science from its birth, has, strangely enough, been distinctly behindhand in the study of bacteriology (fraught as it is with interest of such vital importance to the health and prosperity of the nation); and of the provision of institutes of the kind which have been established abroad, such as the Pasteur Institute in Paris, the Hygienische Institut in Berlin, Königsberg, Breslau, Wiesbaden, St. Petersburg, Moscow, Odessa, Tiflis, Warsaw, Cracow, Naples, Turin, Rome, Milan, Palermo, Malta, Barcelona, Constantinople, Bucharest, Budapest, Rio Janeiro, New York, Washington, we have no example in the United Kingdom. In these institutions, the study of the morphology, biology, physiology, and chemistry of micro-organisms, whether pathogenic or not, is being actively pushed forward, and a thorough analysis of their subtle influence as causative factors of disease pursued.

In this manner the poisons of the following maladies, the effects of which are among the direst evils to humanity, viz. pyæmia, anthrax, erysipelas, septicæmia, glanders, tubercle, diphtheria, &c., have been isolated, and discovered to be micro-organisms which are now known certainly to be the active principle of the virus. When we reflect that, for centuries and centuries, the crippling effects of epidemic and devastating diseases have been only too well known, but attributed to the operation of all manner of causes, *e.g.* supernatural agencies, Divine wrath, meteorological and climatic influences, &c., &c., the fact that the real truth concerning the nature of their causes has been ascertained only within the last few years by laboratory research is, in itself, overwhelmingly expressive of the immense value of Bacteriological Institutes and their work.

But their value does not stop here. Knowing, as thanks to bacteriology we now do, the origin of these diseases, it may be asked what has the same science done towards stamping them out and preventing their development, or haply arresting their progress should they unfortunately gain access to, and invade, the tissues of the body. To express ourselves more plainly, the question might be put in this form, "What has bacteriological science done to discover the antidotes of such poisons?" The answer is, that whereas centuries of clinical observation have done very little indeed—by watching the sick and the employment of drugs—towards the direct arrest of the virus of infective maladies, laboratory work, on the other hand, has already provided us, not merely with many invaluable and additional facts to general science on the subject of immunity, vaccination, *i.e.* protection before infection, resistance of tissues to invasion by parasitic organisms, &c.; but has given to medical science, what no pharmacopœia has ever been able to do—namely, chemical antidotes which by their specific action upon the virus of diseases alone successfully save human beings as well as the lower animals from death and incapacitating illness.

Of these new methods, perhaps the most noteworthy is Pasteur's treatment of hydrophobia, but others have been already discovered, and are being examined and tested for practical employment in medicine and surgery.

A large institute of this kind, however, is not reserved solely for the investigation of the problems of disease—on the contrary, it has a far wider sphere of usefulness. Bacteriology, which Pasteur showed was the key to the secrets of fermentation, is, of necessity, all-important to many very extensive trades and commercial undertakings. The botanical and biological researches of the Pasteur Institute are thus to a large extent utilized by the French manufacturers, as well as by those of other countries, to their great profit.

The particular bearing of this branch of science has never been fully comprehended by the public, who are not aware what an enormous debt of obligation they owe to M. Pasteur, and to the extension of scientific research, which received its impetus from his genius, and which has resulted in so much direct gain and benefit to the community. In like manner, to agriculture, the questions of changes in soils—such, for example, as nitrification, now known to be due to the action of micro-organisms—are not less important, and indeed essential. A Bacteriological Institute, therefore, has in agriculture, quite apart from the subject of diseases of animals, a fertile source of work of the utmost value and assistance to practical men. But, in addition, there has of later years arisen a branch of chemical industry directed towards the synthetic production of numerous substances which prove to be powerful drugs. The knowledge of these is, of course, incomplete and dangerous until thorough experimental investigation of the action of these substances has been made. In this country, however, our chemists are precluded, by the harassing legislation under which their co-workers in physiology, pathology, and medicine labour, from pursuing this useful line of research, without great trouble and endless restrictions, although such work is solely directed towards the therapeutic relief of disease and suffering.

The chemistry of disinfection offers in itself an extensive field of research which can alone be cultivated in an institution of this kind reserved for bacteriological investigations.

Lastly, in such an institute two subjects of general interest receive special careful attention. These are (1) the technical instruction of medical men, health officers, chemists, and manufacturers, in bacteriology, both in its morphological and biological aspects; and (2) the examination of tissues and substances suspected to be the seat or vehicle of infectious diseases and submitted for investigation and report. The functions of a Bacteriological Institute, therefore, clearly involve interests of the highest national as well as particular or individual import.

Since the formation of the Pasteur Mansion House

Fund, which has provided for the treatment in Paris of many English sufferers from the bites of rabid dogs, some of the members of the Committee of that Fund, as well as of the Mansion House meeting at which it was inaugurated, knowing the importance to the community of having a similar institute in Great Britain, determined to make an effort to establish the same.

A survey of the conditions under which bacteriology is practised in Great Britain is sufficient to show at once the pressing need of creating a centre of the kind, since, although several medical schools and Universities have provided for the teaching of bacteriology to a degree suitable for diplomas in public health medicine, and although in the laboratories of the College of Physicians and Surgeons in Edinburgh, and of the conjoint London Colleges, besides those of University College, King's College, and the College of State Medicine, there is room and provision for a certain amount of original work, still it is quite notorious that the majority of original investigators are driven to go to Paris and Berlin, not only on account of the splendid collection of material and freedom of experiment there, but also for lack of sufficient accommodation in the laboratories of the United Kingdom. To remedy this state of things, and to provide an establishment which would greatly assist the medical schools and technical education generally, is therefore the object of the promoters of the British Institute of Preventive Medicine. The development of the scheme has now arrived at a very interesting point, which, as usual in this country, resolves itself into a contest between the friends and enemies of science. The object of the Institute being purely charitable and scientific, it was from the outset necessary to give its constitution a firm basis, in order to obtain the confidence of the public from whom naturally the cost of creating the Institute is to come. It has therefore to be incorporated, and such incorporation can practically only be obtained by permission of the Board of Trade, which grants leave for the registration of such institutes as limited companies, the word limited being omitted, thus insuring the appropriation of the funds for none but purposes identical with the original object for which they were intended. The Executive Committee of the British Institute, therefore, made through their solicitors, Messrs. Hunter and Haynes, the formal application for such registration to Sir Michael Hicks Beach, the President of the Board of Trade. To their surprise Sir Michael refused to register the Institute, and this without assigning in his letter any reason for his refusal. It is, however, understood that he has done so in consequence of his having received petitions from a few bodies of anti-vivisectionists, among whom are to be found as usual certain names, mostly ecclesiastical, of gentlemen whose intentions, however admirable, are dictated by absolute ignorance of the questions which they presume to discuss.

We understand (though it is incomprehensible how a Minister should have allowed himself to be placed in such a false position) that Sir Michael Hicks Beach alleges privately that by registering the Institute, a portion of the work of which will naturally include experiments on animals, he will be encroaching on the duties of the Home Office, to which department alone, however, as a matter of fact, is intrusted the administration of the utterly incompetent and harassing so-called Vivisection Act. Nothing can excuse the confusion of mind or ignorance which is thus displayed by an official of the Government, for, as is evident to the merest tyro in law, the question of experimental science has nothing whatever to do with the matter submitted to the Board of Trade. That body has only to make sure that the funds of the Institute cannot in the future be misappropriated to any other object. That is all it is asked to do, and that solely in the interests of the public.

The official seal of the Board of Trade having thus been given to stamp the Institute with the character designed for it by its promoters—namely, that of a charitable and not a commercial undertaking—it would then, of course, be necessary for the Executive Committee to apply to the Home Office for the registration of the Institute as a place where experimental science may be carried on.

With this second registration the Board of Trade has nothing whatever to do, and by taking upon himself the duty of considering this part of its constitution, the President has gone out of his way to raise difficulties in the formation by private individuals of a National Institute, which in other more intelligent and far-seeing countries the Governments have hastened to take the initiative in establishing and liberally supporting.

It is evident that Sir Michael Hicks Beach has been greatly misinformed on this matter, and we look forward with interest to the result of the representations of a very powerful deputation which we learn is to wait upon him on Friday, June 5, at 11 a.m., and which, constituted as it is of distinguished men in all branches of science, as well as of those of the general public who are interested in philanthropic sanitary measures, will point out to him the real facts of the case on which he has to adjudicate, and rescue the question from the erroneous position which it now occupies, owing to his unfortunate readiness to listen to the calumnious assertions of the haters of science and progress.

It is not difficult, we believe, to read between the lines in such a case as this. No beings are more human than Ministers and members of Parliament, or, in fact, all those whose own position or that of their party depends upon popular clamour. Such unfortunates listen like Eve with a fatal fascination to the voice of the deceiver, but, with a taste less worthy than hers, the fruit which attracts them is not that of the tree of universal knowledge, but of the ballot-box. They have hitherto laboured under the mistaken impression that an energetic and noisy group of agitators, leading in their train a few unscientific quasi-public men, were an important political body, and they consequently sacrifice to their misrepresentations the liberties of science and the good of commerce. The day is coming, or is rather come, when the scientific and cultured world will refuse to submit any longer to such a condition of affairs, and when all its branches, physiologists, agriculturists, chemists, engineers, medical and legal men, will unite in a compact body for the protection of their common interests, and we rather welcome the present difficulty, which has served to bring prominently forward the spirit animating them, and which no administrator will do wisely in failing to recognize.

THE GEOLOGY AND PHYSICAL GEOGRAPHY OF NORTH SYRIA.

Grundzüge der Geologie und physikalischen Geographie von Nord-Syrien. Von Dr. Max Blanckenhorn. Mit Zwei Karten, &c. (Berlin: Friedländer, 1891.)

IN this excellent treatise the author presents the reader with a synoptical view of the results of his observations over a region but little known; referring to his previous essays on the geology, palæontology, and petrology of North Syria for fuller details. The region described extends from the northern slopes of the Lebanon to those of the Taurus Mountains, and from the Mediterranean coast to the banks of the Euphrates and the ruins of Palmyra, embracing an area of about 45,000 square miles. It also includes the whole of the Orontes Valley and the Kurdish Mountains. The mountainous tracts immediately to the south have already been ably described, as regards their physical structure, by Carl Diener, in an essay which was favourably reviewed in NATURE at the time of its publication in 1886, and these observations on the geology of the Lebanon and Hermon have been taken up and extended by Dr. Blanckenhorn to the borders of Asia Minor. Still further south, we have the geology of Palestine illustrated and described by Fraas, Lartet, Tristram, and the officers of the Palestine Exploration Fund, extending into Edom and Moab and the Sinaitic peninsula; so that, as far as it is possible for travellers to carry out such a work as that of the geological portraiture of the region, we have now the whole tract from the shores of the Red Sea to the Taurus Mountains very fully described and illustrated. Two maps on a large scale, one showing the topography, the other the geology, accompany the present work. That there should be uncoloured spaces at intervals in the latter was inevitable, and is a proof of the caution exercised by the author in its preparation. The text itself also contains numerous geological sections and illustrations.

In comparing the geological structure of the Lebanon, as described by Diener,[1] with that of the range between the valley of the Orontes and the coast, called Djebel Ansârige (Nusairier-gebirge), the author observes that the representatives of the Upper Jura and Cenomanian lying at the base of the Lebanon formations are absent in the more northerly tracts, the lowest beds of the series being represented by the "Rudisten-kalk," of probably Turonian age. The engraved longitudinal section which the author gives to illustrate this, amongst other physical features, is drawn from the coast at Latakia (Lâdikije) over Dj. Hassan Erai to the Orontes at Mischalûm, and is of much interest as illustrating the general structure of this part of Northern Syria. The valley of the Orontes is shown to be in the line of a great fault, or system of faults, by which the Eocene limestone beds are "thrown down" along the eastern side of the valley against the older Cretaceous strata, which are elevated into the ranges of Dj. el Ansârije and Hassan Erai, capped by the same Eocene limestones which form the bed of the Orontes, but at a difference of relative level of about 1600 feet. On the eastern side of the valley the Eocene strata rise into high ridges, partly by the aid of a N.-S. fault, which is not improbably a continuation of the "great Jordan-Arabah fault," which has produced such remarkable effects in connection with the physical structure of Palestine and Arabia Petræa.[1] The position of this fault seems also to be indicated in the section across the Orontes at Hammâm Sheikh Isa, illustrating the region of Mons Cassius.

The author gives a graphic description of the gorge of the Orontes in the neighbourhood of the hot springs (Hammam) above the great bend which the river takes from its northerly course towards the west in order to reach the Mediterranean. At Djisr esh-Schughr the river enters a cañon which has been worn down to a depth of 160 metres in beds of Eocene limestone and marble rich in Nullipores, and amongst the massive Miocene limestone (Grobkalk); while to the left rises the plateau of Dj. el Koseir, breaking off in successive terraces towards the Orontes Valley, and on the right the crest of Dj. el 'Alâ. On leaving this gorge the river enters an extensive alluvial plain, making a magnificent sweep round to the westward; and in its course through a rocky and broken country bathes the ruined walls of Antioch, the once famous capital of Syria—a city which bears so honourable a place in the early history of Christianity.

The region of Northern Syria physically divides itself into three distinct regions which are adopted for purposes of description by the author. The first includes the coast ranges; the second, the depression lying to the east of these, including the valleys of the Orontes and the Kara sea and river; the third, the "Hinterland," or interior tracts of North Syria lying to the east of the depression, and including the Khurdish Mountains: we can only here specially notice this last. This region is remarkable for the great tracts of Miocene strata, reposing sometimes on those of Eocene, sometimes on those of Cretaceous, ages of the Palmyrene wilderness and of Anti-Lebanon, and which are in turn largely overspread by great sheets of plateau basalt. Of these Miocene strata the plains round Aleppo are chiefly formed. Here they are nearly horizontal, but towards the north they are tilted, and the Eocene and Cretaceous strata again rise to the surface and terminate in the escarpment of Kardalar Dagh, beyond which rises the high plateau of Kâwâr, and still further towards the north-west the lofty ridge of Giaur Dagh, which reaches an elevation of 1330 metres. This latter is formed of Devonian limestone, slate, and grit, which appear to be the fundamental rocks of this part of Syria. The plateau of Kâwâr, which intervenes between the Giaur Dagh and the Kurdish ranges, is formed of gabbro, norite, schillerfels, and serpentine, of an age intervening between the Upper Chalk and the Eocene. The Miocene strata which occupy so extensive a part of Northern Syria were formed, according to the author, under the waters of an arm of the Mediterranean, which extended inwards at the base of Dj. el-Koseir beyond the Kuweik and the vicinity of Aleppo, bounded by irregular ranges of emergent hills of Eocene and Cretaceous strata. The formation consists of basal conglomerates of flint pebbles, passing into calcareous sands, clays, and finally the massive limestone (Grobkalk) already referred to, and has yielded forms of *Operculina*, *Clypeaster*, &c., clearly indicating its marine origin. This epoch was remarkable for the display of volcanic energy on a vast scale. Great sheets of augitic lava, together with tuff and agglomerate, were erupted during the Miocene epoch, not only in Northern Syria but in the East Jordanic region to the south, and were again renewed in Post-Pliocene times. It is probable that to volcanic action we must refer the origin of some of the peculiar little lakes of Northern Syria, such as those of Homs and Kara, one occupying the bed of the Orontes, the other that of the Kara, where the ground probably fell in and became filled with water. The Pliocene period is represented by both marine and freshwater strata, deposited in bays and depressions along the margins of uprising lands, formed of all the older formations, including those of the Miocene period. All of these had been disturbed, upraised, and partially eroded before the deposition of the Pliocene strata. In this, as in other physical phenomena of Northern Syria, we are reminded of those of Palestine and Egypt. Throughout all this region the Nummulitic and Cretaceous strata were disturbed and upraised into dry land, and subjected to extensive denudation at the close of the Eocene and again at the close of the Miocene epochs, so that the stratigraphical continuity of these Tertiary formations has been repeatedly broken.

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[1] "Libanon, Grundlinien der phys. Geographie u. Geologie v. Mittelsyrien," 1886.

[1] "Mem. on the Physical Geology and Geography of Arabia Petræa, Palestine, &c." (Palestine Exploration Fund), 1886, pp. 103-12.

It may be worth while, in conclusion, to glance at the points of analogy, as well as of difference, between the physical conditions of Syria and of the region to the south of the Lebanon. In Northern Syria, and along the ranges of the Taurus and Anti-Taurus, the fundamental rocks on which are superimposed the great calcareous formations of Cretaceous and Tertiary ages consist of Devonian schists, greywacke, and limestone,[1] together with masses of various igneous rock. In Southern Palestine and the Sinaitic peninsula, on the other hand, the fundamental rocks consist of granite, gneiss, various crystalline schists of Archæan age, traversed by innumerable dykes of hornblendic, augitic, and felspathic rock; surmounted at intervals by Lower Carboniferous beds; this is a remark able contrast. But a still greater, perhaps, is to be found at the next stage. All along the eastern border of the Jordan Valley, south of the Sea of Galilee, extending southwards along the table-land of Moab, Edom, and the Arabah Valley, as well as through the Sinaitic peninsula, and into Upper Egypt, the base of the Cretaceous series is represented by the Nubian sandstone,[2] a formation of great persistency, and interesting from an architectural point of view for its extensive use as a building-stone in the great structures of Ancient Egypt; as, for example, in the colossal figures of Amenophis in the plain of Thebes, as also in the temples and sepulchres of Petra. This formation appears to be altogether wanting north of the Lebanon, where, according to Herr Blanckenhorn, the Cretaceous strata of the Turonian stage are the lowest of the series.[3] The points of contrast, however, here terminate; for over the whole region from Upper Egypt and the Libyan Desert on the south to the Taurus Mountains on the north, a distance of 1000 miles and beyond, the Cretaceous and Eocene limestones were deposited, and formed part of the floor of the ancient ocean, the original limits of which it is hard to determine with any approach to accuracy.

At the close of the Eocene epoch this ocean bed was subjected to powerful movements. Large tracts, including the Libyan Desert and Egypt, Palestine and Syria, were elevated into dry land; while the strata were bent, folded, and faulted along lines ranging generally from north to south. To this period is to be referred the production of the great Jordan-Arabah fault, which has now been traced at intervals from the Gulf of Akabah to the valley of the Orontes, a distance of over 350 miles, while the main features, especially the mountains, had the outlines which they now present marked out. During the Miocene period, along with a partial re-submergence, volcanic action came into play over a region generally bounded by the Jordanic depression on the west, and extending from the Arabian Desert to the base of the Taurus, and the head waters of the Euphrates. In Northern Syria, extensive sheets of basaltic lava are found west of the Orontes Valley, as well as at Antioch, Aleppo, and other parts. At a later period, bordering on the present, fresh eruptions were added. The region we have been considering has its natural boundary towards the north in the Taurus range, where a system of E.–W. flexures take the place of those of the region to the south, where (as we have seen) the prevalent direction of the flexures is meridional.

EDWARD HULL.

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[1] As determined by Hamilton, Warington Smyth, Tchihatcheff, and others.

[2] Probably of Neocomian age.

[3] Representing those of the chalk-marl of England.

EUROPEAN BOTANY.

Plantæ Europeæ: enumeratio systematica et synonymica plantarum phanerogamicarum in Europa sponte crescentium vel mere inquilinarum. Autore K. Richter. Tomus I., pp. 378. (Leipzig: Verlag von Wilhelm Engelmann, 1890.)

WHAT is most wanted in systematic botany at the present time is a general flora of Europe, worked out for the different countries on one uniform plan, with the sub-species and varieties placed in their proper subordination under the primary specific types, and the synonyms worked out carefully. The number of plants in Europe is about the same as in the United States. For these Asa Gray planned a general flora in three volumes, of which the middle one, containing the Gamopetalæ, was published shortly before his death, and the first and third left in a forward state of preparation. Many years ago Mr. Bentham planned and carried out, with the assistance of Baron von Mueller, a complete flora of Australia. There are 40 or 50 per cent. more plants in India than in Europe. Sir Joseph Hooker's "Flora of British India," containing descriptions and full synonymy of every species, has reached the end of the Dicotyledons, and in the last part the Orchideæ are finished, so that five-sixths of the work is now done. There is, however, no such book in existence as a general descriptive flora of Europe. For Europe the difficulty lies far more in the bibliography than in the plants themselves. An enormous number of subordinate forms have been described under specific names, and the number of channels of publication in the way of journals and reports of societies becomes greater and greater every year. Nyman's "Sylloge," published in 1854–55, and his later "Conspectus," have been a great boon to all European workers. Though they do not contain any descriptions, they give a tabular view of the whole European flora, tracing out in detail the geographical distribution of the species; and in the "Conspectus" especially, great pains has been taken to separate the subordinate from the primary types. The present work, like Nyman's, does not contain any descriptions. It deals with the geographical range of the species much more briefly, indicating it within the compass of a single line. Its strong point is bibliography, and it gives under species a list of all the names that have been applied to it by different authors, with a citation of the book and page where each name is published, with a note of the date of publication. The plan followed can be best illustrated by an example, and the following is the way in which the cultivated wheats are dealt with:—

TRITICUM, Section *Sitopyros*.

19. *T. monococcum*, L., Sp. Pl., edit. 1, p. 86 (1753).
Syn.: *Ægilops Crithodium*, Steud., Syn. Gl., i. p. 355 (1855).
Crithodium ægilopoides, Lk., in Linn., iv. p. 142 (1829).
T. bœticum, Bss., Diagn. Pl. Or., i. 13, p. 69 (1853).
T. pubescens, MB., Casp. M., p. 81 (1800).
Europa austro-orientalis (Ceterum cultum). (Caucasus.)

20. *T. sativum*, Lam., Enc., ii. p. 554 (1786).
(*a*) *Spelta*, L., Sp. Pl., ed. 1, p. 86 (1753.
Syn.: *T. Zea*, Host, Gram., iii. t. 29 (1805).
(*b*) *dicoccum*, Schrk., Baier. Fl., p. 389 (1789).
Syn.: *T. amyleum*, Ser., Mel. Bot., i. p. 124 (1818).
T. atratum, Host, Gram., iv. t. 8 (1809).
T. Cienfugos, Lag., El., p. 6 (1816).
T. Gærtnerianum, Lag., *ib.*
T. Spelta, Host, Gram., iii. t. 30 (1805).
T. tricoccum, Schuebl., in Flora, 1820, p. 458.
(*c*) *sativum*, Hack., in Nat. Pflzf., ii. 2, p. 85 (1887).
α. *vulgare*, Vill., Pl. Dauph., ii. p. 153 (1787).
Syn.: *T. æstivum*, L., Sp. Pl., ed. 1, p. 85 (1753).
T. cereale, Bmg., En., ii. p. 266 (1846).
T. hybernum, L., *l.c.*, p. 86.
β. *compactum*, Host, Gram., iv. t. 7 (1809).
Syn.: *T. velutinum*, Schübl., Diss., p. 13 (1818).
γ. *turgidum*, L., Sp. Pl., ed. 1, p. 86 (1753).
Syn.: *T. compositum*, Linn., f. Suppl., p. 477 (1781).
T. Linnæanum, Lag., El., p. 6 (1816).
δ. *aurum*, Desf., Fl. Atlant., i. p. 114 (1798).
Syn.: *T. Bauhini*, Lag., El., p. 6 (1816).
T. brachystachyum, Lag., *ib.*
T. cochleare, Lag., *ib.*
T. fastuosum, Lag., *ib.*
T. hordeiforme, Host, Gram., iv. t. 5 (1809).
T. platystachyum, Lag., *l.c.*
T. sativum β, Pers., Syn., i. p. 109 (1805).
T. tomentosum, Bayle-Bar., Mon., p. 40 (1809).
T. villosum, Host, Gram., iv. t. 6 (1809).
Cultum in diversis varietatibus.

21. *T. polonicum*, L., Sp. Pl., ed. 1, p. 86 (1753).
Syn.: *T. Cevallos*, Lag., El., p. 6 (1816).
Cultum.

Of course it is impossible for an author covering such a wide field to work out for himself all the details, and in the critical genera, such as Potamogeton, Festuca, Crocus, Iris, Tulipa, and Narcissus, no two authors are ever likely to agree as to which should be classed as primary, which as subordinate types, and which as mere synonyms. The present portion of the work includes only the Gymnosperms and Monocotyledons. The author admits 250 European genera, 1830 species, and 840 sub-species. He keeps up the oldest specific name published under any genus, not, as is usual in England, the name first published under the genus in which the plant is now placed. I find that a considerable number of books and papers published in England have not been taken into account; for instance, Maw's magnificent monograph of the genus Crocus, C. B. Clarke's monograph of the European species of Eleocharis in the *Journal of Botany*, 1887, p. 267, and Arthur Bennett's work on Potamogeton, as summarized in the last edition of Hooker's "Student's Flora." The book has cost great care and pains, and will be found very useful by all who work at European botany.

J. G. BAKER.

OUR BOOK SHELF.

The Missouri Botanical Garden. 8vo, with several Maps and Engravings. (Printed for private circulation by the Managers, 1891.)

THE Missouri Botanical Garden is situated at the city of St. Louis, and was founded by the late Henry Shaw. He was born at Sheffield in the year 1800, and emigrated to Canada with his father at the age of eighteen, and a year later moved southward to St. Louis, which was then a small isolated French trading post. He established himself in business as a dealer in cutlery, made a fortune of 250,000 dollars by the time he was forty years of age, and then retired from business. In 1840 he visited Europe for the first time, and in 1842–45 made a three years' tour in the Old World. In 1851 he visited Chatsworth, and particularly admired its garden and conservatories. This led him to entertain the idea of forming a large garden at home. One of the best American botanists, Dr. Engelmann, lived at St. Louis, and Mr. Shaw sought his help and advice. In 1857 he opened a correspondence with Sir William Hooker. He engaged from the Royal Botanic Garden in Regent's Park Mr. James Gurney to superintend the carrying out of his plans. He died in 1889, and bequeathed to his trustees 760 acres of land, situated partly within and partly outside the limits of the city of St. Louis, to be kept up as a Botanic Garden open to the public, containing a museum and library.

On the recommendation of Dr. Asa Gray, Mr. William Trelease, who was then Professor of Botany in the Wisconsin University at Madison, was appointed in 1885 Director of the Garden, a post which he still holds, and provision was made for the establishment of a school of botany and the endowment of six scholarships for garden pupils, each worth 300 dollars a year, with free lodging and free tuition.

The present volume contains a biographical sketch of the founder of the Gardens; a copy of his will; of the Act that was passed to enable him to convey the land to the trustees, and of the deed of gift for the endowment of the School of Botany; a copy of the inaugural address by Prof. Trelease, when the School of Botany was founded; also of the first annual report of the Director; of the proceedings at the first annual banquet of the trustees, to which a large number of eminent men of science and other guests were invited; and of the first annual flower sermon, which was preached in Christ Church Cathedral on May 18, 1890, by the Bishop of Missouri. The book is illustrated by plans of the garden, a large number of views of the museums and other buildings, including Mr. Shaw's house and a fine statue of Humboldt.

Everything is now in full working order, and we have just received from Prof. Trelease a capital synopsis of the American species of the difficult genus Epilobium, containing full botanical descriptions and figures of all the species. The herbarium now contains about 20,000 mounted sheets of flowering plants and ferns, also a large collection of Fungi and other Cryptogamia.

J. G. B.

Géologie: Principes—Explication de l'Époque Quaternaire sans Hypothèses. Par H. Hermite. Pp. 145. (Neuchatel, 1891.)

ON taking up this little book the geological reader is at once struck by the words "sans Hypothèses" in the title. A volume on Pleistocene geology free from hypotheses would seem to him to usher in a new era in geology, and would be most heartily welcomed by him. The title of the present work, however, is misleading; the book is almost entirely devoted to theoretical explanations of purely hypothetical facts. We have not space to notice in detail the various subjects of which the author treats, but as an example of his method we may point to his "Origine des Pluies Quaternaires" (p. 39). In this section he accepts the hypothetical Quaternary "Pluvial Period"—which, by the way, seems to have been characterized by a singularly poor aquatic fauna and flora—and he then accounts for the supposed excessive rainfall during Tertiary and Quaternary time by the amount of vapour thrown out by volcanoes, adding that the small rainfall of the Secondary periods is accounted for by the absence of volcanic action during those periods! Then we meet with our old acquaintance the former excess of carbonic acid in the air and its influence on the ancient climate of the polar regions—possibly correct, but certainly hypothetical. Further on, speaking of the origin of the continental platform at a depth of 200 mètres, the author states that this feature results from the raising of the general level of the sea from the melting of the Quaternary ice; and from this hypothetical raising he arrives at the result that the mass of the Quaternary ice corresponded to the total mass of the sea now lying above the level of the continental platform. Another speculation relates to the breaking through of the Indian Ocean across Siberia to the Polar seas, thus causing a milder climate, and accounting also for the parallel roads of Glen Roy and the terraces in Norway and Greenland. We cannot pretend to follow the reasoning, but it is all somehow connected with the author's theory "qu'à une diminution de la densité des mers correspond un abaissement de leur surface."

C. R.

Webster's International Dictionary of the English Language. Revised and Enlarged under the Supervision of Noah Porter, D.D., LL.D. (London: George Bell and Sons. Springfield, Mass. U.S.A.: G. and C. Merriam and Co.)

WEBSTER'S Dictionary is so well known on both sides of the Atlantic that it is unnecessary to do much more than note the appearance of the present edition. The work was published originally in 1828, after which it was steadily improved in successive issues. It has now been revised so thoroughly, and with the aid of so many competent scholars, that for popular use it can hardly fail to maintain the ground it has already won. Much prominence is given to "the definitions and illustrations of scientific, technological, and zoological terms," and in the preface to the English edition it is stated that no pains have been spared to make this part of the book "as perfect as possible in both text and illustration." The definitions in particular branches of science have been revised by such men as Prof. H. A. Newton and Prof. E. S. Dana—names which are a sufficient guarantee for the way in which the task has been accomplished. In the department of etymology, Prof. E. S. Sheldon, of Harvard University, has carefully dealt with the results presented in the last edition, bringing them into accord with the philological ideas of the present day. The pictorial illustrations are numerous, and well adapted to the purposes for which they are inserted.

Elementary Chemistry; for Beginners. By W. Jerome Harrison, F.G.S. (London: Blackie and Son, 1890.)

THIS volume of 144 pages consists of an expansion of the author's notes of lessons prepared for teaching children from nine to thirteen years of age according to the outlines given in the education code. The information is conveyed in familiar language, and each chapter closes with a series of questions which are well calculated to test the child's progress. It is a pity to issue any book that deals with scientific matters without a contents table and an index, and we fear that the absence of these in the present case will lead to inconvenience. And we would suggest that the quantities selected for the examples might approximate more closely to those most generally employed. The hydrogen from the use of a ton of zinc, the preparation of 1000 lbs. of carbon dioxide, eighteen quarts of oxygen mixed with an equal volume of hydrogen and exploded, ten gallons of hydrogen mixed with half its volume of chlorine and exposed to sunlight, indicate experiments on an extravagant if not an appalling scale. These, however, are matters of detail. The notes of so successful a teacher as Mr. Jerome Harrison cannot fail to be valuable to others who are engaged in a like work as well as to the students themselves.

Examination of Water for Sanitary and Technical Purposes. By Henry Leffmann, M.D., Ph.D., and William Beam, M.A. Second Edition. (London: Kegan Paul, Trench, Trübner and Co., Ltd., 1891.)

THE fact that a second edition has been called for only two years after the issue of the first, shows that this excellent hand-book has been very generally appreciated. The authors have revised the work and made many additions to it chiefly of processes that have recently grown in importance. Among the principal of these additions, we observe that the three pages on "Living Organisms in Water" of the first edition are now expanded into a chapter of thirteen pages entitled, "Biological Examinations." A table of culture phenomena of some of the more important microbes is given. But concerning this matter the authors state that "until pathogenic microbes are more clearly indicated and described, the methods will be of little use in dealing with the problem of the determination of the sanitary and technical value of water supplies."

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The University of London.

I DO not wish to criticize in the least Prof. Lankester's valuable statement in your last issue, with which I entirely agree; but I desire to point out that unless some energetic action is taken very soon we are likely to be farther than ever from the ideal which he has in view—namely, the establishment of a strong professorial University in London. The only scheme at present in the field is that put forward by the Councils of University and King's Colleges in the proposed charter for an Albert University.

This scheme has never met with the cordial support of a large section at least of the teaching staff of University College, and for the very obvious reason that it does not constitute a professorial University, but creates a new examining body on which the two Colleges will be, in the beginning at any rate, largely represented. The Albert University charter would create a second Victoria University in London. Now, both Mr. Dyer and Prof. Lankester are agreed that we do not want a federal University like Victoria in London; but they seem to forget that this pettifogging excuse for a University—a scheme drafted by bureaucratic rather than academic minds—is the only scheme in the field, and that, further, the Lord President of the Council has determined to hear by counsel, on an early day in June, what can be said for and against this scheme. It is further rumoured that the Burlington House Senate intends, after its recent discomfiture, to remain absolutely neutral. The danger, then, that we shall have a repetition in London of the difficulties of Manchester is a very immediate one. Let me point out exactly the anomalies of the Albert scheme. In the first place, it does not create a teaching University, but a new examining body. The University as such will have no control over the appointment of the professoriate either at University or King's Colleges, it will have no funds to dispose of, and there will be nothing to prevent rival second-rate teachers and teaching equipment instead of first-rate central teaching and central laboratories. For example, at the present time, putting aside the Central Institute, we have some half-dozen second-rate physical laboratories in London, but not a really first-class one worthy of a modern University among them. So long as there is competition between the Colleges, so long as they possess a double staff competing at every turn with each other for students' fees, this is unlikely to be remedied. Prof. Lankester speaks of a *union* of King's and University, and talks about their *combined* resources. The fusion of these two Colleges would certainly be the first stage to a true professorial University in London, but there is nothing in the Albert charter to bring this about: it unites the two Colleges not for teaching but for examining purposes. But what is still worse, while these two Colleges will remain autonomous, the Albert charter proposes to admit any further autonomous bodies, the teaching of which can be shown to have reached a certain academic standard. These bodies will not be absorbed, but their independent staffs will be represented on the Faculties and Senate. Here we have in fact the University of London over again,—at first composed almost entirely of the two Colleges, afterwards embracing all sorts and conditions of institutions in London, and ultimately open to every isolated text-book reader in the universe. It cannot be therefore too strongly insisted upon that the Albert charter, if granted, will not call into existence a professorial University, but federate a group, and an ever-widening group, of competing institutions for the purposes of *examination*. If it sheds for a time any additional lustre on the teaching staffs of the two Colleges—which I am much inclined to doubt—it will not achieve, what most of us have at heart, the establishment in London, at any rate in the germ, of a great University in the Scottish or German sense. A University, on the scale we hope for, would *absorb* the plant of University and King's Colleges, of the Royal College of Science, and of the Central Institute without the least difficulty. With the death or transference of existing teachers, whose pecuniary interests would have of course to be carefully safeguarded, special branches of higher teaching and research might be localized at these various centres,[1] and we thus might reach in the future an efficient University organization in London. This may indeed be considered a merely ideal future, but any scheme like the proposed Albert University, which will only impede its ultimate realization, ought to meet with strenuous opposition from those who believe that a great professorial University must sooner or later be established in London.

The difficulty as to the granting of medical degrees will for long be the stumbling-block of any scheme, but the true way to surmount it seems to be that suggested by Prof. Lankester—namely, the complete divorce of the clinical teaching at University and King's Colleges from the science teaching, and the establishment of separate clinical schools at the existing College hospitals on precisely the same footing with regard to the University as the other medical schools. The preliminary science teaching at the various medical schools might then be safely intrusted to University readers, who might continue to be, as they now largely are, peripatetic. These readers would naturally belong to the science faculty of the new University, and if largely paid by students' fees might be trusted to safeguard the "preliminary scientific interests" of the medical schools. It seems to me, therefore, that some vigorous effort ought to be made to obtain the modification of the Albert University scheme in the sense indicated by the following proposals:—

PROPOSALS *in re* TEACHING UNIVERSITY.

No scheme for the constitution of a teaching University in London will be satisfactory which does not:

1. Place the appointment of the teaching staff, as well as the control of laboratories, libraries, and buildings, in the hands of a single executive body, hereinafter spoken of as the new University Senate, or of bodies, such as Faculties or boards of study, to which it may delegate its powers.

2. Confer on the new University Senate the power of granting degrees in all Faculties, including that of Medicine.

3. Give to the teaching staff an immediate representation of one-third, and an ultimate representation of at least one-half, on the new University Senate.

These conditions would probably be best fulfilled by:

4. The immediate fusion of the Councils of University and King's Colleges, and the Council or Governing Body of any other institution doing work of admittedly academic character in London, which may be willing that its laboratories and equipment should be placed under the control of the new University Senate.

[This would remove any ground from the objection that the two Colleges are claiming powers which they are not willing to share with the Royal College of Science or the Central Institute. It provides for these latter coming into the scheme on the same terms, if that is possible.]

5. The granting of a Charter to a body consisting of these combined Councils together with representatives of the teachers in the combined institutions.

6. The constitution of the new University Senate in the following manner:—

A. Immediate constitution—
   (1) The fused Councils of King's and University Colleges or their representatives.
   (2) The Councils of other academic bodies in London willing to be absorbed, or their representatives.
   (3) Representatives of the teachers to the extent of one-third of the total number.

B. Ultimate constitution—
   (1) University professors, either as *ipso facto* members or as representatives of the body of professors.
   (2) Representatives of the Faculties (*i.e.* of the readers and professors of each Faculty).
   (3) Co-optated members, not to be selected from the teaching staff.

   And possibly,

   (4) Representatives of bodies willing to endow professorships in the new University, or to hand over to the control of the University existing professorships or lectureships, *e.g.* (*a*) the Corporation of the City and the Mercers' Company as trustees of Sir Thomas Gresham's estate; (*b*) the Inns of Court—provided these bodies are willing to attach the Gresham Lecturers and the Readerships instituted by the Council of Legal Education to the new University.
   (5) Representatives of the Medical Schools and Royal Colleges of Physicians and Surgeons other than those selected by the Medical Faculty. This would only be a matter for consideration when the power to grant medical degrees became actual.

7. The transition from the immediate to the ultimate constitution of the new University Senate in the following manner:—

   (*a*) By not filling up vacancies among the members contributed to the new Senate by the existing College Councils as such occur.
   (*b*) By the increase of professorial members and representatives of the Faculties.

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[1] *Elementary* teaching in many branches might for local convenience be still carried on at several centres.

8. The suspension of the power to grant medical degrees until such time as the Senate of the new University shall have satisfied the Lord President of the Council that an agreement has been reached with the Royal Colleges and the chief London Medical Schools as to the terms on which medical degrees shall be granted.

9. Providing, on the repeal of the Acts of Incorporation of University and King's Colleges which would accompany the granting of the new Charter, special regulations for the control of certain portions of the endowments or of certain branches of the College teaching, which it may not seem possible or advisable at present to hand over without special conditions to the management of the new Senate. For example, the Department of Divinity at King's College.

10. Paying due regard to the pecuniary interests of existing teachers (many of whom depend entirely upon students' fees) in the appointment of future University professors or readers.

11. Offering those professors of the existing Colleges, who might be willing to surrender the title of College professor, that of University reader, but not creating the occupants of chairs in any of the existing Colleges *ipso facto* professors in the new University.

In this mere sketch I have said nothing as to how faculties and boards of study might be constituted or as to how the University should grant degrees, for these seem to me "academical" problems, *i.e.* problems to be thrashed out by the University itself when it is once incorporated. Objection will be taken to much of the above by many individuals, but I believe it foreshadows the direction in which the *only scheme at present under discussion* must be modified if it is to lead to the ultimate establishment of a great teaching University in London, and not to a mere organization of teachers for examination purposes.

KARL PEARSON.

IT seems to me that the force of the arguments of Profs. Lankester and Ramsay in last week's NATURE (May 28, pp. 76, 78), so far as they harmonize with each other, would have to be admitted, if the main object of a University were to foster that premature specialism, which, under the scholarship system, has already wrought great mischief to real education in this country, or to increase as far as possible the number of clever but half-educated specialists, with which a close acquaintance with any of the great scientific societies makes one only too familiar. The example of this has been well set by at least one of the great metropolitan day schools. The fatal weakness of the arguments referred to is that they ignore, as no University ought to do, the claims of general education. If the advancement of scientific research is really desired by University and King's Colleges, all they have to do is to institute on their own account a diploma of the nature of the Associateship of the Royal School of Mines or College of Science, and make the training for it so good and thorough that the possessors of such a diploma shall be such a *desideratum* in those "commercial" quarters to which Prof. Ramsay appeals as a sort of final authority, that they shall drive such creatures as B.Sc.'s out of the field. Special brain-power, highly developed, is no doubt a splendid thing in its way, and recognition of it in the field of science is fully provided for in the B.Sc. honours, and in the ultimate D.Sc. degree; but, in considering the terms on which a *degree* should be given, general education and culture cannot be left out of account. In Germany something of the sort is guaranteed by the examinations which have to be passed on leaving the gymnasium (or high school) before students proceed to the University to specialize; in England it has been found necessary to institute the matriculation examination. That need, however, is no longer so imperative as it was; and for my own part I see no real objection to the "leaving certificate" of the Oxford and Cambridge Examining Board being accepted in lieu thereof; for I speak of what I know, when I say that this carries with it a guarantee of as much education and culture as the Matriculation Examination does, and often a great deal more. I would only stipulate that it should include one modern language and one branch of science.

Prof. Ramsay has over-ridden his horse, by the emphatic preference he gives to a German degree. He is a comparatively young man; but some of us (who are not yet quite senile) can remember the time when the facilities for obtaining the German Ph.D. degree were such (they are such to this day in America) that the degree became a by-word and a reproach, and still carries with it suspicions altogether disadvantageous to those who have taken the genuine degree in Germany. This is surely a warning against the multiplication of small Universities in this country. Again, if the time-honoured Universities of Oxford and Cambridge are not proof against the temptation to swell the contents of the University chest by accepting fees for the silken degree of M.A., which in the eyes of the *vulgus* is supposed to represent higher intellectual attainments than the B.A., can we expect greater virtue in a small and brand-new University struggling to "make both ends meet"? Were any further illustration required of the way things would be likely to drift with small and independent degree-granting corporations, we might find it in the readiness with which the authorities of King's College threw over Latin two years ago in the mercantile department of their school (then in a state of depression), at the mere bidding of the Chambers of Commerce, although its retention had been advocated by two leading scientific men. The really inspiring motive of this agitation is, I think, astutely kept in the background. A. IRVING.

Wellington College, Berks, June 1.

ONE of the taunts most frequently levelled at the London University—or "Burlington Gardens," to use Prof. Lankester's favourite expression—by certain professors of University College and other advocates of a "teaching University in and for London" is, that the present University is a "mere examining board." The University has, it is true, a Brown Professor of Physiology and Pathology, who delivers annually a course of lectures relating to the studies and researches carried on at the Brown Institution. But this professorship is an exception, though the University, by accepting the Brown Trust, showed clearly enough that it did not recognize any obligation to abstain from appointing University Professors and Lecturers. We have been previously told that there was a "tacit understanding" at the foundation of the University that this should not be done. But Prof. Ray Lankester goes far beyond the assertion of a "tacit understanding." He talks of "pledges" given by the founders of the University being "falsified," and "most solemn obligations" violated—terrible crimes, which, however, have been committed already by the appointment of the Brown Professor. But how such "obligations" and "pledges," or even a "tacit understanding," could ever have existed, I fail altogether to see, for it was the expressed intention of the founders of the University that its powers and privileges should be the same as those of the Universities of Oxford and Cambridge. Testimony as to this pledge may be found in the evidence given before the recent Commission. The late Dr. Carpenter's view of this matter was stated by Mr. Dickins in his communication to NATURE. Convocation has, years ago, voted in favour of the establishment of University Professorships and Lectureships, though I do not in the least believe that the graduates would sanction any proposal involving that the University should prepare candidates for its examinations, or compete with the ordinary work of the Professors in University College and other similar institutions. Whether research is or is not carried on successfully at University College is a matter on which I express no opinion. But, however this may be, it should be remembered that the students of this College have become only a small fraction of the candidates for London degrees. It would be, it seems to me, in the public interest that the University should make provision for the encouragement and reward of those among the great majority of its members who show a capacity for research and a power to extend the boundaries of knowledge. That the University has only one solitary Professor is due, I believe, in great measure to the narrowminded and unwise jealousy of University College, and to the fear lest some endowments should chance to be diverted to the University.

Prof. Lankester abandons altogether the scheme set forth in the Draft Charter of the "Albert University of London." This Charter proposed the establishment of a University whose range of activity should extend over colleges or other institutions in an area with a diameter of thirty miles. Prof. Lankester's ideal University, which would still be federal, is to consist only of University and King's Colleges. These institutions have not as yet shown any disposition to amalgamate the one with the other, and such a disposition is not likely to arise. They are, in fact, founded on distinct principles. The motto of the one, if I recollect rightly, is *Cuncti adsint* and of the other *Sancte et sapienter*. Some time ago I heard of a Society of University

College students being compelled to meet elsewhere instead of in the College on account of there being something of a religious character connected with their meetings, while there are facts of a different character in the history of King's College which may be easily remembered. That a federal University consisting of institutions so dissimilar would work harmoniously I very much doubt. Probably the graduates of the existing University would care but little, except on general public grounds, about University and King's Colleges having power to grant degrees, if as a University they would assume a name not likely to be mistaken for that of the University of London. As yet the Victoria University is not a conspicuous success, and the London University examinations are still held at Owens College.

With the views set forth by Mr. Thiselton Dyer I should be disposed in great measure to agree, though there are some points on which I should have liked to make some remarks; but I fear, if I did so, I should trespass too far on your space.

London, May 29. THOMAS TYLER.

THOSE who have taken part in the interesting discussion on the University of London, in your columns, have all viewed the subject from the academic standpoint. Would it not be well to consider it also from another point of view, viz. that of the educational needs of London? Prof. Ramsay contends "that a University is primarily a place for the extension of the bounds of knowledge." It is surely more accurate to say that a University, under the conditions that now exist, has two main functions—the one the extension of the bounds of knowledge by research, and the other the wide diffusion of that knowledge. The purpose of such diffusion should be to afford, as far as possible, to every individual the opportunity of obtaining such a training as would qualify him or her to take part in the development of some branch of knowledge, or at any rate to follow with appreciation and interest the advance made by others.

It needs no argument to show that it would be for the advantage of research, and for the well-being of the community, that real University training should be as widespread as possible. Ability and bent for some special study may frequently not be developed until somewhat late in life, after a business career has been begun. There is scarcely a branch of science that does not owe much to investigators whose researches were carried on during hours spared from some bread-winning occupation. The late Prof. John Morris was in early life a chemist in the Borough; Dr. James Croll was for years the janitor of the Andersonian University, Glasgow; even in the very number of NATURE containing Mr. Dyer's letter, the case of M. Rouault, one of the pioneers in the geology of Brittany, is mentioned, who did his early work while carrying on the business of hairdresser. A University training would have been of inestimable value to such students as these (and there are hundreds of such, with capacity for good work, scattered over London and the country), but no provision is made for them in our existing system.

Surely the important question therefore is, What kind of University would discharge most effectively for London the duty of providing for the needs of every class of students? The University should clearly recognize all organized teaching of University rank, whether given within the walls of a specified College or not. One of the most urgent needs of London is a co-ordinating head for all its multifarious higher educational agencies. The only University that will really adequately meet the needs and stir the enthusiasm of Londoners will be a University in vital relation with and directing and controlling all the higher teaching of the metropolis. This would, no doubt, be a new type of University, but the changed conditions of these times necessitate large modifications in the constitution of our institutions. This is sufficiently illustrated by the fact that the University of London itself was a new type of University, as also was the more recent Victoria University.

The new teaching University for London should have as its accredited professors and lecturers the staffs of University and King's Colleges, the Royal College of Science, the various medical schools, and any other institutions of equal rank, and in addition a large staff of lecturers at work in different parts of the metropolis at convenient centres. It would be possible, by an extension of the principle admitted into the draft scheme for the re-constitution of the University of London, viz. that of requiring from every University teacher a syllabus of his course of teaching, and further, by making such syllabus the basis of the examination, to incorporate all the work done by the accredited teachers of the University into its curriculum for degrees. This would make it possible to open up a University career to evening students. While day students would complete their course of study in three or four years, evening students would take nine or ten, and the curriculum could without serious difficulty be modified to meet the conditions.

May 30. R. D. ROBERTS.

I WOULD ask whether it is quite fair to assume that, because Convocation has rejected the Charter proposed for the University of London, it therefore follows that that body is out of sympathy with the attempts that are being made to establish a "real University," whatever that may mean. Is it not possible that a large proportion of those adverse votes were recorded because there were elements in the scheme which were felt to be impracticable or open to serious objection? At all events, I feel sure that there are many who would refrain from regarding the vote as being an expression on the main issue.

The views so well put forward by Prof. Ray Lankester as to the undesirability of establishing what he terms federal Universities fully enlist our sympathies; but are we not sailing very near the wind in the suggestion that University and King's Colleges and "other institutions" should be incorporated on University lines?

I say, by all means avoid centralization and beware of the "never-ending Committees and schedules of such clumsily-organized Universities." But what of value is then left that University College does not already possess? Would the appropriate definition and allotment of degrees of all shades and grades have contributed one iota to the work and influence of Graham, Sanderson, Sharpey, Foster, Williamson, and Prof. Lankester himself, or have added to the benefit they have conferred upon University College? One does not surely regard the granting of degrees as an important element in the German University: its distinguished professors are not Berlin men or Strassburg men—they are pupils of Liebig, of Wöhler, of Bunsen, and the like; and its students are not regarded as graduates of Heidelberg or Giessen, but in like manner as pupils of so-and-so. And University College is, I take it, much more nearly in function a German University now than ever it is likely to be as a federal University. I verily believe that such is the taste of the so-called properly ordered English mind for schemes, plans, and organizations, that a governing body, even though largely composed of the most uncrystallizable elements, would shortly be found carefully hedging itself round (and the students) with that beautiful machinery which Prof. Lankester so heartily detests. Prof. Ramsay's association of "examination on the brain" with the London University undergraduate I fear does the said undergraduate an injustice, if it is meant to differentiate him from his fellows of the "real Universities."

The men who regard the College Calendar with its traditional questions as their *vade mecum*, and whose only other study is the idiosyncrasies of the examiner, are ubiquitous, and their name is legion. If I could think they were confined to the "Burlington Gardens University," I, for one, would vote against the alteration of one jot or tittle of the present organization, if only lest they might be disturbed from their resting-place there. G. H. BAILEY.

May 30.

Quaternions and the Ausdehnungslehre.

PROF. GIBBS' second long letter was evidently written before he could have read my reply to the first. This is unfortunate, as it tends to confuse those third parties who may be interested in the question now raised. Of course that question is naturally confined to the invention of methods, for it would be preposterous to compare Grassmann with Hamilton as an analyst.

I have again read my article "Quaternions" in the *Encyc. Brit.*, and have consulted once more the authorities there referred to. I have not found anything which I should wish to *alter*. There is much, of course, which I should have liked to extend, had the Editor permitted. An article on Quaternions, rigorously limited to four pages, could obviously be no place for a discussion of Grassmann's scientific work, except in its bearings upon Hamilton's calculus. Moreover, had a similar article on the *Ausdehnungslehre* been asked of me, I should certainly have declined to undertake it. Since 1860, when I ceased to be a Professor of Mathematics, I have paid no special attention to general systems of *Sets*, *Matrices*, or *Algebras;* and without much further knowledge I should not attempt to write in any detail about such subjects. I may, however, call attention to the facts which follow; for they appear to be decisive of the question now raised. Cauchy (*Comptes Rendus*, 10/1/53) claimed *quaternia* as a special case of his "clefs algébriques." Grassmann, in turn, (*Comptes Rendus*, 17/4/54; and *Crelle*, 49) declared Cauchy's methods to be precisely those of the Ausdehnungslehre. But Hamilton (*Lectures*, Pref. p. (64), foot-note) says of the clefs algébriques (and therefore, *on Grassmann's own showing*, of the methods of the Ausdehnungslehre) that they are "*included* in that theory of SETS in algebra . . . . . . announced by me in 1835 . . . . . . of which SETS I have always considered the QUATERNIONS . . . . . . to be merely a *particular* CASE."

But all this has nothing to do with Quaternions, regarded as a calculus "*uniquely* adapted to Euclidian space." Grassmann lived to have his fling at them, but (so far as I know) he ventured on no claim to priority. Hamilton, on the other hand, even after reading the first *Ausdehnungslehre*, did claim priority and was never answered. He quoted, and commented upon, the very passage (of the *Preface* to that work) my allusion to which is censured by Prof. Gibbs. [*Lectures*, Pref. p. (62), foot-note.] I still think, and it would seem that Hamilton also thought, that it was *solely because Grassmann had not realized the conception of the quaternion*, whether as $\beta\alpha$ or as $\beta\alpha^{-1}$, that he felt those difficulties (as to angles in space) which he says he had not had leisure to overcome. I have not seen the original work, but I have consulted what professes to be a *verbatim* reprint, produced under the author's supervision. [*Die Ausdehnungslehre von* 1844, *oder die lineale Ausdehnungslehre, &c. Zweite, im Text unveränderte Auflage. Leipzig*, 1878.] Prof. Gibbs' citations from my article give a very incomplete and one-sided representation of the few remarks I felt it necessary and sufficient to make about Grassmann. I need not quote them here, as anyone interested in the matter can readily consult the article.

In regard to Matrices, I do not think I have ever claimed anything for Hamilton beyond the *separable* ϕ, and the symbolic cubic (or biquadratic, as the case may be) with its linear factors; and these I still assert to be exclusively his. My own work in this direction has been confined to Hamilton's ϕ, with its square-root, its applications to stress and strain, &c.

As to the general history, of which (as I have said above) I claim no exact or extensive knowledge, Cayley and Sylvester will, no doubt, defend themselves if they see fit. It would be at once ridiculous and impertinent on my part were I to take up the cudgels in their behalf. P. G. TAIT.

The Spinning Ring.

I CANNOT suppose that the mathematicians are all in error; but venture modestly to ask what are the assumed conditions under which a girdle round the earth at the equator would be subject to strain. If the surface of our globe at the equator were continuous and level land, about 30,000,000 of persons—more than 1000 to a mile—standing at equal distances and joining hands, would form a girdle without any strain, or the girdle might be formed of separate pieces of wire placed end to end in close contact, which, if afterwards soldered, would form a girdle, without strain.

Then, it is stated, in NATURE, vol. xliii. p. 514, that a wire girdle supported on poles, if "relieved from gravitation," but acted upon by a (greatly augmented) "centrifugal force *equal* to the cable's weight"—that is, by an equal force acting in the opposite direction—would be subjected to a 20-*fold* strain. Why? REGINALD COURTENAY.

4 Serjeants' Inn, Fleet Street, April 30.

BISHOP COURTENAY'S questions may perhaps be clearly answered as follows. The centrifugal force of a free spinning hoop has to be balanced by its peripheral tension; but this, having a large tangential and a small radial component, acts at a disadvantage, and may have to be very big to balance even a moderate centrifugal force. The larger the hoop the more marked is the magnitude of the tangential component as compared with the radial or effective component; so that a hoop 8000 miles in diameter could not rotate even once a day without tearing itself asunder.

An actual girdle round the earth is not dependent on peripheral tension for balancing its centrifugal force, since it is subject to an overpowering centripetal force due to the earth's gravitation.

The statement made by Mr. Herschel on p. 514, vol. xliii., involved not a 20-fold stress but a 20-fold speed, which means a 400-fold stress. OLIVER J. LODGE.

The Use of Startling Colours and Noises.

LAST January a friend showed me a smew (*Mergus albellus*) shot on the Dee, near Chester, the crop of which he had found to be full of young flat-fish. He called attention to the dazzling whiteness of the bird's breast, and suggested that it must frighten the fish, and so be a disadvantage to it. A little consideration showed that the effect would be precisely the reverse. As long as the flat-fish remains at rest, its colouring assimilates so closely to the sand on which it lies, and with which it partly covers itself, that it would not be easily seen by the smew. But if, startled by the white object flashing down on it from above, it moves, it is seen at once, and of course captured. Anybody who has ever collected small insects, such as beetles, will admit the truth of this at once.

The same effect is probably produced by the hooting or screaming of owls when hunting at night. A mouse, which would be invisible even to the sharp eyes of an owl when motionless, would be seen at once if startled into motion by the sudden "shout" of the bird, whose noiseless flight had brought it unperceived into close proximity.

Perhaps these suggestions may serve to explain other apparent difficulties in the way of natural selection.

The brown owl hoots throughout the winter here, so that it cannot be a sexual call. ALFRED O. WALKER.

Nantyglyn, Colwyn Bay, May 25.

The Formation of Language.

I PERCEIVE that my note on the evolution of speech in the case of one of my children has excited some interest and called out communications both to myself and to you; but I must trespass again on your kindness to explain that what I considered noteworthy in that case was not the invention of words, which is not of rare occurrence, but the, to me, far more important phenomenon of the evolution of the habit of speech through the three stages, so distinctly marked in this case—of simulation, the faculty we share with the monkey, and which does not imply the possession of the idea; of invention of symbols, which indicates the birth of the power of conception, and perhaps the formation of what Max Müller calls "concepts," and the perception by the young mind of a community of interest and intelligence; and, finally, the faculty of learning from others ideas already formed, or what must be considered the germ of science: and it was the clear demarcation of the three states which interested me more than the mere invention of words. And this interest is the greater as the case appears to illustrate a law that the development of the individual follows the lines of the universal, so that the child but repeats, in a very much abbreviated sequence, what humanity had gone through as a whole. My purpose in bringing the case before your readers was rather to invite the repetitions of my observations with a view to the establishing of the law, than to publish an isolated phenomenon. W. J. STILLMAN.

Rome, May 8.

Cordylophora lacustris.

IT will be interesting to zoologists to know that Prof. Weldon recently found very large quantities of *Cordylophora lacustris* on submerged roots and stems in the Rivers Ant, about Ludham Bridge, and Thurne, at Heigham Bridges, Norfolk. From my own knowledge, I can say that it is very generally met with throughout the whole system of rivers and broads in connection with the Bure. At the places spoken of, a fresh-water tide of from 6 to 18 inches is felt. I think I am safe in saying that a salt tide has but once been known so high up these rivers.

JOHN BIDGOOD.

7 Richmond Terrace, Gateshead-on-Tyne.

ON SOME POINTS IN THE EARLY HISTORY OF ASTRONOMY.[1]

IV.

FROM what has been stated it is not too much to assume that the Egyptians observed the sun on the horizon. This being so, the chances are that at first they would observe the stars on the horizon too, both stars rising and stars setting; and that is rendered more probable by the very careful way in which early astronomers defined the various conditions under which a star can rise or set, always, be it well remembered, in relation to the sun. They spoke of a star as rising or setting achronically, heliacally, or cosmically.

The cosmic rising meant that the star rose, and the cosmic setting meant that the star set, at the same moment as the sun—that is, that along the eastern horizon we should see the star rising at the moment of sunrise, or along the western horizon a star setting at the moment of the sun setting. The achronical rising is different from the cosmic in this respect—that we have the star rising when the sun is setting and setting when the sun is rising. Finally we have the heliacal rising and setting; that is taken to be that the star appeared in the morning a little in advance of the sunrise, or set at twilight a little later than the sun. The following table from Biot[1] should make matters quite clear :—

Star at eastern horizon. ...	Rising. ...	Morning. ...	True or cosmic.	Sun rising.
			Apparent or heliacal.	Sun not yet risen, but depressed below horizon sufficiently to enable the star to be seen.
		Evening. ...	True or achronic.	Sun setting.
			Apparent or heliacal.	Sun just set, and depressed below horizon sufficiently to enable the star to be seen.
Star at western horizon. ...	Setting. ...	Evening. ...	True or cosmic.	Sun setting.
			Apparent or heliacal.	Sun set, and depressed below horizon sufficiently to enable the star to be seen.
		Morning. ...	True or achronic.	Sun rising.
			Apparent or heliacal.	Sun not yet risen, but depressed below horizon sufficiently to enable the star to be seen.

It is Ideler's opinion that, in Ptolemy's time, in the case of stars of the first magnitude, for heliacal risings and settings, if the star and sun were on the same horizon a depression of 11° was taken; if on opposite horizons a depression of 7°. For stars of the second magnitude, these values were 14° and 8½.° But if temples were employed as I have suggested, even cosmic and achronic risings and settings could be observed in the case of the brightest stars.

Before we begin to consider the question of stars at all, we must be able to describe them, to speak of them in a way that shall define exactly what star is meant. We can in these days define a star according to its constellation or its equatorial or ecliptic co-ordinates, but all these means of reference were unknown to the earliest observers; still we may assume that the Egyptians could define some of the stars in some fashion, and it is evident that we here approach a matter of the very highest importance for our subject.

So far, as we have been dealing with the sun and the observations of the sun at rising and setting, we have taken for granted that the amplitude of the sun at the solstices does not change; the amplitude of 26° at Thebes, for the solstices, is practically invariable for a thousand years; but one of the results of astronomical work is that the *stars* are known to behave quite differently. In consequence of what is called precession the stars change their place with regard to the pole of the heavens, and further, in consequence of this movement, the position of the sun among the stars at the solstices and equinoxes changes also.

In the last lecture we considered what were called the ecliptic and the equatorial co-ordinates. The ecliptic was the plane in which the earth moves round the sun, and 90° from that plane we had the pole of the heavens; celestial latitude we found reckoned from the plane of the ecliptic north and south up to the pole of the heavens, and celestial longitude we reckoned along the plane of the ecliptic from the first point of Aries. We had also declination reckoned from the equator of the earth prolonged to the stars, and right ascension reckoned along the equator from the first point of Aries. The pole of the heavens then we must regard as fixed, but the pole of the earth is not fixed, but slowly moves round it. *In consequence of that movement there is a change of declination in a star's place.*

Going back to the tables, we find that the amplitude of a body rising or setting at Thebes or anywhere else depends upon its declination, so that if from any cause the declination of a star changes, its amplitude must change at any particular place.

That is the first point where we meet with difficulty, because if the amplitude changes it is the same as saying that the place of star rising or star setting changes; that is, a star which rose in the east in a certain amplitude this year will change its amplitude at some future time.

The real cause of the precession of the stars lies in the fact that the earth is not a sphere, its equatorial diameter being longer than its polar diameter, so that there is a mass of matter round the equator in excess of what we should get if the earth were spherical. Suppose that matter to be represented by a ring. The ring is differently presented to the sun, one part being nearer than the other, the nearer part being attracted more forcibly. If we take the point where there is the greatest attraction, and draw a line to the least, we can show that the case stands in this way: that the sun's pull may be analyzed into two forces, one of them between the sun and the point in a direction parallel to the line joining the centre of the sun and the centre of the ring, and another force at right angles to it. The question is, what will that force at right angles do?

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[1] Continued from p. 60.

[1] Biot, "Traité élémentaire d'Astronomie physique," 3rd edition, vol. iv. p. 625.

Here we have a model showing the rotation of the earth on its axis, and the concurrent revolution of the sun round the earth once a year. To represent the downward pull it is perfectly fair if I add a weight. Then the earth's axis, instead of retaining its direction to the same point as it did before, is now describing a circle round the pole of the heavens. It is now a recognized principle that there is, so to speak, a wobble of the earth's axis round the pole of the heavens in consequence of the attraction of the sun on the nearer point of this equatorial ring being greater than on the part of the equatorial ring removed from it. That precession movement is not quite so simple as it is shown by this model, because what the sun does in this way is done to a very much larger extent by the moon, the moon being so very much nearer to us.

In consequence, then, of this luni-solar precession we have a variation of the points of intersection of the planes of the earth's equator and of the ecliptic; in consequence of that we have a difference in the constellations in which the sun is at the time of the solstices and at the equinoxes; and, still more important, we have another difference, viz. that the declinations, and therefore the amplitudes, and therefore the places of setting and rising of the stars, change from century to century.

Having thus become acquainted with the physical cause of that movement of the earth's axis which gives rise to what is called the precession of the equinoxes, we have next to inquire into some of the results of the movement. The change of direction of the axis in space has a cycle of something between 25,000 and 26,000 years. As it is a question of the change of the position of the celestial equator, or rather of the pole of the celestial equator, amongst the stars in relation to the pole of the heavens, of course the declinations of stars will be changed to a very considerable extent; indeed, we easily see that the declination of a star can vary by twice the amount of the obliquity, or 47°, so that a star at one time may have zero declination—that is, it may lie on the equator—and at another it may have a declination of 47° N. or S. Or, again, a star may be the pole star at one particular time, and at another it will be distant from the pole no less than 47°. Although we get this enormous change in one equatorial co-ordinate, there would from this cause alone be practically no change with regard to the corresponding ecliptic co-ordinate—that is to say, the position of the star with reference to the earth's movement round the sun. This movement takes place quite independently of the direction of the axis, so that while we get this tremendous swirl in declination, the latitudes of the stars or their distance from the ecliptic north or south will scarcely change at all.

Among the most important results of these movements dependent upon precession we have the various changes in the pole star from period to period, due to the various positions occupied by the pole of the earth's equator. We thus see how in this period of 25,000 years or thereabouts the pole stars will change, for a pole star is merely the star near the pole of the equator for the time being. At present, as we all know, the pole star is in the constellation Ursa Minor. During the last 25,000 years the pole stars have been those lying nearest to a circle struck from the pole of the heavens with a radius of 23½°, which is equal to the obliquity of the ecliptic; so that about 10,000 or 12,000 years ago the pole star was no longer the little star in Ursa Minor that we all know, but the big star Vega in the constellation Lyra. Of course 25,000 years ago the pole star was practically the same as it is at present.

Associated with this change of the pole star there is another matter of the highest importance to be considered, because as the axis is being drawn round in this way, the point of intersection of the two fundamental planes, the plane of the earth's rotation and the plane of the earth's revolution, will be liable to change, and the period will be the same, about 25,000 years. Where these two planes cut each other we have the equinoxes, because the intersection of the planes defines for us the vernal and the autumnal equinoxes; when the sun is highest and lowest between these points we have the solstices. In a period of 25,000 years the star which is nearest to the equinox will return to it, and that which is nearest the solstice will return to it. During the period there will be a constant change of stars marking the equinoxes and the solstices.

The chief points in the sun's yearly path then will change among the stars in consequence of this precession. It is perfectly clear that if we have a means of calculating back the old positions of stars, and if we have any very old observations, we can help matters very much, because the old observations—if they were accurately made—would tell us that such and such a star rose with the sun at the solstice or at the equinox at some special point of ancient time. If it be possible to calculate the time at which that star occupied that position with regard to the sun, we have an astronomical means of determining the time, within a few years, at which that particular observation was made.

Very fortunately we have such a means of calculation, and it has been employed very extensively at different periods, chiefly by M. Biot in France, and quite recently by German astronomers, in calculating the positions of the stars from the present time to a period of 2000 years B.C. We can thus determine with a very high degree of accuracy, the latitude, longitude, right ascension, declination, and the relation of the stars to an equinox, a solstice, or a pole, as far back as 2000 years B.C. Since we have the planes of the equator and ecliptic cutting each other at different points in consequence of the cause which I have pointed out—the attraction of the sun and moon—we have a fixed equator and a variable equator depending upon that. In consequence of the attraction of the planets upon the earth, the plane of the ecliptic itself is not fixed, so that we have not only a variable equator but also a variable ecliptic. What has been done in these calculations is to determine the relations and the results of these variations.

A simpler, though not so accurate a method, consists in the use of the precessional globe, one of which I have here. In this we have two fixed points at the part of the globe representing the poles of the heavens, on which the globe may be rotated; when this is done the stars move absolutely without any reference to the earth or to the plane of the equator, but purely with reference to the ecliptic. We have, then, this globe quite independent of the earth's axis. How can we make it dependent upon the earth's axis? We have two brass circles at a distance of 23½° from each pole of the heavens (north and south), these represent the circle described by the pole of the earth in the period of 26,000 years. In these circles are 24 holes in which I can fix two additional clamping screws, and rotate the globe with respect to them by throwing out of gear the two points which produced the ecliptic revolution. If I use that part of the brass circle which is occupied by our present pole star, we get the apparent rotation of the heavens with the earth's axis pointing to the present pole star.

If we wish to investigate the position of things, say 8000 years ago, we bring the globe back again to its bearings, and then adjust the screws into the holes in the brass circles which are proper for that period. When we have the globe arranged to 6000 years B.C. (*i.e.* 8000 years ago), in order to determine the equator at that time all we have to do is to paint a line on the globe in some water-colour, by holding a camel's hair pencil at the east or west point. That line represents the equator 8000 years ago. Having that line, of course the intersection of the equator with the ecliptic will give us the equinoxes, so that we may affix a wafer to represent the vernal equinox. Or if we take that part of the ecliptic which is nearest to the north pole and therefore the declination of which is greatest, viz. 23½° N., we have there the position of the sun at the summer solstice, and 23½° S. will give us the position of the sun at the winter solstice. So by means of such a globe as this it is quite possible to determine the position of the equator among the stars, and note those four important points in the solar year, the two equinoxes and the two solstices. I have taken a period of 8000 years, but I might just as easily have taken a greater or a smaller number. By means of this arrangement, therefore, we can determine within a very small degree of error without any laborious calculations, the distance of any body north or south of the equator, *i.e.* its declination.

The positions thus found, say, for intervals of 1000 years, may be plotted on a curve, so that we can, with a considerable amount of accuracy, obtain the star's place for any year. Thus the globe may be made to tell us that in the year 1000 A.D. the declination of Fomalhaut was 35° S., in 1000 B.C. it was 42°, in 2000 it was about 44°, in 4000 it was a little over 42° again, but in 6000 B.C. it had got up to about 33°, and in 8000 B.C. to about 22°.

The curve of Capella falls from 41° N. at 0 A.D., to 10° at 6000 B.C., so we have in these 6000 years in the case of this star run through a large part of that variation to which I drew your attention.

Here is the curve of Sirius. This star, in 0 A.D., had a declination of 24° S.; but 5000 years B.C. it had a declination of something like 31½°. In Sirius we have the curve plotted from the computations of Mr. Hind, who has kindly placed them at my disposal. From other computations supplied by him, I have ascertained that the globe is a very good guide indeed within something like 1° of declination, always assuming that the star has no great proper motion. Considering the difficulty of the determination of amplitudes in the case of buildings, it is clear that the globe may be utilized with advantage, at all events in the first instance.

Now that we are familiar with the effect of the precession of the equinoxes in changing the amplitudes of the rising and setting places of stars, we can return to the consideration of the temples. So far, we have considered those built in relation to the sun, in the case of which body there is, of course, no precessional movement, so that a temple once oriented to the sun would remain so for a long time. After some thousands of years, however, the change in the obliquity of the ecliptic would produce a small change in the amplitude of a solstice.

Suppose we take, as before, that region of the earth's surface in the Nile valley with a latitude of about 26° N. The temples there built to observe the sun will have an east and west aspect true if they have anything to do with the sun at the equinoxes, and will have an amplitude of about 26° N. or S. if they have anything to do with the sun at the solstices.

The archæologists who have endeavoured to investigate the orientations of these buildings have found that they practically face in all directions; the statement is that their arrangement is principally characterized by the want of it; they have been put down higgledy-piggledy; there has been a symmetrophobia, mitigated by a general desire that the temple should face the Nile. This view may be the true one, if stars were not observed as well as the sun; for at Thebes, if any temple have an amplitude more than 26° N. or S. of E. or W., it cannot by any possibility have been used, as we have seen the temples at Karnak might have been used, for observations of the sun; for since the maximum declination of the sun is almost 24½° (it is at present only 23½°), represented by an amplitude of 27°, no temple oriented in a direction more northerly or more southerly could get the light of the sun along its axis.

Let us see, then, if the builders of them had any idea in their minds connected with astronomy. If they had, we may conclude that there was some purpose of utility to be served, as the solar temples were used undoubtedly, among other things, for determining the exact length of the solar year. When we come to examine these non-solar temples, the first question is, Do they resemble in construction the solar ones? Are the horizontal telescope conditions retained? The evidence on this point is overwhelming. Take the temple of Denderah. It points very far away from the sun; the sun's light could never have enfiladed it. In many others pointing well to the north or south, the axis extends from the exterior pylon to the Sanctuary or Naos which is found always at the closed end of the temple. We have the same number of pylons, gradually getting narrower and narrower as we get to the Naos, and in some there is a gradual rise from the first exterior pylon to the part which represents the section of the Naos, so that a beam of horizontal light coming through the central door might enter it over the heads of the people flocking into the temple, and pass uninterruptedly into the Sanctuary.

In these, as at Karnak, you see we have this collimating axis. We have the other end of the temple blocked; we have these various diaphragms or pylons, so that, practically, there is absolutely no question of principle of construction involved in this temple that was not involved in the great solar temple at Karnak itself.

We made out that in the case of the temples devoted to sun-worship, and to the determination of the length of the year, there was very good reason why all these attempts should be made to cut off the light, by all these diaphragms and stone ceilings, because, among other things, one wanted to find the precise point occupied by the sunbeam on the two or three days near the winter and summer solstices in order to determine the exact moment of the solstice.

But if a temple is not intended to observe the sun, why these diaphragms? Why keep the astronomer, or the priest, so much in the dark? There is a very good reason indeed; because the truer the orientation of the temple to the star, and the greater the darkness he was kept in, the sooner would he catch the rising star. In the first place, the diaphragms would indicate the true line that he had to watch; he would not have to *search* for the star which he expected; and obviously the more he was kept in the dark the sooner could he see the star.

The next point that I have to make is that in the case of some of these temples which are not directed to the sun we get exactly the same amplitudes in different localities. To show this clearly it will be convenient to bring together the chief temples near Karnak and those having the same amplitudes elsewhere.

We can do this by laying down along a circle the different amplitudes to which these various temples point. To begin with, I will draw your attention to those temples which we have already discussed with an amplitude of 27° or 26°, at Abydos, Thebes, and Karnak. Next we have non-solar amplitudes at Karnak and Thebes, associated with temples having the same amplitude at Denderah, Abydos, and other places. We have the majority of the non-solar temples removed just as far as they can be in amplitude from the solar ones, for the reason that they are as nearly as possible *at right angles* to them. We have temples with the same amplitudes high north and high south, in different places—temples, therefore, which could not have been built with reference to the sun; just as we have at different places temples with the same amplitudes which *could* have been used for solar purposes.

In connection with the possible astronomical uses of these temples, I find that when one of these temples has been built, the horizon has always been very carefully left open; there has always been a possibility of vision along the collimating axis prolonged. Lines of sphinxes have been broken to ensure this; at Medinet Abou, on the opposite side of the river to Karnak, we have outside this great temple a model of a Syrian fort. If we prolong the line of the temple from the middle of the Naos through the systems of pylons, we find that in the model of the fort an opening was left, so that the vision from the Sanctuary of the temple was left absolutely free to command the horizon.

It may be said that that cannot be true of Karnak, because we see on the general plan that one of the temples, with an azimuth of 71° N., had its collimating axis blocked by numerous buildings. That is true; but when one comes to examine into the date of these buildings, it is found that they are all very late; whereas there is evidence that the temple was one of the first, if not the very first, of the temples built at Thebes.

Mariette spent a long time in examining the temple of Karnak. His idea is that the part of the temple near the Sanctuary represents the first part of the building; and at that time the great temple of Karnak—enormous though it is now—was so small and entirely out of the way of the line of the axis of the temple of Maut that its existence might have been entirely neglected. There was first a square court like the court of the Tabernacle, and very shortly after that a very laboured system of pylons was introduced to restrict the light. The next stage shows the Sanctuary thrown back away from the court; then, after that, more complication is introduced by the addition of pylons, until finally, after two or three extensions, the length of the temple was quadrupled. So that the proof is positive that at first the horizon of the temple of Maut was left perfectly clear. Why it was subsequently blocked I shall suggest afterwards.

The next point to be noticed is that there is in very many cases a rectangular arrangement, so that if the sun were observed in one temple and a star in the other, there would be a difference of 90° between the position of the sun and the position of the star at that moment. This would, of course, apply also to two stars. Sometimes this rectangular arrangement is in the same temple, as at Karnak, sometimes in an adjacent one, as at Denderah.

If we look at Denderah we find that we have there a large temple inclosed in a square *temenos* wall, the sides of which are parallel to the sides of the temple; and also a little temple at right angles to the principal one.

It is hardly fair to say that a rectangular arrangement, repeated in different localities, is accidental; it is one which is used to some extent in our modern observatories.

The perpetual recurrence of these rectangular temples shows, I think, that in all the pairs of temples which are thus represented, there was some definite view in the minds of those who built them.

Another point is that, when we get some temples pointing a certain number of degrees south of east, we get other temples pointing the same number of degrees south of west, so that some temples may have been used to observe risings and others settings of stars in the same declination. It is then natural of course to conclude that these temples were arranged to observe the rising and setting of the same stars.

J. Norman Lockyer.

(*To be continued.*)

BOTANICAL ENTERPRISE IN THE WEST INDIES.

We have several times had occasion to mention the mission of Mr. D. Morris, the Assistant Director of the Royal Gardens, Kew, to the West Indies, in connection with the extension and organization of botanical stations in the British colonies of that region; and the *Kew Bulletin* for May and June, as we have already noted, contains his report thereon. It is a lengthy and interesting document, from which we propose to extract some particulars that may be welcome to our readers, and serve to put on record the reviving enterprise in the development of the natural resources of that part of the Empire. The primary object of Mr. Morris's visit was to settle the practical details of a scheme for establishing and administering a number of smaller botanical gardens in connection with the larger gardens of Trinidad and Jamaica. The main purpose of these gardens is to raise plants of economic value, suitable for cultivation in the various islands, "and to do all that is possible to encourage a diversified system of cultural industries, and thus relieve the planters from the results inevitable from the fluctuations of prices in the one or two staples to which they have hitherto confined their attention"; but they will also be made, as far as possible, pleasant places of public resort. Mr. Morris met with a hearty reception everywhere, and great interest was manifested in the work by the negro freeholders, in some of the islands, as well as the English colonists. The men in charge of these experimental stations, as they may be called, rather than botanical gardens, are mostly trained men from Kew; and Kew is the centre from which plants and seeds of economic plants likely to succeed in the West Indies are distributed. Mr. Morris left Kew in November last, and returned home at the end of February. Advantage was taken of his outward journey to send by the same ship, under his immediate supervision, a number of Wardian cases filled with Gambier plants. Gambier, it may be added, is the name of a substance used in tanning, obtained from *Uncaria Gambier*, Roxb.; and the plants had been raised at Kew from seeds received from the Straits Settlements, several attempts to introduce plants from the East having failed. How the plants were successfully carried to the West Indies we learn from the following passage in the report:—

"Owing to the cold weather, the cases containing the plants on board the *Atrato* were placed below in the main saloon. There was very little direct light in the daytime, but the question of warmth was for the moment of more importance than that of light. It was also hoped that they could be placed on deck in a day or two at the most. The weather during the whole of the first week, however, continued very cold, and it was impossible to expose the plants on deck. Under these circumstances it was fortunate that the electric light, with which every part of the ship was supplied, was available to try an experiment of some interest. Although the plants received very little light during the day, they had a good supply of the electric light during the night, and the plants in the cases more fully exposed to the electric light were afterwards found to be in a much better condition than the others. It is well known that plants will thrive under the influence of artificial light, but in this instance there was so little direct light available during the day, that the plants had to depend almost entirely on the light they received at night. The Gambier plants are particularly sensitive as regards a diminution of light. During the prevalence of fogs at Kew they have been known to drop their leaves within a day or two, and to remain bare during the rest of the winter. This may have been, in some measure, also due to the injurious influence of the fog itself.

"The use of electric light for the safe transit of such valuable plants as are obliged to be despatched from this country during the winter months is evidently capable of being greatly extended. It may also be utilized in the case of tropical plants arriving in this country from abroad, during the prevalence of cold weather. Such plants could be placed below directly the weather is becoming too cold for them on deck, and then the more electric light they have the better."

Out of the whole consignment to the various islands only ten plants succumbed; but this was due to an oversight in carrying the case on to Trinidad and La Guayra, and having to bring it back again to St. Vincent, thereby causing a delay in landing of ten days.

Mr. Morris visited successively Antigua, Dominica, Montserrat, St. Kitts, Anguilla, Tortola, Santa Lucia, St. Vincent, Grenada, Barbados, and Jamaica, being present at the opening of the Exhibition at the last-named island. Everywhere the Governors and other officials seem to have done their utmost, both personally and indirectly, to assist Mr. Morris in fulfilling the object of his mission. Established gardens were inspected, sites for new gardens selected, means discussed, and addresses delivered, from which it is confidently hoped that substantial advantages to the cultural industries may accrue.

Mr. Morris's Report, which may be obtained for the sum of fourpence, is a valuable and interesting account of the present condition and future prospects of planting in the various islands, and should be in the hands of all concerned. We conclude this notice with an extract from a description of the lime plantations in Montserrat, "where the immense golden heaps of ripe fruit were alone worth a journey to the West Indies."

"The West Indian lime (*Citrus medica*, var. *acida*) appears to be a thin-skinned local variety, little known outside the West India Islands. It yields juice of a singularly pure acid flavour, and it deserves to be much better known in this country in the fresh state for making 'lemon' beverages, as well as for general use in cookery. The enterprise of the Montserrat Company extends to other things besides limes. Nevertheless, from limes alone it is possible to produce a variety of articles more or less valuable. The limes themselves are exported as gathered, or they are preserved in salt water, and shipped in a pickled state for consumption in certain parts of the United States. Lime-juice, obtained by compression, is exported either raw or in a concentrated state. This latter is obtained by evaporating the raw juice in boilers until it is reduced to about one-twelfth of the original bulk, when it is ready for export as a dark, viscid substance like molasses. This is used for the preparation of commercial citric acid. From the rind of the fruit, by a process known as 'ecuelling,' which consists of gently rubbing the fruit on rounded projections arranged inside a brass basin, a very fine essence of limes is obtained. Again, by distilling the raw lime-juice a spirit is obtained known as oil of limes."

NOTES.

THE deputation which is to submit to Sir Michael Hicks Beach to-morrow a statement of the facts relating to the proposed British Institute of Preventive Medicine, will be large, influential, and thoroughly representative of the various departments of science. It is expected that the following gentlemen will speak: Sir Joseph Lister, the Duke of Westminster, Sir Henry Roscoe, Prof. Dewar, Mr. Haldane, M.P., Q.C., and Prof. Ray Lankester. A letter from Prof. Huxley will be read.

THE list of those selected for Birthday Honours includes Dr. Archibald Geikie, on whom the honour of knighthood has been conferred, and Mr. Robert Giffen, who has been made C.B.

IN the course of an investigation, part of which has already been communicated to the Royal Society, Prof. Roberts-Austen has discovered the most brilliantly coloured alloy as yet known. It has a rich purple colour, and bright ruby tints are obtained when light is reflected from one surface of the alloy to another. It contains about 78 per cent. of gold, the rest of the alloy being aluminium. The constants of the aluminium-gold series of alloys are now being examined, and will shortly be published.

ON Tuesday last, at Oxford, Convocation sanctioned the expenditure of very considerable sums of money in order to provide increased accommodation for the medical and science schools. The Lecturer in Human Anatomy, Mr. Arthur Thomson, estimated that the immediate wants of his department necessitated the expenditure of £7000. With this sum might be provided a laboratory, which would include dissecting-rooms, a museum, working rooms, and a lecture theatre. Hitherto the accommodation provided for the lecturer has been of a temporary character, and has now proved itself utterly inadequate for the requirements of his class. The number of students now studying in Oxford with the intention of passing the M.B. examination is 67. As illustrating the growth of the class, and the interest taken in this school, it may be mentioned that in 1885 the lecturer's class consisted of only three members. The Deputy Professor of Physiology (Dr. Ray Lankester) required the more modest sum of £2000 in order to supply the deficiencies in the department of Morphology. With this sum two laboratories could be provided, one 40 × 20 feet, and the other 30 × 20 feet. Meanwhile the departments of Ethnology and Geology find themselves cramped for space at the University Museum, and Convocation has granted the sum of £1300 to provide rooms for the use of the Curator and the servants of the Museum, and increased accommodation for teaching. The Hope Professor of Zoology (Prof. Westwood) needed only the expenditure of £350 upon additions and improvements in his department at the University Museum. The expenditure of these various sums, amounting in the aggregate to nearly £11,000, will place the School of Medicine and the related sciences in a satisfactory position, and the University of Oxford is to be congratulated on its appreciation of the importance of these departments, and the liberality with which it maintains them.

THE Gold Medal of the Linnean Society has this year been awarded to Dr. Edouard Bornet, of Paris, for distinguished researches in botany. His earliest publications related to the structure and life-history of Fungi and Lichens, but his name is best known for the important researches in which, with his friend M. Thuret, he has been for some years engaged, on the life-histories of Algæ, and for his valuable contributions on this subject in the "Études Phycologiques," and the "Notes Algologiques," with their beautiful illustrations.

AT a meeting of the Ashmolean Society, Oxford, on June 1, there was an interesting discussion on a paper, by Mr. Romanes, on Weismann's theories of heredity, in which Prof. Lankester and Mr. Poulton took prominent parts.

M. DOULIOT, Demonstrator in Botany at the Museum of Natural History, Paris, has undertaken a scientific expedition to Madagascar.

MR. NORMAN LOCKYER, F.R.S., has undertaken to give a lecture at Bedford College (for Ladies), Baker Street, on Wednesday next, at 4 o'clock, "On Natural Philosophy for Artists."

WE regret to have to record the death of Sir John Hawkshaw, F.R.S. He died on Tuesday last at his town residence, Belgrave Mansions, in his 81st year. The greatest of his many engineering feats was the construction of the Severn Tunnel. He was President of the Institution of Civil Engineers in 1862–63, and of the British Association at its Bristol meeting in 1875. He received the honour of knighthood in 1873.

SEVEN years have elapsed since the first International Ornithological Congress took place in Vienna, under the presidency of the late Crown Prince Rudolph. England was on that occasion, as a correspondent wrote at the time, "conspicuous by her absence," and at the second Congress, which has just been held at Budapest, Great Britain was but feebly represented. It is difficult to understand this unwillingness of Englishmen to visit an International Congress. Our countrymen are always sure of a hospitable reception, the interchange of ideas with foreign colleagues is pleasant and profitable, the personal friendships which result are of permanent value, and in the case of Museum officials the relations established with the Museums of the Continent invariably result in mutual benefit. The great question which all zoologists can discuss is that of nomenclature. This year a preliminary skirmish took place at Frankfort, where the annual meeting of the German Ornithological Society was held on May 11 and 12, under the presidency of Prof. Wilhelm Blasius, of Brunswick. The Senckenburg Museum at Frankfort had been closed for four years, and had been opened to the public only four days before the arrival of the visitors. Prof. Noll, the well-known editor of the *Zoologischer Garten*, welcomed the German Ornithological Society in a few well-chosen words, and then followed the discussion on zoological nomenclature, which occupied the best part of two days of hard work. The proposals of the Committee appointed to examine into and report on the rules of zoological nomenclature were fully discussed, and were adopted, though, by the courtesy of the members, Mr. Bowdler Sharpe, and Mr. Büttikofer, of the Leyden Museum, were allowed to state their objections to some of the propositions. The members and guests of the Society were conducted round the Museum by Prof. Noll and Dr. Hartert, and great satisfaction was expressed at the excellent condition in which Prof. Rüppell's types were found to be. The ornithological collection has been carefully catalogued by Dr. Hartert, and his recently-published catalogue of the collection is an admirable piece of work. At the conclusion of the meeting, an adjournment took place to the Zoological Gardens, where the visitors were hospitably entertained by the Director, who personally conducted them round the Gardens. From Frankfort a detachment of members and guests proceeded to Vienna and thence to Budapest, to attend the meeting of the Ornithological Congress.

MESSRS. MACMILLAN have nearly ready for publication "A History of Human Marriage," by Dr. Edward Westermarck, Lecturer on Sociology at the University of Finland, Helsingfors. In an introductory note the work is commended to the attention of students by Dr. A. R. Wallace, who expresses a high opinion of the learning and insight displayed by the author. Dr. Westermarck differs widely in many respects from the opinions hitherto held by most anthropologists as to the development of the various forms of marriage.

IN the House of Commons on Friday last, there was an interesting debate on the Ordnance Survey. Mr. Roby, who introduced the subject, had much to say as to the unsatisfactory rate at which the Survey is proceeding, and Sir George Campbell effectively contrasted the work done in England with that done in other countries. In India, he said, the surveys were incomparably ahead of those in the United Kingdom; he was often surprised at the perfection of the surveys even of those portions of that vast country only reached by sportsmen or explorers. "In his own country he found nothing of the kind. There, in one of the most cultivated and civilized places in the world, they had nothing but the old survey. It was a disgrace to the country that we should not have decent maps." Mr. Chaplin, under whose department the Ordnance Survey has been placed, said what he could in defence of existing arrangements, but was not disposed to deny that there was much solid ground for complaint. He promised that his influence should be used to secure reform in various directions.

THE University College Biological Society has arranged for an excursion to Sheerness on Saturday, June 6. The excursion will leave Victoria at 10 a.m., and the time at Sheerness will be spent either in dredging or shore work. The party will be accompanied by Prof. Weldon.

THE Eastern papers report that an expedition has, by order of the Straits Government, commenced work on the frontier between Burmah and the Malay Peninsula. Its operations will be chiefly confined to Pahang. It is placed under the charge of Mr. Ridley, Director of Gardens and Forests in the Straits Settlements, accompanied by Mr. William Davison, Curator of the Raffles Library, Singapore, and Lieutenant Kelsall, R.A. The funds available for the expedition are 2000 dollars voted from the Straits Treasury. The object is to ascend the highest mountain in Pahang, incidentally noting all that can be learned about the physical features and the flora and fauna of the country. The expedition was to go by steamer to Pekan; thence up stream to Kuala Lipis; thence northerly up the Tembelinis and Sat rivers. Having ascended the latter river so far as it may be navigable for small canoes, the expedition will strike through forest and jungle, estimated to extend for sixty miles, till they emerge at Gunong Tahan, which is said to be about 8000 feet high. Ascending this mountain, and crossing what is called Cameron's plateau, they will then ascend Gunong Siam, a mountain the height of which has been estimated to be as much as 14,000 feet. Having completed this ascent, they will return by the same route, the estimated period of absence from Singapore being between two and three months. The party were to take with them three Tamil hunters and collectors attached to Mr. Davison's Museum staff, and three Malays of the Gardens and Forests Department.

AT the meeting of the French Meteorological Society on May 5, a discussion by M. Millot of fifty years' observations at Nancy was presented. The temperature and rainfall values were divided into two periods, viz. 1841–79 and 1880–90. These averages showed that the mean temperature had considerably decreased since the winter of 1879–80, and that the amount of rainfall had increased; the climate showed a tendency to become more continental. M. Teisserenc de Bort communicated the results of his inquiries respecting a destructive tornado which visited the town of Dreux on August 18 last. At 10h. 5m. p.m., Paris time, a sharp clap of thunder occurred, followed by heavy rain and hail for about a minute, and five minutes later the tornado broke over the town with a noise resembling that of an express train, making a furrow in the ground, and in less than a minute tiles were flying about, trees uprooted, and several houses destroyed. After a short course the effects of the tornado ceased, and it appeared to rise to the upper strata of air, but descended again with equal violence near Epone about 60 kilometres distant, the rate of translation being about 29 miles an hour. The action of the electricity seemed to be of an unusual nature; although much damage was done by it, no metallic object was fused, but only traces of fusion could be found in bad conducting bodies. Among other incidents an iron bedstead was dismounted, without trace of fusion. The paper was illustrated by several photographs, showing the damage done in various parts of the path.

DR. J. HANN has communicated another important treatise to the Vienna Academy, entitled "Studies on the Conditions of Air-pressure and Temperature on the Summit of the Sonnblick, with remarks upon their importance for the theory of cyclones and anticyclones." The work is based upon four years' observations, and is divided into eight sections, viz.:—(1) An investigation of the general meteorological conditions under which the maxima and minima of air-pressure occur on the Sonnblick. The anomalies of pressure are more marked above than below, and are increased by the accompanying temperature anomaly, which is relatively high in barometric maxima, and relatively low in barometric minima. (2) The range of temperature during the passage of a barometric wave. This is, at least during the winter season, the opposite to that at the lower level. (3) Temperature with varying amount of cloud in winter. The highest temperature coincides with the least cloud, upon the summit, and conversely on the plain. The clear winter days on the Sonnblick have relatively high temperature with great dryness, and these conditions are characteristic of the barometric maxima. (4) Monthly maxima and minima of temperature. The former mostly occur during barometric maxima, and the latter when the high pressure lies in the west or north, and while a barometric minimum exists over Italy or the Adriatic. (5) Temperature and air-pressure on the Sonnblick during barometric minima over Central Europe, especially over the Eastern Alps. The mean temperature at the height of 6650 feet during the passage of barometric minima was below the normal, amounting on an average to 2°·5 F. during the winter season. The use of deviations of pressure and temperature in answering many questions of atmospheric physics is here discussed. (6) Vertical distribution of temperature, and mean temperature in a column of air of 3 kilometres in height. The calculations have been made separately for each winter. (7) Preliminary indications respecting the relations of the wind-directions to barometric maxima and minima. A considerable divergence (45°–90°) is shown from the directions as observed below, and the results confirm the conclusions drawn from cloud observations by J. A. Broun and others. (8) Refutation of some objections against the conclusiveness of temperature observations on mountain summits, and general remarks on cyclones and anticyclones. The author points out that recent mountain temperature observations and other facts are opposed to the explanation of barometric maxima and minima in extra-tropical regions by purely thermic considerations.

THE relations of weather and disease have been recently investigated by Herr Magelssen, of Leipzig, who, having formerly called attention to the nature of certain "waves" which recur in the variations of temperature (distinguishing waves of about 12 days, 50 days, and 18 to 20 years duration), now traces a connection of these with diseases and mortality. The year-waves especially show this connection; the mortality (in our latitudes) varying with the winter temperature. The least mortality (relatively) is at the middle part of the temperature periods. The injurious influence of heat is dominant in the more southern latitudes (such as Vienna), while cold begins to act beneficially. In northern places, mild winters prove injurious where several very mild winters come in succession (*e.g.* Stockholm in 1871–74). The most favourable conditions seem to be an alternation of moderately cold and moderately mild winters. Too much importance, the author thinks, has been attached to relative humidity. He further offers proof that infectious disease is even more dependent on weather than disease of the respiratory organs, or arising from chill.

THE value of systematic observation of snow is now being recognized in meteorology; and in Russia observations were commenced in January last year at 428 stations in the European portion of the Empire, 21 in the Asiatic, and 55 in the Caucasus. At first it was simply reported daily whether there was a continuous snow-covering about the station or not. But last winter the inquiry has been extended to the depth and general behaviour of the snow. Thus it is expected that in a few years, some valuable climatological material will have been accumulated at St. Petersburg. The report of Herr Berg on the snow in the early months of 1890, in European Russia (*Repert. für Meteor.*), contains a map showing the southern and western limit of the continuous snow-covering for the first and fifteenth of each of the months January to April. In the west the snow extended steadily till the beginning of March, the limit being then close to the Baltic. In the south-east, there was steady advance till February, and as far as the coast of the Caspian. In the south, the advance was fluctuating, there being a maximum in the middle of January, and the middle of February, both reaching to the Black Sea coast. The retirement of the snow-limit began in the south and south-east in the middle of February; in the west about half a month later. The general direction was north-east. On April 15 the limit passed through Onega on the White Sea, Wetluga, and Katherinenburg. By the first of May, all European Russia was free from snow. Herr Berg describes the weather accompanying the disappearance of the snow, and traces its causation.

A DIRECT observation of hail in the process of formation is recorded in the *Naturw. Rundschau*. In the afternoon of a squally day Prof. Tosetti, looking eastwards through the window of a house (in Northern Italy) which, with two others, enclosed a court, saw the rain which streamed down from the roof to the right, caught by a very cold wind from the north, and driven back and up in thick drops. Suddenly a south wind blew, and the drops, tossed about in all directions, were transformed into ice balls. When the south wind ceased, this transformation also ceased, but whenever the south wind recurred, the phenomenon was reproduced, and this was observed three or four times in ten minutes.

Engineering of the 29th ult. states that an extraordinary accident had occurred at the London-Paris Telephone Office in the Palais de la Bourse. One of the *employés*, a gentleman named Weller, wished to communicate with the London office on a matter of service. He had already rung up the English officials, and, the bell having sounded in reply, took up the receivers and put them to his ears, when he suddenly sustained a shock of electricity of such severity that it threw him staggering backwards against the door of the telephone cabinet, which, not having been properly fastened, flew open, with the result that he was thrown heavily to the ground. It appears from inquiries that similar accidents, although less serious, have occurred at this telephone office on several previous occasions. The officials attribute them to lightning striking the wire, either at San Gatte, where the submarine cable ends, or at the terminus of the land wire on the Palais de la Bourse. Such accidents, it is declared, might be easily prevented by the simple expedient of erecting lightning conductors at the point where the cable comes ashore, and at the terminus in Paris.

IN the nineteenth annual report of the directors of the Zoological Society of Philadelphia, attention is called to the unprecedented destruction of many of the more valuable and important animals of the native American fauna, and to the need for the immediate adoption of every means which can be employed to save them from complete extinction. The directors think that a good deal may be done in furtherance of this object, both in zoological gardens and private preserves. Of all the bisons now surviving outside the National Park, probably nine-tenths are comprised in a few herds owned by private individuals and zoological societies.

A FINE tortoise, weighing 87 pounds, obtained by the U.S. Fish Commission steamer *Albatross*, during her recent visit to the Galapagos Islands, has recently been deposited in the Zoological Park at Washington, D.C. The specimen was collected by Mr. C. H. Townsend on Duncan Island, and is of much interest, not only on account of the locality it represents, but as showing that Dr. Baur was a little hasty in deciding that *Testudo ephippium* is only a synonym of *T. abingdoni*. The Duncan Island tortoise agrees exactly with Dr. Günther's figure of *T. ephippium*, and is entirely distinct from the Abingdon Island species, which is also well-figured in Dr. Günther's paper. This figure shows a little emargination in the second marginal scute, which might seem accidental, but as it is exactly repeated in the specimen belonging to the U.S. National Museum, and as the emargination exists in the bony carapace, it is probably a constant specific character. Dr. Günther gives Indefatigable Island as the locality of *T. ephippium*, and if this be correct the species occurs on at least two islands of the group. Besides the Duncan Island Tortoise, examples of *T. vicina* and *T. nigrita* are now living in the Zoological Park, while the U.S. National Museum possesses skeletons of *T. abingdoni* (imperfect), *T. vicina*, and *T. nigrita*. The locality of this last-named species is still uncertain, but there is some reason to suppose that it may be from Chatham Island. *T. nigrita* has the most arched carapace of any species, *T. ephippium* and *T. abingdoni* the longest and anteriorly most compressed and elevated carapaces. Between these lie in the order named *T. microphyes* and *T. vicina*. There is a direct correlation between the anterior height of the carapace and the length of the neck, the rule being the higher the carapace the longer the neck, *T. nigrita* and *T. abingdoni* having respectively the shortest and longest necks. Mr. Townsend writes that tortoises are now extremely rare on Duncan Island.

THE June number of the *Zoologist* contains an interesting paper on the habits of the moose, by Mr. J. G. Lockhart. One of the points noted by the author is, that moose generally lie with the tail to windward, trusting to their senses of hearing and smelling, which are remarkably acute, to warn them of approaching danger from that quarter; they can use their eyes to warn them from danger to leeward, where hearing, and especially smelling, would be of little use. While they are sleeping or chewing the cud, their ears are in perpetual motion, one backward, the other forward, alternately. They also have the remarkable insight to make a short turn and sleep below the wind of their fresh track, so that anyone falling thereon and following it up is sure to be heard or smelt before he can get within shooting distance.

MR. L. UPCOTT GILL has published as a pamphlet a paper read by the Rev. H. A. Soames before the Bromley Naturalists' Society on the scientific measurement of children. Mr. Soames says he finds such measurements as he describes, taken every term, a good guide as to whether his pupils may be pressed with work or not. "If the increase is regular and the weight fair, according to the height, I do not fear to press them; but if, on the other hand, the weight is low, or if the height increases and not the weight, or if the increase in height is too rapid, I think it a very fair excuse for laziness, and take great care that too much work is not expected from them."

THE first volume of Sir William Thomson's "Popular Lectures and Addresses" (Macmillan), has reached a second edition. The third volume has also just been published, and the author hopes that the second volume may appear in the course of a year or two.

THE new number of the Journal of the Anthropological Institute (vol. xx., No. 4) opens with a paper in which Lady Welby calls attention to what she calls an apparent paradox in mental evolution. The number also includes a paper, by Mr. F. W. Rudler, on the source of the jade used for ancient implements in Europe and America; and the Presidential address delivered by Dr. Beddoe.

THE Botanical Society of Edinburgh has issued the eighteenth volume of its Transactions and Proceedings. Dr. Aitchison's "Notes on the Products of Western Afghanistan and of North-Eastern Persia," forming the first part of the volume, may be obtained separately.

Two new parts (62 and 63) of the elaborate dictionary of Chemistry included in the "Encyclopaedie der Wissenschaften" (Breslau: Eduard Trewendt) have appeared. The eighth part of the hand-book of Physics, in the same Encyclopædia, has also been published.

THE ninth edition of "Telegraphy," by W. H. Preece and J. Sivewright (Longmans), has been published. The edition is described as "almost a new book." No fewer than 24 figures have been altered and 44 excluded, and there are now 265 as compared with 194 in the last edition. The authors have aimed at "providing such a general introduction to the art and science of telegraphy as will enable the student to proceed to the study of more advanced works, and give to the operator an intelligible explanation of the apparatus with which he has to deal."

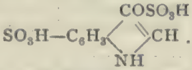
MESSRS. LONGMANS, GREEN, AND CO. are issuing the tenth edition of Quain's "Elements of Anatomy." It will appear in three volumes, and is being edited by Prof. E. A. Schäfer and Prof. G. D. Thane. The second part of the first volume—by Prof. Schäfer—has just been published. The subject is general anatomy or histology.

PART 32 of Cassell's "New Popular Educator" has been published. Besides illustrations in the text, it contains a coloured map of Switzerland.

THE Geological Survey Department of Canada has issued the first of a series of descriptive and illustrated quarto memoirs on the Vertebrata of the Tertiary and Cretaceous rocks of the Canadian North-West Territory, prepared for the Survey by Prof. E. D. Cope, of Philadelphia. The Report is devoted exclusively to a consideration of the species from the Lower Miocene deposits of the Cypress Hills, in the district of Alberta, and consists of twenty-seven pages of letterpress, illustrated by fourteen full-page lithographic plates. The second part, which will contain illustrated descriptions of the Vertebrates of the Laramie formation of the North-West Territory, by the same author, is now in course of preparation.

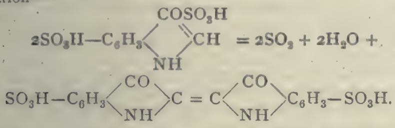
MR. PERCY F. KENDALL has prepared a little volume entitled "Hints for the Guidance of Observers of Glacial Geology." It is intended to serve as an answer to the requests for guidance which have been made by members of the North-West of England Boulder Committee. The work is printed only on alternate pages, so that students using it will have space for occasional brief notes.

"AN approved Treatise of Hawks and Hawking by Edmund Bert," 1619, has just been reprinted, with an introduction by Mr. J. E. Harting. It is the rarest of English books on falconry, and no copy has come into the market for nearly twenty years. The reprint is as nearly a facsimile as it is possible to make it without the aid of photography, and a hundred copies only have been printed. It is issued by Mr. Quaritch.

INDIGOCARMINE, the commercially important disulphonic acid of indigo, has been synthesized in an extremely simple manner by Dr. Heymann in the laboratory of Messrs. Bayer and Co. of Elberfeld, and a description of the mode of operation is given in the new number of the *Berichte*. The reaction merely consists in acting with excess of fuming sulphuric acid upon phenyl glycocoll, C_6H_5—NH—CH_2—COOH, the aniline derivative of glycollic acid. When a quantity of fuming sulphuric acid is poured upon a tenth of its weight of phenyl glycocoll in a test tube, the phenyl glycocoll rapidly dissolves, the acid becoming coloured yellow and slightly elevated in temperature, while sulphur dioxide commences to be evolved. If the solution is then poured over ice the greenish-blue colour of indigocarmine is at once obtained. The best conditions for working the process on the large scale are as follows. One part of phenyl glycocoll is mixed with ten to twenty times its weight of fine sand so as to avoid local superheating during the process of addition to the fuming acid. The mixture is then introduced into about twenty times its weight of fuming sulphuric acid at a temperature of about 20°-25°. The fuming acid should contain at least 80 per cent. of sulphuric anhydride, and the temperature should be so controlled that it never exceeds 30° during the process of adding the mixture. The yellow solution thus obtained yields instantly the blue coloration due to indigocarmine on removing the large excess of sulphuric anhydride by the addition of ordinary oil of vitrol, sulphur dioxide being evolved. Upon further diluting with ice and addition of common salt (indigocarmine being more difficultly soluble in salt solutions than in pure water) the colouring-matter is precipitated, and may be readily isolated. The product thus obtained is found to consist of pure indigocarmine. The tints obtained with this product are vastly superior in beauty and clearness to those obtained with even the better kinds of commercial indigocarmine, on account of the higher degree of purity attained by this mode of preparation. The chemical changes occurring during the process appear to be as follows. The first product of the action of fuming sulphuric acid upon phenylglycocoll appears to be the sulphonic acid of indoxyl sulphate, 

$$\mathrm{SO_3H{-}C_6H_3}\Big\langle\begin{matrix}\mathrm{COSO_3H}\\ \\ \mathrm{NH}\end{matrix}\Big\rangle\mathrm{CH}.$$

This substance, however, is unstable, and decomposes upon the removal of the excess of SO_3 into indigo disulphonic acid, sulphur dioxide, and water, probably according to the following equation—



$$2\mathrm{SO_3H{-}C_6H_3}\Big\langle\begin{matrix}\mathrm{COSO_3H}\\ \\ \mathrm{NH}\end{matrix}\Big\rangle\mathrm{CH} = 2\mathrm{SO_2} + 2\mathrm{H_2O} +$$

$$\mathrm{SO_3H{-}C_6H_3}\Big\langle\begin{matrix}\mathrm{CO}\\ \mathrm{NH}\end{matrix}\Big\rangle\mathrm{C}=\mathrm{C}\Big\langle\begin{matrix}\mathrm{CO}\\ \mathrm{NH}\end{matrix}\Big\rangle\mathrm{C_6H_3{-}SO_3H}.$$

Of course the most important point of commercial interest about a new reaction is the yield, and in this respect Dr. Heymann is very fortunate, for already 60 per cent. of the theoretical has been attained. The process has consequently been patented by Messrs. Bayer and Co., and appears likely to become a very successful one.

THE additions to the Zoological Society's Gardens during the past week include a Water Buck (*Cobus ellipsiprymnus* ♀), a Leopard (*Felis pardus*), two Vulturine Guinea Fowls (*Numida vulturina*), two Mitred Guinea Fowls (*Numida mitrata*) from East Africa, presented by Mr. G. S. Mackenzie, F.Z.S.; a Peregrine Falcon (*Falco peregrinus*) from Scotland, presented by Mr. Thomas C. Smith; a Mountain Ka-Ka (*Nestor notabilis*) from New Zealand, presented by Mr. Herbert Furber; a Grey Squirrel (*Sciurus griseus*), a Squirrel (*Sciurus* sp. inc.) from North America, a Ducorp's Cockatoo (*Cacatua ducorpsi*) from the Solomon Islands, presented by Mr. Nicholas O'Reilly; two Ravens (*Corvus corax*) from Ireland, presented by Captain Ogilby; a Cheetah (*Cynælurus jubatus*) from Persia, three Blandford's Rats (*Mus blandfordi*), two —— Terrapins (*Clemmys* sp. inc.) from India, deposited; two Coypus (*Myopotamus coypus*) from South America, two Andaman Starlings (*Sturnia andamanensis*) from the Andaman Islands, two Red-billed Hornbills (*Toccus erythrorhynchus*), two African White Spoonbills (*Platalea alba*) from Africa, two Virginian Eagle Owls (*Bubo virginianus*) from North America, purchased; a Red Deer (*Cervus elaphus* ♂), a Japanese Deer (*Cervus sika* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE MERIDIAN PHOTOMETER.—In vol. xxiii. of the *Annals of the Harvard College Observatory*, Prof. E. C. Pickering and O. C. Wendell give and discuss the observations made at Cambridge, U.S., with the meridian photometer during the years 1882-88. The observations relate principally to stars north of the declination −40°. Vol. xiv. of the *Annals* contained the results of observations of the brightness of stars made with a small meridian photometer. The present volume deals with the photometric measurements of somewhat fainter stars, made by means of a similar but larger instrument.

REPORT OF HARVARD COLLEGE OBSERVATORY.—Prof. Pickering has just issued his Report for last year. He again urges the necessity of a fire-proof building for storing the 27,000 photographic plates of spectra, 9000 of which were taken in 1890. Legacies for the endowment of science in America are so common that it is not surprising to learn that the Observatory has received a gift of 25,000 dollars through the late Mr. J. I. Bowditch. During the past year 1309 photographs of stellar spectra have been taken with the Bache telescope at the station near Closica, in Peru. Nearly all of them relate to the region south of −20°. Mrs. Draper has added another instrument of the same kind to the Henry Draper Memorial. This is mounted in the Observatory grounds at Cambridge, and since September 1889, 2157 photographs have been taken with it, covering the sky north of −20°. By placing a prism of small angle over the objective, the spectra of stars as faint as the tenth magnitude have been obtained. Six stars with Type IV. spectra have been discovered. Spectra of fifteen planetary nebulæ have been photographed. The hydrogen line F has been shown to be bright in eight stars. Bright line stars of the Wolf-Rayet type now number twenty-eight, three having been added to the list during the past year. The names are given of thirty variable stars of long period, in which the hydrogen lines are bright at maximum. This peculiarity has furnished a means of discovering seven new variable stars. The 11-inch telescope has been used for a detailed study of the spectra of the brightest stars, with the result that β Aurigæ and ζ Ursæ Majoris have been discovered to be close binaries. One photograph of σ Herculis seems to show that this star also is double, but this has not been confirmed. With the 12-inch telescope a number of "canals" on Mars have been recognized, but only one of them was distinctly seen to be double. An important accession to the white spot surrounding the southern pole was found by photographs to have occurred between the nights of April 9 and 10. The Report concludes with a list of the numerous publications issued by the Observatory during the year.

THE SOLAR PARALLAX AND ITS RELATED CONSTANTS.

IT would be difficult to conceive a more masterly and comprehensive exposition of astronomical and physical constants than one just issued by Prof. W. Harkness, of the United States Naval Observatory. As is rightly pointed out, "The solar parallax is not an independent constant. On the contrary, it is entangled with the lunar parallax, the constants of precession and nutation, the parallactic inequality of the moon, the lunar inequality of the earth, the masses of the earth and moon, the ratio of the solar and lunar tides, the constant of aberration, the velocity of light, and the light equation." It should therefore be determined simultaneously with all these quantities by means of a least-square adjustment, and Prof. Harkness develops such a method. The equations connecting the constants are given, whilst the numerical values which are discussed are based upon an enormous mass of astronomical, geodetic, gravitational, and tidal observations which have required more than two hundred years for their accumulation. The sources of probable error are also examined, and it is suggested how some of the constants may be improved in the future. The completeness of the lists of constants, and the careful manner in which they are discussed and corrected by the comprehensive least-square adjustment which is developed, justifies our giving *seriatim* the results obtained:—

Equatorial semi-diameter of the earth—

3963·124 ± 0·078 miles.

Polar semi-diameter of the earth—

3949·922 ± 0·062 miles.

One earth quadrant—

10001816 ± 125·1 metres.

Oblateness or flattening of the earth—

1/300·205 ± 2·964.

Eccentricity of the earth—

0·006651018.

Mean density of the earth—

$$5{\cdot}576 \pm 0016.$$

Surface density of the earth—

$$2{\cdot}56 \pm 0{\cdot}16.$$

Length of the seconds pendulum (ϕ = latitude)—

$$39{\cdot}012540 + 0{\cdot}208268 \sin^2\phi \text{ inches.}$$

Acceleration due to gravity—

$$32{\cdot}086528 + 0{\cdot}171293 \sin^2\phi \text{ feet.}$$

Length of the sidereal year—

$$365\text{d. } 6\text{h. } 9\text{m. } 9{\cdot}314\text{s.}$$

Length of the tropical year at time t—

$$365\text{d. } 5\text{h. } 48\text{m. } 46{\cdot}069\text{s.} - 0{\cdot}53675\text{s.} \left(\frac{t - 1850}{100}\right).$$

Length of the sidereal month—

$$27\text{d. } 7\text{h. } 43\text{m. } 11{\cdot}524\text{s.} - 0{\cdot}022671\text{s.} \left(\frac{t - 1800}{100}\right).$$

Length of the synodical month—

$$29\text{d. } 12\text{h. } 44\text{m. } 2{\cdot}841\text{s.} - 0{\cdot}026522\text{s.} \left(\frac{t - 1800}{100}\right).$$

Length of the sidereal day—

$$86164{\cdot}09965 \text{ mean solar seconds.}$$

Ratio of the mean motions of the sun and moon—

$$0{\cdot}074801329112.$$

Mass of Mercury (Sun = 1), $\frac{1}{8374672 \pm 1765762}$

" Venus " $\frac{1}{408968 \pm 1874}$

" Earth " $\frac{1}{327214 \pm 624}$

" Mars " $\frac{1}{3093500 \pm 3295}$

" Jupiter " $\frac{1}{1047{\cdot}55 \pm 0{\cdot}20}$

" Saturn " $\frac{1}{3501{\cdot}6 \pm 0{\cdot}78}$

" Uranus " $\frac{1}{22600 \pm 36}$

" Neptune " $\frac{1}{18780 \pm 300}$

" Moon (Earth = 1) $\frac{1}{81{\cdot}068 \pm 0{\cdot}238}$

Constant of solar parallax—

$$8''{\cdot}80905 \pm 0''{\cdot}00567.$$

Mean distance of earth from sun—

$$92796950 \pm 59715 \text{ miles.}$$

Eccentricity of the earth's orbit—

$$0{\cdot}016771049.$$

Lunar inequality of the earth—

$$6''{\cdot}52294 \pm 0''{\cdot}01854.$$

Lunar parallax—

$$3422''{\cdot}54216 \pm 0''{\cdot}12533.$$

Mean distance from earth to moon—

$$238854{\cdot}75 \pm 9{\cdot}916 \text{ miles.}$$

Eccentricity of moon's orbit—

$$0{\cdot}054899720.$$

Inclination of moon's orbit—

$$5^\circ\ 8'\ 43''{\cdot}3546.$$

Mean motion of the moon's node in 365¼ days—

$$-19^\circ\ 21'\ 19''{\cdot}6191 + 0''{\cdot}14136 \left(\frac{t - 1800}{100}\right).$$

Parallactic inequality of the moon—

$$124''{\cdot}95126 \pm 0''{\cdot}08197.$$

Constant of luni-solar precession—

$$50''{\cdot}35710 \pm 0''{\cdot}00349.$$

Constant of nutation—

$$9''{\cdot}22054 \pm 0''{\cdot}00859.$$

Constant of aberration—

$$20''{\cdot}45451 \pm 0''{\cdot}01258.$$

The time taken by light to traverse the mean radius of the earth's orbit (the light equation)—

$$498{\cdot}00595\text{s.} \pm 0{\cdot}30834\text{s.}$$

The velocity of light *in vacuo* per second of mean solar time—

$$186337{\cdot}00 \pm 49{\cdot}722 \text{ miles.}$$

In order to improve the system of constants discussed, Prof. Harkness thinks that the parallax of the moon should be determined by the diurnal method at one or more stations as near as possible to the equator, and that the Observatories in the northern and southern hemispheres should co-operate with each other for two or three years in systematically making meridian observations of the moon to improve our knowledge of its parallax. Numerous pendulum observations are required, and new determinations of the constants of aberration and nutation by as many different methods as possible. The most probable coefficient of the lunar inequality of the earth's motion should be obtained from Greenwich and Washington meridian observations of the sun, whilst the opposition of Mars in 1892, and favourably situated asteroids, should be utilized for new determinations of the solar parallax.

The laborious character of an investigation which leads to the results here given is patent to all. To say, therefore, that all the computations involved were made and checked by Prof. Harkness himself is to testify to industry very rarely excelled.

TECHNICAL EDUCATION IN RUSSIA.

An interesting report on technical education in Russia has been laid before Parliament by the Foreign Office. It is a digest by Mr. Harford of a very voluminous Report, compiled by Mr. Anopoff, Director of the Nicholas Industrial School at St. Petersburg, on technical education in Russia, and is described by Sir R. Morier as giving an exhaustive review of all that has been done during the last 20 years in Russia in this important branch of national education, and is of special interest as furnishing information on the most recent legislation respecting schools about to be founded.

M. Anopoff confines himself to giving full details of intermediate and elementary technical and industrial teaching institutions, without attempting a description of the higher schools. The establishment of these former classes of schools dates, he says, from only some 25 years back, but in that short space of time they have spread to the confines of the Russian Empire. In 1883, a special section for technical and professional education was created in the Ministry of Education. According to the new regulations of the *Realschulen*, intermediate and elementary technical and industrial schools are to be opened at the public expense. M. Anopoff remarks, however, that these new schools cannot be expected to be at first as successful as the existing schools with their long practical experience. He adds, too, that the greater number of the technical schools in Russia were founded at the initiative, and often even at the expense, of local societies and private persons. The various technical and industrial institutions in Russia are divided into five groups:—(1) Technical schools with the course of intermediate schools resembling the *Realschulen*, but differing from them by their professional character being more strongly marked. The task of these schools, which, as regards the knowledge required, is about equivalent to the standard of the *Realschulen*, with a course of from six to eight years, consists in imparting a general acquaintance with the technical and partly commercial subjects which are indispensable for the assistants of engineers, and for independent managers of small technical undertakings. (2) To the second group may be referred institutions in which subjects of general education are taught within the scope of the courses of municipal schools and district and village schools with two classes. From those who enter them a knowledge is required approximate to the scope of primary schools, the full course of study lasting from four to six years. In these schools, besides the subjects taught in the municipal schools under the regulations of 1872, the following additional subjects are taken up: physics, mechanics, technology of metals and woods, bookkeeping, &c., while to drawing, both freehand and geometrical, much attention is given. The object of these institutions is the preparation of skilled artisans for factories, of lesser mechanical specialists, machinists, and draughtsmen. In this category should be included the railway schools, but as they are under the control of the Ministry of Communications, and serve certain special objects exclusively connected with railways, no account of them is given. (3) Industrial schools with a course of general education not exceeding the scope of the course of primary schools, or sometimes reaching the standard of the second class in village schools with two classes. In most of them pupils are received who have completed the course in the public school, and who repeat what they have gone through in it. These schools are founded with the object of preparing skilled artisans for village and domestic industries, and also factory hands. They contain workshops for joiners, blacksmiths, carpenters, fitters, tailors, shoemakers, saddlers, bookbinders, &c.; but few of these institutions can boast of a systematic course of instruction in trades. (4) To this group belong various special and general educational schools for adults, as the school for foremen builders, the school for printers, the evening and Sunday special classes of the Imperial Technical Society at St. Petersburg, the Riga Industrial School, &c. The teaching in these institutions takes place in the evenings of week-days, and on Sundays, *i.e.* when the adult workmen for whom they are intended are free from their work. (5) This group consists of elementary schools of general education, *i.e.* primary, district, or municipal schools with supplementary industrial sections. It is worthy of notice that persons who have gone through the whole course, or at least reached a certain standard, at any of the schools of these five groups, enjoy certain privileges with regard to exemption from military service.

The report then goes on to describe in detail the courses of some of the leading industrial schools as types of the different groups, as well as of the industrial classes attached to the elementary schools. In conclusion, the report summarizes the more important provisions of the ukase of March 7/19, 1888, respecting the conditions under which technical and industrial schools may be opened in Russia, either wholly or in part, at the expense of the State (given in Appendices I., II., III.). The cost of maintenance of these schools is respectively estimated in the ukase as follows: the intermediate mechanical technical schools at 27,311r. (£2730) per annum; the elementary mechanical technical schools at 19,436r. (£1945) per annum; and the trade schools at 11,960r. (£1200) per annum. The Ministry of Education has assigned for this year the sum of £50,000 for the creation of these technical schools, and it is reported that the Ministry has been urged to devote a considerable portion of this sum to founding schools in the districts where village industries prevail, the richer manufacturing districts being better able to dispense with State aid. The provisions of the ukase are:—(1) The industrial schools for the male inhabitants of the Empire exist for the purpose of diffusing among the population technical education of the intermediate and elementary standards, as well as instruction in handicrafts. (2) The intermediate technical schools impart the instruction and skill indispensable to artificers who are destined in time to act as the trusted assistants of engineers and of other managers of industrial enterprises. (3) The elementary technical schools, besides initiation into the mysteries and methods of some one definite handicraft, likewise impart the knowledge and skill indispensable to men whose duty it will in time become to act as master-workers and immediate overseers of the operations of artisans in industrial establishments. (4) The trade schools exist for the purpose of giving practical tuition in the methods of any one trade, and at the same time of communicating such knowledge and skill as are absolutely necessary to the intelligent execution of the work of such trade. (5) Industrial schools of each of the above-mentioned categories can exist either apart or in conjunction with other similar schools of various degrees and specialities. (7) The industrial schools are supported at the expense of the Crown, or of the *zemstvos*, societies, guilds, or private individuals, or by funds contributed simultaneously from all these sources. (8) The course in the intermediate technical schools is not to exceed four years; that of the elementary and trade schools three years. (9) Those who enter trade schools are required to produce a certificate of their having gone through the course of an elementary school; those who enter the elementary technical schools, a certificate of having gone through the course in a municipal school, or village school, with two classes; while those who enter intermediate technical schools must have gone through five classes of a *Realschule*. (10) Those who are unable to satisfy the conditions mentioned in the preceding paragraph, but who have worked not less than two years in industrial institutions, and have proved that they can successfully follow the course at the school they wish to enter, may be also admitted. (11) Industrial schools must have: (*a*) a library, (*b*) a room with appliances for geometrical and freehand drawing, (*c*) where possible a room with appliances for modelling, (*d*) the necessary school books for the special object for which the school is intended, and in addition the requisite appliances for the practical work of the apprentices. (14) Pupils who have successfully completed their education in an intermediate technical school, after a four years' course, receive the appellation of artificer in their specific calling. Those who have only gone through a two or three years' course, only receive this appellation after three or two years respectively, spent uninterruptedly in industrial work. Those who are so styled obtain certain privileges as regards their civil status and in respect to military service, and they enjoy in addition the right of entering the higher technical schools. Those who have completed the course at the other two categories of schools enjoy the privileges as regards civil status and military service which correspond to the general education they have received.

FOSSIL FISH OF THE SCANDINAVIAN CHALK.

MR. DAVIS has availed himself of the opportunities presented to him by the chief officers of the Museums of Lund, Stockholm, and Copenhagen, and has published a monographic account of the fish remains from the Cretaceous formations of Scandinavia.

Over seventy years ago Sven Nillson first discovered fish remains in the Swedish chalk. Since then numerous large collections have been made by the officers of the Geological Survey of Sweden and others, and the greater number of these specimens were unreservedly placed at the disposal of Mr. Davis for description in his memoir; he has also had the opportunity of consulting some smaller collections in Sweden, and most of the forms have been figured from the original specimens by Mr. Crowther.

These fish remains show a closer relationship to the Cretaceous fish remains of the north of Europe, as represented by the English and French chalk fish, than to the more highly specialized chalk fauna of Asia Minor, but they do not afford representatives of several of the Physostomous Teleosteans, such as Ichthyodectes, Protosphyræna, and Pachyrhizodus, which have been found in the English chalk, and have also occurred in the Upper Cretaceous rocks of North America.

The great majority of the fish remains are Selachian, and comprise twenty-four species. Of these, *Carcharodon rondeletii*, *Otodus obliquus*, and *Odontaspis acutissimus* are regarded as indicating a Tertiary fauna, but in the Scandinavian chalk they have been found asoociated with many undoubted Cretaceous forms in the Faxe limestone or chalk. The character and extent of this fauna indicates conditions very similar to those accompanying the deposition of the English and French chalk and of that of Central Europe generally, whilst it affords comparatively few data for comparison with that of Lebanon. The occurrence of numerous teeth of several species of Scapanorhynchus in the Swedish area is worthy of note, but the fish are not found preserved bodily as they are in the chalk of Lebanon.

This memoir is published as Part vi. of vol. iv. of the Transactions of the Royal Dublin Society, and is illustrated with an atlas of nine plates.

SOCIETIES AND ACADEMIES.

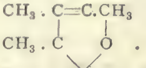
LONDON.

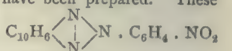
Royal Society, May 28.—"On the Bases (Organic) in the Juice of Flesh. Part I." By George Stillingfleet Johnson, M.R.C.S., F.C.S., F.I.C. Communicated by Prof. G. Johnson, F.R.S.

The author has endeavoured to ascertain by careful experiments how far the substances hitherto prepared from flesh are true "*educts*," and really present in the flesh itself, or merely products, due to (1) the action of chemical or physical agencies applied in the course of extraction, or (2) to bacterial action modifying the composition of the flesh before it comes into the hands of the operator.

The final conclusion drawn is that sarcous kreatine is not present in fresh muscle, but results from bacterial action whereas sarcous kreatinin is probably a true "educt."

Chemical Society, May 7.—Dr. J. H. Gladstone, F.R.S., Vice-President, in the chair.—The following papers were read:—The action of alkalis on the nitro-compounds of the paraffin series, by W. R. Dunstan and T. S. Dymond. The paper contains the results of further investigation of the interaction of alkalis and nitroethane, of which a preliminary account has already been given (Chem. Soc. Proc., 1888, p. 117). Nitroethane and alkali carbonates in the cold interact to yield carbon dioxide, and the alkali derivative of nitroethane, which is obtained when alkali hydroxide is employed. Ammonia combines with nitroethane in the cold to form a crystalline compound, analogous to the potassium and sodium derivatives. The action of alkalis proceeds further on warming, and there are formed alkali nitrite, acetonitrile, and a compound, boiling at 171°, and solidifying to a crystalline mass when cooled to 3°·5. The authors find that this compound is trimethylisoxazole



It is very stable, and is almost unaffected by heating in closed tubes with strong acids and alkalis. Permanganate oxidizes it to acetic acid, and nitric acid to acetic and oxalic acids. By reducing-agents it is slowly decomposed with formation of ammonia, acetic acid, and secondary butyl alcohol. By the action of sodium on a well-cooled moist ethereal solution, a *dihydride* $C_6H_{11}NO$ (m.p. 110°) is formed, which, when heated with water, is decomposed into ammonium acetate and ethyl methyl ketone. The *mercurichloride* has the formula C_6H_9NO, Hg_2Cl_2, and the *aurichloride* the formula $C_6H_9NO, AuCl_3$. Nitropropane, when acted on by alkalis, yields triethylisoxazole, propionitrile, and alkali nitrite, but the reaction occurs with greater difficulty than in the case of nitroethane. Nitromethane is readily acted on by alkalis, and hydrogen cyanide, alkali nitrite, and much resin are formed. The parent isoxazole could not be isolated. Secondary nitropropane is attacked with difficulty by alkalis, and no isoxazole is formed.—Some new addition compounds of thiocarbamide which afford evidence of its constitution, by J. E. Reynolds, F.R.S. Thiocarbamide combines with ammonium bromide, iodide, and chloride at the temperature of boiling alcohol, and forms compounds of the type $(H_4N_2CS)_4H_4NR'$. Under the conditions specified no compounds were obtained containing less than four molecular proportions of the amide to one of the ammonium haloid salt. Methyl-, ethyl-, allyl-, phenyl-, diphenyl-, and acetylphenyl-thiocarbamides do not yield compounds with ammonium bromide at the temperature of boiling alcohol. Thiocarbamide and tetrethylammonium bromide and iodide yield compounds of the type $(H_4N_2CS)_2Et_4NR'$. Under the experimental conditions, no well-defined substance was obtained containing more than two molecular proportions of the amide to one of the tetrethylammonium salt. Thiocarbamide and diethylammonium bromide form the compound $(H_4N_2CS)_3Et_2H_2NBr$. Thiocarbamide, when treated with triethylammonium bromide yields a mixture of the two compounds $(H_4N_2CS)_3Et_3HNBr$ and $(H_4N_2CS)_2Et_3HNBr$. With methylammonium bromide the amide forms the compound $(H_4N_2CS)_4MeH_3NBr$. It does not, however, combine with ethylammonium bromide, and when heated with the salt in the molecular proportions 4 : 1 at 135° in a sealed tube, together with alcohol, it yielded ethyl oxide and tetrathiocarbamidammonium bromide. The author points out that these facts supply evidence against the symmetrical constitution of thiocarbamide $CS(NH_2)_2$, and altogether in favour of the unsymmetrical constitution $HN : C(SH)NH_2$.—The action of acetic anhydride on substituted thiocarbamides; and an improved method for preparing aromatic mustard oils, by E. A. Werner, Trinity College, Dublin. The action of acetic anhydride on diphenyl-, ortho-, meta- and para-ditolyl-, meta-dixylyl-, dibenzyl- and diethyl-thiocarbamides has been studied. In the case of the aromatic derivatives, no acetylated derivatives of the thiocarbamides were produced. The solution of the thiocarbamide in acetic anhydride is accompanied by simultaneous decomposition into "anilid" and mustard oil in accordance with the equation $CS(NHR)_2 + (CH_3CO)_2O = CH_3CONHR + R.NCS + CH_3COOH$. When the solution is heated for five minutes at the boiling-point of acetic anhydride, an almost theoretical yield of mustard oil is obtained. Prolonged heating produces a secondary reaction expressed by the equation $R . NCS + CH_3COOH = CH_3CONHR + COS$. In the case of fatty thiocarbamides a well-defined acetylated thiocarbamide is first produced, and prolonged heating gives rise to the formation of mustard oil, but the yield of the latter is never high, and as final product a substituted amide is produced.—The decomposition of silver chloride by light, by A. Richardson. When pure silver chloride is exposed to light under water oxygen is evolved, part of which is present as ozone; when small quantities of water are present, chlorine and hydrogen chloride are found in solution; with large quantities of water, hydrogen chloride, but no chlorine, is found. The influence of hydrogen chloride in retarding the decomposition of silver chloride is considered, and is explained from experimental results given, which show that even minute quantities of hydrogen chloride exercise a marked influence on the stability of chlorine water when exposed to light, the rate of decomposition of the silver chloride being dependent on the readiness with which the chlorine in solution and water interact to form hydrogen chloride. The author describes experiments which show that the darkened product obtained by exposure of silver chloride to light contains no oxygen, and he concludes that it is of the nature of a sub-chloride rather than an oxychloride.—The addition of the elements of alcohol to the ethereal salts of unsaturated acids, by T. Purdie and W. Marshall. The authors record the results of experiments on the addition of the elements of alcohol to ethereal salts of fumaric and maleic acids by the agency of small quantities of sodium alkylate; they also describe a series of experiments with other ethereal salts, the object of which was to ascertain if the ethereal salts of unsaturated acids in general are capable of undergoing the same additive change. By the action of a small quantity of sodium methylate in the cold, on a mixture of methylic alcohol and methylic fumarate, an almost theoretical yield of methylic methoxysuccinate is obtained. Methyl fumarate, on heating with alcoholic sodium methylate, yielded a compound of the formula $C_{11}H_{12}O_7$, formed by the abstraction of 3 mols. of methyl alcohol from 2 mols. of methylic methoxysuccinate. Under similar conditions methylic amylate gave methylic methylpropionate. Methylic and ethylic crotonate gave methylic methoxybutyrate and ethylic ethoxybutyrate. The authors think that the alkyloxy-group attaches itself to the β-carbon atom. Ethylic methacrylate also formed additive compound; but pure products were not obtained from the reaction. Ethylic angelate, ethylic allylacetate, methylic and ethylic cinnamate and ethylic *o*-(β) ethylcumarate do not undergo additive change.—Notes on the azo-derivatives of β-naphthylamine, by R. Meldola, F.R.S., and F. Hughes. The authors have completed the series of azo-derivatives obtainable from the nitranilines and β-naphthylamine by preparing orthonitrobenzene azo-β-naphthylamine. The latter by the action of nitrite in a warm acetic acid solution gives orthonitrobenzeneazo-β-naphthol. In cold acetic acid solution the naphthyl acetate is formed. Acetyl derivatives of the ortho-, meta-, and para-nitroazo-derivatives of β-naphthylamine have also been prepared. The pseudazimides from the para- and metanitro-compounds have been prepared. These compounds have the formula  $C_{10}H_6\left\langle\begin{matrix}N\\ \vdots\\ N\end{matrix}\right\rangle N . C_6H_4 . NO_2$ (*p* or *m*). The action of aldehydes on these β-naphthylamine azo-derivatives gives rise to the formation of triazines, which are being investigated.—A method for the estimation of nitrates, by G. McGowan, Ph.D. This estimation is based on the interaction $HNO_3 + 3HCl = NOCl + Cl_2 + 2H_2O$. The gaseous products are led into a solution of potassium iodide.—New benzylic derivatives of thiocarbamide, by A. E. Dixon, M.D. A re-examination of "monobenzyl-thiocarbamide" has shown that the substance hitherto bearing this name is benzylamine thiocyanate; the latter can be converted into the isomeric thiocarbamide by heating for a short time at 150°–160°. The author describes a great number of benzylic derivatives of thiocarbamide.

Linnean Society, May 24.—Anniversary Meeting.—Prof. Stewart, President, in the chair.—The Treasurer presented his Annual Report duly audited, and the Secretary having announced the elections and deaths during the past twelve months, the usual ballot took place for new members of Council, when the following were elected: Messrs. C. B. Clarke, G. B. Howes, Arthur Lister, St. G. Mivart, and F. W. Oliver. The President and officers were re-elected. The usual formal business having been transacted, the President proceeded to deliver his annual address, taking for his subject "The Secondary Sexual Characters of Animals and Plants," of which he gave several interesting examples, illustrating his remarks with graphic sketches in coloured chalks. On the motion of Mr. H. Druce, seconded by Mr. C. Tyler, a vote of thanks was accorded to the President for his able address, with a request that he would allow it to be printed.—The Linnean Society's Gold Medal for the year 1891 was then formally awarded to Dr. Edouard Bornet, of Paris, for his researches in botany, and on his behalf was presented to M. Raymond Lecomte, Secretary to the French Embassy. The proceedings then terminated.

Institution of Civil Engineers, May 26.—Annual General Meeting.—Sir John Coode, K.C.M.G., President, in the chair.—In the Report of the Council for the session 1890–91, it was remarked that the salient feature of the session, now terminated, had been the realization of a proposal made more than forty years ago—namely, the formal reception by the President and Council on stated evenings after the ordinary meetings of the members and visitors then present. A series of receptions was held after the ordinary meetings in the months of January, February, March, April, and May, of this year. An endeavour has also been made to identify, in some degree, each gathering with a particular branch of engineering, both in respect to those invited to be present and to the models and other objects of interest exhibited. These receptions were believed to have been most successful, and experience would doubtless suggest directions in which they might be rendered still more useful and attractive in the future. The effective increase in the roll of the Institution during the past year was 247. The number of members of all classes, students excepted, on March 31 last, was 5150, as against 4903 on the same day last year, representing an increase at the rate of 5 per cent.—The adoption of the Report was moved, seconded, and carried, and it was ordered to be printed in the Minutes of Proceedings. Cordial votes of thanks were then passed to the President, to the Vice-Presidents and other Members of the Council, to the Auditors, to the Secretaries and Staff, and to the Scrutineers.—The ballot for Council resulted in the election of Mr. George Berkley as President; of Mr. H. Hayter, Mr. A. Giles, M.P., Sir Robert Rawlinson, K.C.B., and Sir Benjamin Baker, K.C.M.G., as Vice-Presidents; and of Mr. W. Anderson, D.C.L., Mr. J. Wolfe Barry, Mr. E. A. Cowper, Sir Jas. N. Douglass, F.R.S., Sir Douglas Fox, Mr. J. Clarke Hawkshaw, M.A., Mr. Charles Hawksley, Sir Bradford Leslie, K.C.I.E., Mr. George Fosbery Lyster, Mr. J. Mansergh, Sir Guilford Molesworth, K.C.I.E., Mr. W. H. Preece, F.R.S., Sir E. J. Reed, K.C.B., F.R.S., M.P., Mr. W. Shelford, and Mr. F. W. Webb as other Members of the Council.—The session was adjourned until the second Tuesday in November, at 8 p.m. [At the first meeting of the newly-elected Council, the following officers were re-appointed: Mr. H. L. Antrobus, as Treasurer; Dr. Wm. Pole, F.R.S., Honorary Secretary; and Mr. James Forrest, the Secretary.]

EDINBURGH.

Royal Society, May 4.—Sir Douglas Maclagan, President, in the chair.—A preliminary note by Mr. John Aitken, on a method of observing and counting the number of water particles in a fog, was communicated. The phenomena which are denoted by the names fog, mist, and rain, differ merely in degree, and not in kind. In a haze dry dust particles are present in the air to a greater or less extent. The haze turns into a fog when water vapour is condensed upon the particles, and the fog will develop into mist upon the condensation of a sufficient amount of moisture. So that we may regard an ordinary fog and a mist as a dry fog and a wet fog respectively. The water drops in a fog will gradually settle upon the exposed surfaces of bodies. Hence it might seem that, in order to determine the extent to which moisture is present in a fog, it would be sufficient to allow the drops to fall upon a piece of mirror, which they would soon wet. But Mr. Aitken has found that when exposed surfaces are quite dry, a great quantity of water drops are often present in the air. The drops are exceedingly small and evaporate with great rapidity from the surfaces (heated by radiation) upon which they fall. The instrument which Mr. Aitken has adopted for the purpose of determining whether or not water drops are present is essentially identical with his pocket-dust counter. It consists of a glass micrometer divided into squares of a known size, a spot mirror for illuminating the stage, and a strong lens or a microscope for observing the drops on the stage. It is found convenient to observe an area of the stage equal to about 1/16th or 1/20th square centimetre when working with a magnifying lens. In one fog which was observed, objects at a greater distance than 100 yards were quite invisible, and the surfaces of exposed objects were quite dry. The number of drops which fell per minute varied greatly, sometimes reaching 3000 per square centimetre, sometimes only 300 per square centimetre. Two days later the same apparent external conditions regarding fog again obtained, and the number was found to be 1300 per square centimetre per minute—which remained fairly constant until the fog began to clear off when it slowly diminished. In both cases the observation was made at 10 a.m. If the stage be slightly heated, the drops never reach the surface but evaporate in the layer of heated air over it. Mr. Aitken has also modified this apparatus in order to admit of the counting of the number of drops which fall from a column of air of known height. A low power microscope is used, and so a column of air 5 centimetres long can be obtained over the stage. Underneath, and concentric with the microscope, a tube 5 centimetres long and 4 centimetres in diameter is mounted. The top and bottom of this tube can be simultaneously closed by covers which turn on an axis parallel to the axis of the tube. A micrometer, illuminated by a spot mirror, is fixed in the centre of the bottom, and, in the centre of the upper cover, a small opening which corresponds to the lens of the microscope is made. The former instrument may be used to observe the larger particles of dust in the atmosphere.—Dr. J. M. Macfarlane read a paper, illustrated by lantern demonstrations, on a comparison of the minute structure of plant hybrids with that of their parents. He finds that the minute structure of the hybrid, like the larger features, is always intermediate in character between the corresponding structures of the parents.

PARIS.

Academy of Sciences, May 25.—M. Duchartre in the chair.—Researches on the camphene series, by MM. Berthelot and Matignon.—Researches on the vapour-tension of saturated water-vapour at the critical point, and on the determination of this critical point, by MM. Cailletet and Colardeau. In a recent note (*Comptes rendus*, vol. cxii. p. 563, 1891) the authors communicated to the Academy a new method for determining critical temperatures and pressures. They now give the results obtained in the case of water. Six series of experiments with different weights of water indicate that the critical temperature is 365° C., the critical pressure which corresponds to this being 200·5 atmospheres.—On the analysis of the sunlight diffused by the sky, by M. A. Crova. If B be the intensity of the blue light diffused by the sky, and S the intensity of incident sunlight, it may be shown that $\frac{B}{S} = 100\left(\frac{565}{\lambda}\right)^n$, where 565 represents the wave-length of the maximum light intensity of the spectrum, and n is an empirical coefficient. M. Crova calculates values with $n = 4$ and $n = 4·5$, and finds that, although Lord Rayleigh's observations (*Phil. Mag.*, 1871, p. 107) are best in accord in the former case, his own observations at Montpellier give results which are better represented when the latter value of n is used.—On the relative age of the Quaternary stratum of Mont Dol (Ille-et-Vilaine), by M. Sirodot. The author's observations lead him to believe that the *débris* on Mont Dol belongs to an epoch anterior to the movement which in Quaternary times elevated the coasts of certain regions of the Baltic Sea.—On the exact determination of the glycolitic power of the blood, by M. R. Lépine and Barral.—Observation of the passage of Mercury across the sun's disk on May 9, 1891, made with the Plœssl equatorial at the National Observatory of Athens, by M. D. Eginitis. The internal contact of egress occurred at 18h. 17m. 20s., and external contact at 18h. 22m. 0s. (Athens mean time). The irradiation phenomenon known as the "black drop" was not observed.—The atmospheric conditions of Greenwich with regard to the universal hour question, by M. Tondini. The cloudy state of the Greenwich sky, and the many rainy days recorded at the Observatory, are adduced as arguments against the adoption of Greenwich as the prime meridian. The meridian of Jerusalem-Nyanza is said to possess numerous atmospheric and other advantages.—On the algebraic integration of differential equations of the first order, by M. Painlevé.—On the determination of the integrals of equations from derived partials of the first order, by M. J. Collet.—On Abelian equations, by M. A. Pellet.—Researches in thermo-electricity, by MM. Chassagny and Abraham.—Determination of the solar constant, by M. R. Savélief. From an actinometric curve obtained on December 26, 1890, the author obtains for the solar constant, reduced to the mean distance of the sun from the earth, the value 3·47 calories. Langley's value, from his Mount Witney observations, was 3·0 calories.—On the fluctuations in the heights of lake waters, by M. P. du Boys. In lakes, and particularly in the Lake of Geneva, the surface of water regularly rises in one part and lowers in another, performing an oscillatory movement. The region where the level is practically constant is called the node, and the movements referred to go by the name of *seiches*. The author investigates this wave-motion mathematically.—On a new portable sounding-apparatus of steel wire, by M. Émile Belloc.—Study of the barometric gradient, by M. G. Guilbert. Some remarkable relations between the force of the wind and the barometric gradient are given.—Relation between atomic weight and the density of liquids, by M. Al. Moulin.—On the sub-chloride of silver, by M. Guntz. Under the action of heat, the sub-chloride decomposes into silver and silver chloride. This decomposition is easily seen by the change of colours of the sub-chloride. Dilute nitric acid has absolutely no action upon the compound. With hot concentrated nitric acid, chloride of silver mixed with the sub-chloride is obtained. Potassium cyanide rapidly dissolves the compound, and decomposes it. Utilizing this reaction, the author has found that a given weight of chlorine disengages practically the same amount of heat (29 calories), when combining with Ag as when combining with Ag_2.—Action of potassium salts upon the solubility of potassium chlorate, by M. Ch. Blarez.—Electrolysis of the fused salts of boron and silicon, by M. Adolphe Minet. Some interesting experiments indicate that, by the electrolysis of white and red bauxites, it is possible to produce a series of alloys of iron, silicon, and aluminium, and, at the end of the operation, to obtain aluminium chemically pure.—On two new crystalline compounds of platinic chloride with hydrochloric acid, by M. Léon Pigeon. The compounds described have the composition $PtCl_4 . 2HCl . 4H_2O$ and $PtCl_4 . HCl . 2H_2O$.—On salicylate of bismuth, by M. H. Causse.—On the heat of solution and the solubility of some bodies in methyl-, ethyl-, and propyl-alcohols, by M. Timofeiew.—On the Stelleridæ found in the Bay of Biscay, at the Azores, and Newfoundland during the scientific expeditions of the yacht *Hirondelle*, by M. Edmond Perrier.—On the equivalence of the bundles in vascular plants, by M. P. A. Dangeard.—On the trappean formation of Toungouska Pierreuse, Siberia, by M. K. de Kroustchoff.—Researches on the elimination of oxide of carbon from the system, by M. L. de Saint-Martin.

Melbourne.

Royal Society of Victoria, March 12.—The following officers were elected for the year 1891:—President: Prof. Kernot. Treasurer: C. R. Blackett. Secretaries: H. K. Rusden and Prof. W. Baldwin Spencer.—The following papers were read:—A new species of Dictyonema, by T. S. Hall.—A preliminary account of *Synute pulchella*, by Arthur Dendy. This is a new genus and species of calcareous sponge, which is allied to Ute, but in which the individuals are fused together into a common mass.—The geology of the southern portion of the Moorabool valley, by T. S. Hall and G. B. Pritchard.

April 9.—On the occurrence of the genus Belonostomus in the Rolling Downs formation (Cretaceous) of Central Queensland, by R. Etheridge, Jun., and Arthur Smith Woodward, of the British Museum. This is described as a new species, under the name of *Belonostomus sweeti*.—Note on a new genus of Chætopod worm parasitic on a sponge of the genus Rhaphidophlus from Port Phillip, by Prof. W. Baldwin Spencer. The worm is remarkable in having the dorsal surface covered with a series of rows of setæ, each row enclosed in a membranous web, the bunches of setæ on the feet are also enclosed in webs.

Göttingen.

Royal Academy of Science.—In the Journal of the Scientific Academy of Göttingen, the following papers of scientific interest appear (July to December, 1890):—

July.—Fr. Pockels: On the interference phenomena of convergent homogeneous polarized light through twin-plate uniaxal crystals.—Voigt: Determination of the elastic constants of Brazilian tourmaline.

August.—Julius Weingarten: On particular integrals of Laplace's equation, and a class of fluid motions connected with the theory of minimum surfaces.—Venske: A modification of Hermite's first proof that *e* is transcendental.—Riecke: Special cases of equilibrium of a system having several phases.—Meyer: Discriminants and resultants of singularity equations (second notice).—Burkhardt: An equation in the theory of the theta-functions.—Klein: On the zero-points of the hypergeometric series.

October.—Nernst: On the distribution of a substance between two solvents.

December.—Riecke: The thermal potential of weak solutions. On electricification by friction.—Meyer: On discriminants and resultants of singularity equations (third notice).—Voigt: On the vibrations of strings.—Riecke: Molecular theory of diffusion and electrolytic conduction.—Hurwitz: On the zero points of the hypergeometric series.—Voigt: Determination of the constants of elasticity of several non-crystalline minerals.—Auerbach: On hardness and its absolute measurement.

Stockholm.

Royal Academy of Sciences, May 13.—The elements of the hydrography of the Kattegatt and Skagerack, by Prof. O. Pettersson.—Studies on the Solenogastres; i. monograph of *Chætoderma nitidulum*, by Dr. A. Wirén.—Researches on the fossil wood of Sweden, by Dr. Conwentz, in Danzig.—Prof. S. Lovén gave a report on the work executed during the last summer at the zoological station of Kristineberg in Bohuslän, Sweden, and reviewed a paper by Dr. C. Aurivillius on the symbiosis between Pagurus and Hydractinia as well as another by Dr. Wirén on *Chætoderma nitidulum*.—Researches and observations on the method of Koch in treating tubercular diseases by Prof. Bruzelins.—A copper-plate engraving of a map of the world made in the beginning of the fifteenth century, formerly belonging to the museum of Cardinal Borgia in Velletri, described by Baron A. E. Nordenskiöld.—Studies on the brain of teleosteous fishes, by Herr G. Andersson Malme.—A final contribution to the flora of the Chlorophyllophysæ of Siberia, by Herr O. F. Borge.—On phen-ethyl-propyl and phenyliso-propyl-triazol combinations by Dr. T. A. Bladin.—On the specific heat of water between 0° and + 40°, by Herr A. M. Johansson.—A few formulæ to calculate the mortality among annuitants of public offices and private societies by Dr. G. Enström.—A comparison between the methods of Ångström and Neuman for determining the conductibility of heat in bodies; ii. experimental researches, by Dr. Hagström.—Hydrographical researches in the Gullman fiord during the summer of 1890, by Miss A. Palmqvist.

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THURSDAY, JUNE 11, 1891.

MAMMALS LIVING AND EXTINCT.

An Introduction to the Study of Mammals Living and Extinct. By W. H. Flower and Richard Lydekker. (London: Adam and Charles Black, 1891.)

THIS work is, as the authors inform us in the preface, based largely upon the article Mammalia, together with forty shorter articles, written by Prof. Flower for the ninth edition of the "Encyclopædia Britannica." Certain other articles by Dr. Dobson and Dr. St. George Mivart have also been made use of. The illustrations, most of which are admirable, are chiefly those prepared for the "Encyclopædia," but many have been added. Mr. Oldfield Thomas, of the British Museum, has assisted the authors in special matters. To Mr. Lydekker fell the task of arranging the various articles made use of in proper sequence, filling up gaps and adding new matter, a large amount of which treats of the extinct forms.

The book resulting from this process is undoubtedly one which will be found interesting and useful by all students of zoology. There is a great deal in it which is worth reading; especially so are the four introductory chapters on general anatomical characters, origin and classification of the Mammalia, and on geographical and geological distribution. Moreover, with regard to important animals such as the horse, sheep, goat, pig, a great deal of accurate information of varied character is given. The whales are, as we should expect, treated with special mastery, and throughout the book we come upon pages which are models of lucid statement and judicious selection of matter.

It should, however, be clearly understood that the book is not and does not profess to be a complete work of reference on the Mammalia. The references to extinct groups are exceedingly scanty, and whilst they serve to stimulate the reader's desire for further information, do not, as a rule, furnish him with the titles of original works in which such information is to be found. The bulk of the work consists of chapters treating of the orders of Mammalia in systematic sequence, and there is no doubt that, both for the general reader and for the more technical zoologist, they form a mine of valuable information well up to date, and as a rule well set forth by the aid of illustrations. As an example of the latter, I may refer to the excellent woodcuts of the skull of Tritylodon from the Trias of South Africa, and of various lower jaws illustrating the section on Mesozoic Mammalia; but additional figures of this most important and little known series of forms would have been welcome, and one reads with unfeigned disappointment the declaration that "it would be beyond the scope of the present work to describe in detail, or even to mention the names of, all the members of this group."

There are one or two points of general interest in the earlier chapters to which I may briefly call attention.

The view originally formulated by Huxley, that in looking among Vertebrates for the progenitors of the Mammalia we must pass over all known forms of birds and reptiles and go right down to the Amphibia, is maintained by the authors, whilst reconciling this conclusion with Prof. Cope's important observations on the remarkable resemblances which obtain between the extinct reptiles known as Theromorpha (Theriodontia, Pelycosauria) and the Monotreme Mammals. Recent observations have shown such an intimate connection between the South African Theromorpha and the Labyrinthodont Amphibians that there can, our authors maintain, be no hesitation in regarding the one group as the direct descendant of the other, and "we may probably regard the Mammalia as having originated from the same ancestral stock at the time the Amphibian type was passing into the Reptilian."

In reference to classification, the authors observe that it is a simple matter to indicate natural groups, such as orders and sub-orders, among existing Mammals, but when we pass to the extinct world all is changed. New forms are discovered which cannot be placed within any of the existing divisions. "Our present divisions and terminology are," say Prof. Flower and Mr. Lydekker, "no longer sufficient for the purpose [of a classification which shall embrace extinct forms]; and some other method will have to be invented to show the complex relationships existing between different animal forms when viewed as a whole." Apparently the authors mean, by the last five words of this sentence, "when all are viewed together." The necessity for drawing up lists and catalogues in a linear series is deplored, but unhappily no attempt is made by the authors to grapple with the difficulty. A classification of the Mammalia in a linear series is given as far as families; and the names of groups containing only extinct forms are printed in special black type. I should have been very glad to see some attempt to set forth in the form of genealogical trees the senior author's views on the genetic relationships of this confessedly artificial linear series. I cannot admit that the division of the Mammalia into three groups—Prototheria, Metatheria, and Eutheria, or, as De Blainville called them, Ornithodelphia, Didelphia, and Monodelphia—expresses a natural fact, if these three groups are regarded as equipollent, and as succeeding one another as three "grades" of evolution. It is not difficult to come nearer to an expression of actual genealogical relations than this. It appears preferable to divide the Mammalia primarily into two grades: (A.) the Monotrema, and (B.) the Ditrema; only so do we give expression to the wide gap by which the archaic characteristics of the Monotremes separate them from all other Mammals. Then we can divide the Ditrema—not into two successive *grades* of structure—but into two diverging *branches*, viz. Branch *a*. Marsupialia, and Branch *b*. Placentalia. Of the Placentalia our authors say that their affinities with one another are so complex that it is impossible to arrange them serially with any regard to natural affinities. They might, however, it seems to me, embody their own conclusions in classificatory form, and divide the Placentalia into four diverging sub-branches, the chief being (*a*) the Typidentata, the three others being (*b*) the Edentata, (*c*) the Cetacea, and (*d*) the Sirenia. The group which I call Typidentata our authors actually define, though they do not name it and use it as would surely be convenient. They say, "The remaining Eutherian Mammals [*i.e.* Placentals after exclusion of Edentata, Cetacea, and Sirenia] are clearly united by the characters of their teeth, being all heterodont and diphyodont with their dental system reducible to a common formula." I have for many years made use in my lectures of the classification of Mammalia above indicated which may be summarized thus:—

Class MAMMALIA.

Grade A. MONOTREMA.
Grade B. DITREMA.

Branch *a*. Marsupialia.
  Sub-br. *a*. Polyprotodontia.
  Sub-br. *b*. Diprotodontia.
Branch *b*. Placentalia.
  Sub-br Sirenia.
  Sub-br. *b*. Edentata.
  Sub-br. *a*. Typidentata.
  Sub-br. *d*. Cetacea.

No doubt it is difficult, even with the use of the additional terms "grade," "branch," and "sub-branch," to set forth the relations to one another of the known orders and sub-orders of Typidentata; but the attempt must be made, and there are materials in the present work for gathering some indications of the form which such a tentative pedigree would take had the authors gone so far as to formulate it.

In the chapter on geographical distribution, the six zoological regions of the globe proposed by Dr. Sclater in 1857 are accepted. But here, as in regard to the treatment of morphological groups, it seems that a primary grouping of the divisions recognized might with advantage be introduced, which would give a truer expression of the historic relations of existing land surfaces than that adopted. Reference is made to the proposed elevation of New Zealand into a primary region, but would not the truth be more nearly expressed by separating New Zealand and the rest of the world first of all, as Atheriogæa and Theriogæa? Should not the Australian region next be separated from the rest of Theriogæa? Theriogæa would then be divided into the Terra Marsupialium and the Terra Placentalium (without prejudice to the recognition of the occurrence of a limited number of Marsupials in the latter). The Terra Placentalium includes the five regions called by Sclater Palæarctic, Nearctic, Neotropical, Ethiopian, and Indian. The authors of the present work mention Dr. Heilprin's opinion that the Palæarctic and Nearctic regions should be united and called the Holarctic region. But they do not adopt this opinion, nor refer to Huxley's proposal to term this same area Arctogæa, and his suggestive speculations as to the successive connections of the three great peninsulas (as they are at present)—the Neotropical, the Ethiopian, and the Indian—with this northern land surface.

I have ventured to cite one or two instances in which the methods of classification adopted in the "Study of Mammalia" appear to be open to improvement. I trust that I may without offence express a doubt as to what precisely is the meaning of the last part of the following passage:—

"The researches of palæontologists, founded upon studies of casts of the interior of the cranial cavity of extinct forms, have shown that, in many natural groups of Mammals, if not in all, the brain has increased in size and also in complexity of surface foldings with the advance of time, indicating in this, as in so many other respects, a gradual progress from a lower to a higher type of development."

I confess that I do not understand what this "lower" and "higher type of development" refer to. The remarkable thing about the small brains of extinct Ungulata is that, whilst they differ enormously in relative size and in the low development of other features from the brains of living Ungulates, their possessors exhibited no corresponding difference of skeletal structure; so that it appears that the brain has had an independent evolution, advancing in size and complexity from the initial phase of the primitive Ungulate far further than has the general body-structure. The gap in respect of brain between man and the highest apes, accompanied as it is by mere trivial differences of bodily structure, appears to be a less marked case of the same general phenomenon. We may say that the brain in the one case is in a lower and in the other in a higher stage of development; but whether the authors mean this merely, or that the whole animal has passed "from a lower to a higher type of development," and to what kind of morphological doctrine that phraseology belongs, are matters which do not immediately explain themselves.

The only way to write of so large, so comprehensive, and so authoritative a work as the present, is to point out a few matters for discussion which a rapid review of its pages suggests. Such indications of topics on which one would like to know more from the authors of a book of this kind are not fault-findings, but samples of the interest which it awakens in a sympathetic reader.

E. RAY LANKESTER.

FORTY YEARS IN A MOORLAND PARISH.

Forty Years in a Moorland Parish. By the Rev. J. C. Atkinson, D.C.L. (London: Macmillan and Co., 1891.)

THE moorland parish of which Dr. Atkinson writes is the parish of Danby, which lies among the Cleveland Hills, some miles inland from Whitby. Here he has worked as a clergyman for forty-five years. To a man of narrow sympathies and little intellectual curiosity the position might have been trying enough; but in the life of the people, in the aspects of Nature, and in local problems appealing to the antiquary and the historian, Dr. Atkinson has found sources of interest which have never lost their charm. In the present volume he records some reminiscences of the pursuits which have occupied him, and of the impressions which have been made upon him, during all these years; and a very fascinating record it is. He not only has powers of keen and accurate observation, but carries on his researches in a thoroughly scientific spirit; and he is a master of the difficult art of stating problems in a manner that secures attention while they are being gradually solved. His immediate subject is Danby; but if the author had never raised his eyes to look further afield, his readers might soon have felt that he had told them about as much as they wished to know. Facts relating to a particular locality can never be really understood unless they are brought into connection with kindred facts in other parts of the world. This is constantly borne in mind by Dr. Atkinson, and his ample learning enables him to apply the principle in many different ways; so that, when he is talking about Danby, he is often talking at the same time about wide regions of the British Islands, and even about stages of culture through which the greater part of the human race has passed.

One of the most interesting of the sections into which the book is divided is the one headed "Antiquarian." In Danby, as in Cleveland generally, there are many prehistoric burial-mounds, and a large number of these he has carefully excavated. The only traces of bronze he has discovered are "a few mouldering fragments of very thin plate, found with the unprotected bones of a cremated body, and not sufficient to fill a very small pill-box half an inch in diameter." Nevertheless, the contents of the larger "houes" prove conclusively that they belong to the Bronze Age; and Dr. Atkinson is of opinion that they date from the later part of the period. He has found many vases of the Bronze Age type, some jet beads, two polished axe-hammers, various bone pins, arrowheads and other objects of flint; and by far the larger proportion of these treasures may now be studied, along with similar treasures recovered elsewhere, in the British Museum. Dr. Atkinson tells with great spirit the story of the more memorable of his explorations; and he has much that is amusing to say about the wonder excited among his rustic neighbours by what seem to them his mysterious proceedings, and about the interest aroused in the minds of those whom he has from time to time induced to help him. Across the ridges between which lie the dales of the district are ancient earthworks, all of which "are defensive against attack from the south, and in no other direction whatever." Of these dykes, which seem to be of the same period as the burial-mounds, Dr. Atkinson gives a full and lucid account, and he offers some suggestive hints as to their relation to other old fortifications in the neighbourhood. He has also an excellent chapter on various pits which have often been described as the remains of "British settlements." There can be little doubt, as he shows, that in reality these pits are the remains of early mining excavations.

Another valuable part of the book is devoted to folklore. The belief in witches has not even yet wholly died out in Cleveland; and forty years ago it was still a more or less potent factor in the lives of the people. The author gives some curious instances of the power formerly attributed to witches, and of the means by which their devices were supposed to be thwarted by the "wise men" of the district. He suggests that witches may not always have been mere impostors, but that in some cases they may have been able to exercise the kind of influence to which the phenomena of hypnotism are believed to be due. Even more interesting than the traces of faith in witchcraft are the survivals of "fairy," "dwarf," and "Hob" notions. According to a tale told to Dr. Atkinson by an old woman, there was a farm in Glaisdale where Hob, so long as he was not spied upon, did much excellent work at night. At last some one was curious enough to watch him, and it was thought he would be all the better for "something to hap hissel' wiv." Accordingly a coarse shirt, with a belt or girdle to confine it round his middle, was made for him, and left in the barn where he worked. When he found the gift, Hob broke out in the following couplet:—

"Gin Hob mun hae nowght but a hardin' hamp,
He'll coom nae mair, nowther to berry nor stamp."

Dr. Atkinson was delighted with this couplet, for it preserves three words which had become obsolete forty years ago, and two of which—"berry" and "hamp"—had no actual meaning to the speaker. "Stamp" was the word for "the action of knocking off the awns of the barley previously to threshing it, according to the old practice." "Berry," meaning to thresh, he had been "looking and inquiring for, for years, and looking and inquiring in vain." As to "hamp," he had "never had any reason to suppose that it had once been a constituent part of the current Cleveland folk-speech." The hamp was a kind of smock-frock, gathered in about the middle and falling below the knee, and was at one time the characteristic garment of the English peasant. The word "seems to be clearly Old Danish in form and origin."

There are several chapters which will give pleasure to students of geology and ornithology; and in his notes on weddings, burials, the harvest-home, and holy wells, the author displays much ingenuity in detecting survivals of what were in past times wide-spread customs. In the interpretation of old historical documents, and in the purely descriptive parts of the book, he is equally successful. Some readers, finding so many things to lure them on from the beginning of the work to the end, may be disposed to think that Danby is a very exceptional parish. What is exceptional, however, is not so much the writer's subject as the knowledge and insight which enable him to appreciate, and to make others appreciate, its true interest and significance.

OUR BOOK SHELF.

Anatomy, Physiology, Morphology, and Development of the Blow-fly (Calliphora erythrocephala). Part II. By B. Thompson Lowne, F.R.C.S., F.L.S., &c. (London: R. H. Porter, 1891.)

THE general features of this study in insect anatomy have already been noticed (NATURE, vol. xliii. p. 77). Part II. describes the exoskeleton in considerable detail, and contains many useful and elaborate figures. Plate v. and the accompanying explanations give the author's views upon the morphology of the insect-head. The pre-oral part he regards as developed from three bladder-like swellings, to which correspond three primary divisions of the cephalic nerve-centres. The post-oral part is supposed to arise by the fusion of three jaw-bearing segments. The terminal portion of the proboscis is probably developed, according to Mr. Lowne, from the first, and not from the second pair of maxillæ. The description of the mouth-parts is very full, and the figures are extremely good.

The thoracic skeleton is also minutely described, perhaps over-minutely, seeing that, in our author's words, "a classification of the various sclerites indicative of their morphological significance is not possible with our present knowledge." Other careful descriptions by special students show that it is easy to interpret the complex thoracic structures in a different way from that here adopted.

Excellent figures are given of the legs, feet, and wings, and the description of the foot of the fly is of very special interest. The wing-joint is described with great care and thoroughness, in connection with the mechanics of flight.

Comparisons between insect and vertebrate structures are made with great boldness. One example will probably astonish common-place morphologists. Weismann observed that the femoro-tibial part of the fly's leg forms at first a mere lateral prominence, which is converted by segmentation and constriction into a bent knee, the upper part yielding the coxa and femur, the lower part the tibia. Mr. Lowne confirms this account, and illustrates it by figuring five stages (Fig. 34). Next he compares the lateral prominence to the exopodite of a biramous limb. Then he adopts Dr. Gaskell's suggestion that the limbs of an Arthropod may correspond to the visceral arches of a Vertebrate. In the following sentence we reach the climax. "The double character of the embryonic appendages in the Crustacea, and in the maxillæ of insects, as well as in the thoracic limbs of the rudimentary fly-nymph, is certainly very suggestive of the double character of the pterygomaxillary arch, or even of the hyomandibular in vertebrates."

So much conscientious labour has been bestowed upon this treatise, and it is so useful to the student of insect anatomy, that it is a pity to see the text encumbered with discussions which, to avoid dogmatism, we will merely call extremely hazardous. Would it not be better to bring out such views in another place, and leave the *magnum opus* free of doubtful matter?

When all deductions have been made, the book must be counted a valuble addition to the literature of the subject. L. C. M.

Races and Peoples: Lectures on the Science of Ethnography. By Daniel G. Brinton. (New York: N. D. C. Hodges, 1890. Sold by Kegan Paul, Trench, Trübner, and Co.)

THE lectures of which this book consists were delivered at the Academy of Natural Sciences, Philadelphia, early in 1890. They present a good general view of the leading principles of ethnography, as these are understood by the author. He begins with a discussion of what he calls the physical and psychical elements of ethnography, next treats of the beginnings and subdivisions of races, then takes in order the divisions in which he arranges the various groups of mankind, and finally deals with problems relating to "acclimation," amalgamation, and the influence of civilization on savages, and offers some suggestions as to the destiny of races. The human species seems to him to include five races—the Eurafrican, the Austafrican, the Asian, the American, and insular and littoral peoples. Each of these is subdivided into branches, stocks, and groups; and an effort is made to define the traits which, according to Dr. Brinton, the members of each race have in common. It is not always easy to understand the principle of his classification. The Eurafrican race, for instance, includes the following groups: Libyans, Egyptians, East Africans, Arabians, Abyssinians, Chaldæans, Euskarians, Indo-Germanic or Celtindic peoples, and peoples of the Caucasus. These peoples are all white; and Dr. Brinton thinks we may also say of them, "hair wavy, nose narrow." But the differences by which they are separated from one another are, at least in some cases, so profound, that it is extremely doubtful whether we are warranted in attributing to them a common origin, except in the wide sense in which a common origin is attributed to humanity generally. So long, however, as Dr. Brinton's classification is understood to be merely a convenient way of bringing together great masses of facts, it may be of considerable service to students. The book embodies the results of much careful research, and is written in a clear and vigorous style.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Crystals of Platinum.

SINCE writing a note on this subject to NATURE (vol. xliii. p. 541) I have found that it is by no means requisite to use topaz in order to obtain crystals of platinum from a ribbon of that metal heated by a current. Thus the ribbon may be dusted over with quartz dust, and if the temperature be raised to that at which this is slowly melting (1430° C. about), crystals of platinum gather upon projecting points on the quartz. Doubtless the presence of fluorine will facilitate, as described in my former letter, the volatilization of the platinum, but there is little doubt that at a temperature some 300° below its melting-point (1750°; Violle) there is a slow volatilization of the metal due either to heat pure and simple, or to this in conjunction with the presence of a current as in high vacua.

To put the possibility of chemical action out of the question, I weighed a clean ribbon of pure platinum, 9 centimetres in length, and passed such a current through it, for 30 minutes, as raised it to nearly the melting-point of palladium (1500°; Violle). The first weighing was 0·0700 grammes, the second (after heating) 0·0688, indicating a loss of 1·7 per cent. of its weight.

I find that Prof. A. S. Törnebohm, of Stockholm, has described in a recently-published paper (*Aftryck ur Geol. Fören. i Stockholm Förhandl.*, Bd. 13, Häft 2, 1891) cubical crystals of platinum formed by the action of chlorine gas upon platinum black at a high temperature. The figures illustrating his paper depict crystals similar to those obtained by the present method.

J. JOLY.

Physical Laboratory, Trinity College, Dublin.

Porpoises in the Victoria Nyanza.

IN Dr. Carl Peters's "New Light on Dark Africa," he speaks of "some large gray-bellied porpoises tumbling about" in Lake Victoria Nyanza, "and rollicking in the tepid flood" (see p. 445).

I should be glad to know whether there is any other authority for the occurrence of a Cetacean in this lake. It is possible, but very improbable, as no Cetaceans are known to occur in the Nile, or other African fresh waters, although there has been a report of the Manatee being found in the Shári, which runs into Lake Tchad (see Barth, "Reisen," iii. p. 289), and the Manatee also occurs in the Niger. P. L. SCLATER.

The Zoological Station at Naples.

IT is desirable that the names of any biologists who wish to make use of the British Association Table at the Naples Zoological Station, during the year commencing in September next, should be in the possession of the Committee before the meeting of the British Association at Cardiff.

Intending applicants are therefore requested to send in their names, and a statement of the nature of the work they propose to undertake, before June 30, to me as Secretary to the Committee. W. PERCY SLADEN.

13 Hyde Park Gate, S.W., June 6.

A BRITISH INSTITUTE OF PREVENTIVE MEDICINE.

ON Friday, June 5, Sir Michael Hicks-Beach received in one of the large rooms of the Victoria Hotel, Northumberland Avenue, an unusually numerous and influential deputation on behalf of the British Institute of Preventive Medicine. Sir Michael Hicks-Beach was accompanied by Sir Henry Calcraft, K.C.B., Secretary to the Board of Trade, Mr. Courtenay Boyle, C.B., and Mr. Walter J. Howell.

Among the members of the deputation were the Duke of Westminster, the Earl of Feversham, Sir Frederick Abel, Sir F. Bramwell, Sir John Lubbock, Sir Benjamin

Baker, Dr. Farquharson, M.P., Sir William Thomson, Sir James Bain, Sir Joseph Fayrer, Sir Philip Magnus, Sir Jacob Wilson, Prof. Dewar, Sir Douglas Galton, Sir Archibald Geikie, Sir William Houldsworth, M.P., Sir George Humphry, Mr. Haldane, Q.C., Mr. Seager Hunt, M.P., Sir Guyer Hunter, M.P., Prof. Ray Lankester, Prof. Norman Lockyer, Mr. Blundell Maple, M.P., Sir Lyon Playfair, M.P., Sir Robert Rawlinson, Sir Henry Roscoe, M.P., Sir George Gabriel Stokes, M.P., Prof. Burdon Sanderson, Sir Henry Trueman Wood, Prof. Victor Horsley, Dr. Armand Ruffer, Mr. Priestley, Sir Henry Simpson, and other members of the Royal, the Linnean, and other scientific Societies.

The following letters were read from Prof. Tyndall and Prof. Huxley :—

"*Hind Head, Haslemere, June* 3, 1891.

"MY DEAR SIR JOSEPH,—The battered remnant of four deadly assaults, I am still a prisoner in my bed. Were I a free man, I should deem it a privilege to join your deputation to Sir Michael Hicks-Beach on June 5. I entirely sympathize with the movement.

"Let me here record a small experience of my own. Last summer, while crossing from Dover to Calais on my way to the Alps, I noticed, huddled up in a corner of the steamer, a poor English boy. He seemed lonely and depressed, and I spoke to him. 'Where are you going, my boy?' I asked. 'To Paris,' was the reply. 'And what are you going to do in Paris?' 'Well, sir,' said he, 'I have been badly bitten by a mad dog, and I am now on my way to Mr. Pasteur, who I hope will save my life.'

"The case prompted sad and bitter musings. Here was wealthy England, with the amplest means at her disposal, with some of her ablest men ready to investigate and apply those means, insanely forbidding such investigation, and compelling her children to resort to a foreign country to have themselves rescued from the most horrible of deaths. As I spoke to the lad, the virulent rabic virus was probably already in his blood, and his chance of life depended on the promptness with which Pasteur's vaccine could be introduced to combat and destroy that virus. Every hour lost in the collection of money for the boy's journey and in making arrangements with Pasteur for his reception—every hour lost in his transport from England to France—was so much time given to the virulent virus to pursue its fatal work, and to ruin the chances of the boy's rescue. This is the state of things to which we in England are forced to submit; this is the condition to which we are reduced, through the deference paid by English statesmen to a noisy and an ignorant faction.

"But while the investigation and treatment of hydrophobia confer immortal honour on Pasteur, this malady is but a small item in the array of disorders now demanding investigation. Suspected from time to time by men of genius in the past, the fact that all communicable diseases are due to micro-organisms, which increase and multiply after the manner of living things, has, in the opinion of our first authorities, been now reduced to demonstration. Your proposed institute is to be devoted to the investigation of such organisms—to the study, that is, of the science of bacteriology. In regard to questions of life and health, such an institution is the most pressing need of England at the present hour. A good deal of the weary time which I have been forced to spend in bed during the last six months has been devoted to making myself acquainted with what is being done by the staff of the Hygienic Institute of Berlin, an institute of which the German nation may well be proud. I have occupied myself in drawing up an account of the researches recently carried out in connection with the institute. In regard to our most fatal disorders, these researches will effect a revolution, not only in public knowledge, but also in the thoughts and practice of medical men. It would, in my opinion, be a lamentable mistake on the part of an English statesman to place himself in official antagonism to the eminent and illustrious men who on June 5 will advocate the founding of a similar institute in England.

"It is, I think, fortunate that you have in Sir Michael Hicks-Beach a statesman not likely to fall into the extravagances of sentimentalism. The overwhelming preponderance of English intellect will be represented by the deputation. He may rest assured of it that this preponderance will become more and more conspicuous, until finally the misguided opponents of a true philanthropy will cease to engage the attention, much less enlist the sympathy, of the English people.

"Believe me, dear Sir Joseph, most faithfully yours,

"JOHN TYNDALL.

"Sir Joseph Lister, Bart."

"*Hodeslea, Eastbourne, June* 2, 1891.

"DEAR SIR JOSEPH LISTER,—I am very sorry that I am unable to join your deputation on June 5.

"If I could have been with you, I think I should have asked to be permitted to point out to the President of the Board of Trade that medical science is not excepted from the rule which holds good for other branches of natural knowledge, and that it can be advanced only by reasoning based upon observation and experiment, and constantly controlled by both, especially by the latter.

"Further, that by working in this fashion a marvellous improvement of medical science has been effected during the last half-century, and that the harvest of what Bacon called 'fruits,' which is now waiting for the gatherer, might fully occupy half a dozen such institutes as that in which we are interested.

"Starting from the unquestionable facts that the work we propose to undertake is of supreme public utility, and that the number and extent of the problems of pathology are enormously great in proportion to any existing means of dealing with them, I should have ventured to ask why we should be refused the only privilege we seek—namely, that official recognition by the Board of Trade which will afford the institute security against the possible misuse of its funds in future?

"No doubt, however, all these points will be much more effectually put by yourself and other members of the deputation.

"I am yours very faithfully,

"T. H. HUXLEY.

"Sir Joseph Lister, Bart., F.R.S."

Sir Henry Roscoe, M.P., in introducing the deputation, said that it represented not only the whole body of medical men in this country, but also, without exception, all the scientific elements amongst scientific men, and also a large number of others who were interested from the national point of view in the establishment of an institute of preventive medicine for this country, and for which it was proposed to obtain incorporation under the Board of Trade. He need not go into the question as to the national importance of an institution of this kind. There was no civilized country in Europe, and scarcely anywhere else, in which this subject had not awakened the interest and claimed the attention, not only of the scientific men, but also to a great extent of the Governments of those countries. What they asked was that Sir Michael Hicks-Beach would be good enough to enable them to found and to carry on a British Institute of Preventive Medicine, analogous and of a similar form to those great institutes which existed in France, Germany, Russia, and in a great number of other countries. They were sorry to find that the object which they had in view and the request that they made to Sir Michael had not met altogether with the success which they had hoped. They learnt from the answer which he had given to Major Rasch in the House of Commons that the refusal to grant what they requested was based on objections received by the Board of Trade. They merely asked that the institution should be registered under the Limited Company Act, with the omission of the word "limited," in order to impress the public with the fact that the institute was not established for the purpose of gain, but purely for sanitary and scientific objects. The objections were based upon the fact that a part of the work would include experiments on animals. In reply to this they had the opinion of counsel that the Board of Trade had only to satisfy themselves that the object was charitable, and that the promoters were persons whose position was a sufficient guarantee of the high character of the proposed institute.

Sir Joseph Lister said the object of their deputation was to request Sir Michael Hicks-Beach to reconsider his decision, and to grant the licence under the Board of Trade which was really, as it would seem, almost essential to the prosperity, if not indeed to the very existence, of the institute. It was essential, in order that they might hold money in trust, that they should be incorporated. They had been promised a large sum of money, the receipt of which would be essentially dependent upon their incorporation, and if they were incorporated as a limited liability company they would not be able to appeal to the public for funds with any success. In the first place, their institution would have a mercantile character, which would tend to repel subscribers; and they had the opinion of counsel that under such circumstances it would be in the power of the subscribers at any time to agree to have the institute wound up and the funds divided amongst themselves. To appeal to the public for subscriptions, therefore, under these conditions would be absolutely hopeless. On the other hand, if the licence were granted there could be inserted by the Board of Trade a condition that the funds of the institute should be used only for scientific and charitable objects, and in that way their position would be perfectly secured. The only practical alternative, if it was still thought right to refuse their request, would be that they should be incorporated by Act of Parliament—a process which would involve very great loss of time and also very serious expense. The importance of the object which they had at heart was one which he thought need hardly be much dwelt upon. Preventive medicine based upon bacteriology was a matter of comparatively recent experience, but it had been making gigantic strides, and every year and almost every week they were learning of new triumphs achieved in the discovery of the essential nature of disease and of the means of preventing such disease. He might be permitted perhaps to refer to one or two illustrations of the value of the work carried on at such institutes both to man and to the lower animals. The work done by M. Pasteur for the rescue of those bitten by mad dogs from the horrible death of rabies was bearing invaluable fruits. It had been estimated that within four years at the Pasteur Institute 12,000 lives had been saved. During the last six years 403 British subjects had been treated, and out of those 403 only seven had died. If they took into account the loss of time involved in making arrangements for going to Paris, and considered also that the essence of M. Pasteur's treatment was to intercept the disease before it arrived at the vital organisms in the brain, they might anticipate a large amount of success if they had the means in this country of having the same treatment carried out. From Germany had come the discovery of what was termed tubercle bacillus—that was to say, the micro-organism which was the essential cause of tubercle, the greatest physical scourge that afflicted the human race. To establish that that bacillus was really the essential cause of this disease in all its diverse forms required a large amount of investigation such as could only be carried on in institutes like that which they desired to see established. That the institute would be of great benefit also with regard to diseases of the lower animals might be seen from the discoveries made as to the cure of anthrax by M. Pasteur, and as to the treatment of another affliction known as "quarter evil" by a scientist of Lyons. Various bacteriological laboratories had been already established in the British Islands, but it was universally allowed that none of those existing was in the least equal to a great institute such as they desired to see established. One proof that such was the case was presented by the fact that our best workers in these subjects had been going continually to Paris or to Berlin for the superior advantages that they could obtain there. He ventured to think that the mass of educated opinion represented by the deputation was surely more deserving of attention than the views of those who, with whatever excellent intentions, had petitioned against their scheme. The truth was that objections were made because the petitioners objected altogether to the performance of experiments upon living animals, and not because they thought that there was already sufficient opportunity for work of this kind. If those petitioners knew how very small was the amount of suffering really inflicted upon the animals in such an institute, and how scrupulous was the care taken to avoid all needless pain, they would not (at least, the great majority of them would not) have made the opposition that they had made. He even doubted whether the question of their being likely to perform experiments upon living animals was one which the Board of Trade had any fair reason to occupy itself with. The licensing of places for the performance of such experiments, and the licensing of individual experimenters had always rested with the Home Secretary. Foreign institutions such as that which they desired to see established had been largely endowed by the State, and he did not relinquish the hope that our Government might at some future time see its way to give them substantial aid. But, however that might be, they ventured to hope that no department of this Government would oppose any unnecessary obstacle to an enterprise which had for its sole object the welfare of humanity, the health of mankind and the lower animals, and the general progress of the public weal.

Sir Lyon Playfair, M.P., said that experiments on living animals had been sanctioned by Parliament, which had intrusted the Home Secretary to make suitable restrictions for the carrying out of the operations. The proposed institute was promoted differently from those in foreign countries, which were being founded by the State, and the deputation only asked to be allowed to associate for a purpose recognized by Parliament, and with such restrictions as Sir Michael Hicks-Beach or the Home Secretary thought proper to impose.

Prof. Dewar spoke of the importance of the proposed institute from a chemical point of view, and Dr. Ray Lankester and Sir James Crichton Browne also spoke.

Sir M. Hicks-Beach, in reply, said:—I hope that it is not through any fault of mine that those who have arranged for this deputation have not come to me in the ordinary numbers of a deputation, but have thought it necessary for their object to summon from different parts of the country so very large a number of gentlemen who are very actively engaged, and whose time must be very valuable, not only to themselves, but also to the public. I am not disposed to be influenced in any matter by the mere numbers of a deputation. It would be perfectly possible for you and for those who differ from you on the other side to fill a very much larger room than this. I think the deputations should be weighed rather than counted, and if half-a-dozen of those who are now present had come to me saying what has been said to-day, and authorized to speak on behalf of all of you, I can assure you that I should have attached as much weight to their arguments as I can do now. But, of course, I accept your presence here as a strong testimony to the great interest that you feel in this subject. I am sorry to confess to have differed from so many gentlemen of such eminence as those who have supported this movement, and to have found myself unable to grant the application of the British Institute of Preventive Medicine for permission to register the Association without the addition of the word "limited." It is only due to you that I should explain, as shortly as I can the reasons which induce me to arrive at that decision. Now, the section of the Act of 1867, under which you ask me to act, lays down two preliminary requirements which must be proved to the satisfaction of the Board of Trade—first, that the Association shall be formed for one of several purposes, such as, for instance, that of promoting science, or some other useful object; secondly, that the profits or income will be applied to promote the objects of the Association, and that the payment of dividends will be prohibited. Now, I will assume that you have complied with both these requirements; I say nothing to the contrary. But the proof of such compliance does not, in my opinion, compel the Board of Trade to act on the section. Something has been said to-day to the effect that you have obtained counsel's opinion that it does compel the Board of Trade so to act. I have taken another view—I admit without legal advice. If you will place before me the opinion upon which your view is based, of course I shall very carefully consider it, and myself obtain legal advice upon that point, because I view it as an important point, as you will see from what I am going to say. I have considered, as I said, that the section of the Act only empowers the Board of Trade to act, and leaves it to the Board of Trade to decide whether the licence shall be granted or not; and if granted, whether any conditions or regulations should be imposed and inserted in the memorandum and articles of association. It therefore seems to me that the Board of Trade could hardly grant such a licence without expressing approval, by the mere fact of the grant, of the Association to which it is granted. In your case I think I have no right to express such an approval, because, if I rightly interpret Clause 3*a* of your memorandum, I understand—and I also gathered from what has been said to-day—"that experiments on living animals calculated to give pain," to quote the words of the Act of Parliament, are included among your objects; in one word, that vivisection would be part of your work. Now, this is a subject which the Legislature by the Cruelty to Animals Act, 1876, has placed under the control, not of the Board of Trade, but of the Home Office. By that Act, as you know, vivisection is made illegal except by licence from the Home Office, and under the most stringent regulations, including inspection by inspectors of the Home Office. I assume that when you had established this institution, supposing my licence were granted, an application would be made to the

Home Office for a licence or licences for vivisection on the premises of the institute for some one or more of its members. It seems to me that the Home Secretary would have fair ground to complain of my action, if in a matter of such admitted difficulty, rousing as it does the strongest feelings of both sides, I did anything which would enable you to go before him, to whom Parliament has intrusted this subject, with the stamp of approval as it were from another Government department which has nothing to do with the subject at all. Now, I hope I have put that shortly and plainly. What are your alternatives? You have said something to me on this subject to-day. You can, of course, if you choose, remove from your objects anything which could bring you within the Cruelty to Animals Act, 1876. If you did that, my objections would be entirely removed. You could, if you chose, form yourselves as a Society, vesting your property in trustees; associate yourselves under the Companies Act as a limited company, inserting a proviso that you should pay no dividends. Now, I should like to have before me the reasons in writing which have been urged to-day why none of these courses would meet your views. I can only say in conclusion that I have endeavoured to put to you the difficulty which I feel; that I will carefully consider what has been said to-day; and any documents which the promoters of the Association wish to place before me to enforce the views which have been expressed I shall be glad to receive.

Sir John Lubbock, in moving a vote of thanks to Sir Michael Hicks-Beach, said that Sir Henry Roscoe had authorized him to say that the further information which had been asked for should be furnished to the Board of Trade. Vivisection was after all a very small part of the question before them, unless, indeed, vivisection was to be understood as applying to the bacteria. He would venture to remind Sir Michael that although Acts of Parliament might prevent them from destroying the bacteria, they could not prevent the bacteria from destroying human beings, and it seemed almost a significant fact that no members of the community, as he knew to his own cost, had suffered more from them than members of the House of Commons. He had no reason to suppose that bacteria suffered at all, though human beings suffered very much from the bacteria. The bacteria were now experimenting upon them, and all that they asked was that they should be allowed to defend themselves from the bacteria. Something had been said about agriculture, and he believed that such an institute as this would add much to the prosperity of agriculture and probably of manufactures and of commerce. As regarded the technical points which had compelled the right hon. gentleman to adopt the course which he had taken, he thought if Sir Michael went into the matter he would find at least two precedents in which an opposite line had been taken in cases where vivisection was practised.

The President—I ought to mention that any of the precedents which have been mentioned I should like to have placed before me.

The deputation then withdrew.

EARTH-CURRENTS AND THE ELECTRIC RAILWAY.

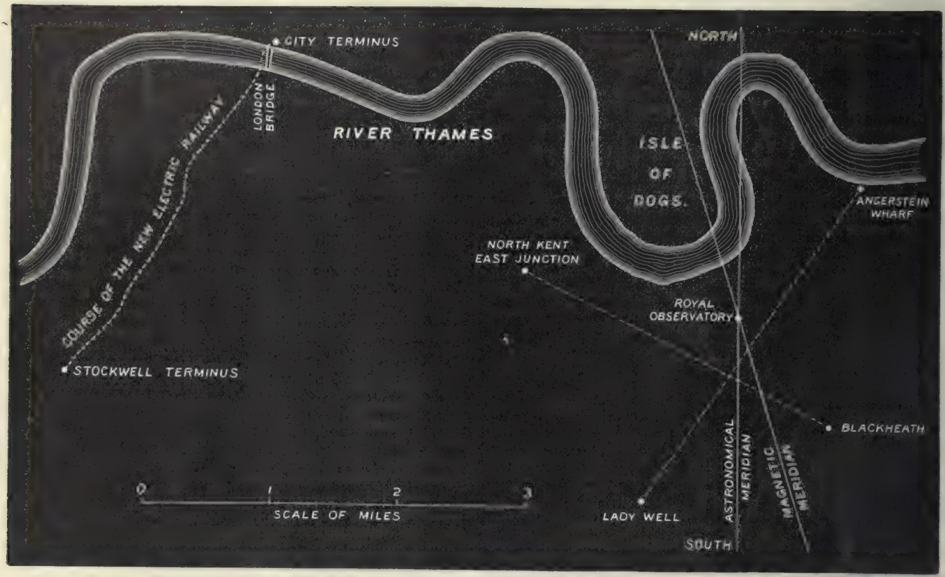
A WELL-MARKED case of interference with the earth-currents recorded at the Royal Observatory, Greenwich, due apparently to the working of the new Electric Railway, having recently been experienced, of which some account might prove to be interesting to electricians, the Astronomer-Royal has kindly allowed me to communicate for publication in NATURE some particulars in regard thereto.

It is known that for many years past a continuous photographic register of earth-currents has been maintained at the Royal Observatory. There are two circuits. For one circuit the earth-plates are at Angerstein Wharf (A.W.), on the southern bank of the River Thames, near to Charlton, and at Lady Well, Lewisham (L.W.); for the other circuit the earth-plates are on Blackheath (B.), at the south end of the North Kent Railway tunnel, and at the North Kent East Junction (N.K.E.J.) of the South-Eastern Railway, the junction of the North Kent and Greenwich lines. The earth connection is in each case made by an independent copper plate; these plates are used only for the earth-current lines, no other wires being attached thereto. From the A.W. earth-plate the wire passes by the South-Eastern Railway lines to the Greenwich Station, thence underground to the Royal Observatory recording apparatus, returning underground to the Greenwich Station, and thence by the railway to the earth-plate at L.W. Similarly for the Blackheath-North Kent East Junction circuit. The direct distance between the A.W. and L.W. earth-plates is 3 miles, and between the B. and N.K.E.J. earth-plates about 2½ miles. The azimuth of the A.W.-L.W. line, reckoning from magnetic north towards east, is 50°; the azimuth of the B.-N.K.E.J. line, reckoning from magnetic north towards west, is 46°. Registration is effected in the usual way. In each circuit there is a horizontal galvanometer the needle of which carries a small mirror; on this the light from a fixed gas-lamp falls, and, reflected therefrom, finally reaches the revolving cylinder as a small spot of light.

Some few particulars concerning earth-current motions generally may perhaps be given. It has been found that all cases of disturbance of the magnets are accompanied by earth-currents, more or less powerful as the magnetic disturbance is more or less pronounced. The correspondence is most complete. No sudden marked motion of the magnets ever occurs without corresponding active earth-currents, as may be seen by the plates (copies of the various registers) given in the several Greenwich volumes since the year 1882. On days on which the magnets are free from disturbance, and show only the ordinary diurnal change, earth-currents are very feeble.

Before speaking of the recent case of interference, we may devote a few words to the description of a previous case in which the interference was much less marked in character, although, with some intermissions, otherwise very persistent. Some five years or more ago it was remarked, in the A.W.-L.W. register, that at one part of the day a slight dislocation of the trace occurred, in no case indicating a change of potential of more than 0·1 volt, frequently much less: after some hours the trace as suddenly returned to its normal position. This was not discernible every day, but still frequently, and still continues. Nothing has been perceived in the other circuit. On examining the A.W.-L.W. records for a number of months, it appears that at all parts of the year the dislocation occurred some three-quarters of an hour after sunset, and the return to normal position at about the same interval before sunrise. The cause of the interference has not been traced, although it has been conjectured that in some way it may be connected with electric lighting in the vicinity of the A.W. earth-plate.

We now come to the recent much more serious case of interference. Towards the end of last year anomalous appearances began to be observed in both of the earth-current registers, not continuously but in a somewhat irregular manner. Now, however, for some months past, these new interruptions have settled down into a regular order. What is perceived is that the interference in question, causing a continuous vibration of the registering needles, commences shortly before 7h. in the morning, goes on all through the day, terminating shortly after 11h. in the evening. This went on for several months on week-days only, ceasing on Sundays, nothing being seen after 11h. p.m. on Saturday, until 7h. a.m. on Monday. But on Sunday, April 5, and on every succeeding Sunday to the present time, the interference has been experienced also on a portion of the Sunday, commencing at about 1h. p.m., and terminating usually at 10h. p.m. or shortly afterwards. Various experiments were made with the view of discovering the cause of these anomalous appearances, but without definite result. Quite recently, Mr. Leonard, the telegraphic superintendent of the South-Eastern Railway, to whom the Observatory is much indebted for considerable assistance in many matters connected with the earth-current work, was led to suggest that the exceptional appearances were most probably due to the influence of the new Electric Railway, three miles in length, and having terminal stations in the City and at Stockwell. A comparison being made between the observed times of interference with the earth-current registers, and the published times of running of the Electric Railway trains, it was found that these were simultaneous. Further, in the early part of the year, during the period in which the earth-current registers were free from interference on Sunday, there were correspondingly no Sunday trains. But on Sunday, April 5, it appears that trains commenced to run on Sunday afternoon, the same day that Sunday interference was first noticed at Greenwich, and these Sunday afternoon trains have been since continued. The whole matter is better seen in the annexed tabular statement:—



Times of interference with earth-current registers at the Royal Observatory, Greenwich.	Train service on Electric Railway.	
On week days.	On week days.	
From shortly before 7 a.m. until shortly after 11 p.m.	First train from Stockwell	6.40 a.m.
	,, ,, City ...	6.50 a.m.
	Last train from Stockwell	10.46 p.m.
	,, ,, City ...	10.58 p.m.
On Sundays, commencing April 5.	On Sundays, commencing April 5.	
From about 1 p.m. until 10 p.m. or shortly afterwards.	First train from Stockwell	1.0 p.m.
	,, ,, City ...	1.5 p.m.
	Last train from Stockwell	9.30 p.m.
	,, ,, City ...	9.30 p.m.

The line of the Electric Railway runs from about north-east to south-west magnetic, or more accurately the azimuth of the line, reckoning from magnetic north towards east, is about 50°. The nearest earth-plate to the railway is the N.K.E.J. plate, which is distant from the railway, in a perpendicular direction from it, about 2½ miles.

The correspondence so far as the comparison goes is complete. During the periods of interference the registering needles at the Observatory are in continual vibration. Whether the impulses are in one direction only or in both directions, and what is their frequency, cannot be readily determined from the registers. Eye observation of the needles may perhaps reveal something to us on these points. The abnormal excursions of the needles indicate a change of potential varying from a small fraction of a volt to perhaps the one-third of a volt or more. When any marked earth-current action arises, the interference becomes in some degree neutralized, and less marked in character.

It was found in the course of previous experiments, that when, instead of employing the complete A.W.-L.W. circuit, the A.W. branch only was allowed to register, by putting the wire to earth at Greenwich, the amplitude of vibration of the needle was not perceptibly changed, neither was it changed when the L.W. branch only was allowed to register. Correspondingly, when the B. branch alone of the B.-N.K.E.J. circuit was allowed to register, the vibration was much diminished, whilst with the N.K.E.J. branch alone registering it was much increased.

William Ellis.

THE ANNUAL VISITATION OF THE ROYAL OBSERVATORY.

THE Report presented by the Astronomer-Royal this year is of more than usual interest. The first part deals with proposed new buildings.

It has been decided that the museum or storehouse for portable instruments and apparatus should be built so as to form the central octagon of a future cruciform structure in the South Ground, which would accommodate the physical branch of the Observatory, and would carry the Lassell equatorial and dome at such a height above the ground that the neighbouring trees would not interfere with the effective use of the instrument. The building for the Museum was commenced at the beginning of March. In consequence of a recent decision of the Admiralty to largely increase the number of chronometers and deck watches for the Navy, additional accommodation for chronometers is required immediately, the space in the present chronometer room being insufficient even for existing requirements.

In other directions the difficulty of providing in the existing Observatory buildings for the accommodation of the increasing staff and of the accumulating material is severely felt, and it is very desirable that the pressure on the space available should now be relieved by means of a comprehensive scheme, which would supply a suitable fireproof brick building to replace the wooden sheds and huts which now disfigure the Observatory grounds, and to provide for the expansion which has taken place in late years, and which may be expected to continue in the future.

To provide for the efficient working of the 28-inch refractor about to be mounted on the south-east equatorial, the Admiralty have authorized the construction of a new iron-framed dome, 36 feet in diameter, to be erected on the south-east tower in place of the existing wooden drum, which, as mentioned in the last Report, has been so much strained in the course of thirty years, that there is great difficulty in turning it. An attempt was made to render the existing dome more serviceable by bolting the framework together more thoroughly, and by substituting properly turned spheres for the cannon-balls, but though the dome is not now liable to stick fast as formerly, it is still very difficult to turn, and cannot be considered serviceable. The new 36-foot dome, which is being constructed by Messrs. T. Cooke and Sons, is of peculiar form, adapted to the conditions of the case, the diameter being greater than that of the tower on which it is erected.

A photographic telescope with 9-inch object-glass by Grubb, and a prism of 9 inches diameter by Hilger, have been generously presented to the Royal Observatory by Sir Henry Thompson. The telescope has been mounted on the Lassell telescope as a photoheliograph, to give 8-inch pictures of the sun; a camera with Dallmeyer doublet (from photoheliograph No. 4), and an exposing shutter, specially designed to give very short exposures, being attached to it.

Six more electric hand lamps and an Ampère gauge (Sir W. Thomson's) have been purchased.

In view of the advantage resulting from the use of electric lighting for the photographic equatorial and for other instruments, the Astronomer-Royal considers it very desirable that an electric light installation should be provided for the Observatory, so that this method of lighting, which is specially adapted to the requirements of an observatory, may be applied to the instruments generally. The system now in use, of charging storage cells from primary batteries, is necessarily extravagant, and it does not admit of the desired extension.

With regard to the work done, the following statement shows the number of observations made with the transit-circle in the year ending May 10, 1891:—

Transits, the separate limbs being counted as separate observations	6036
Determinations of collimation error	307
Determinations of level error	390
Circle observations	5789
Determinations of nadir point (included in the number of circle observations	387
Reflection observations of stars (similarly included)	593

For determining the variation of personal equation with the magnitude of the star, 324 transits, not included in the above, have been observed. The apparent magnitudes of the stars are altered by placing a wire gauze screen in front of the object-glass of the telescope, and part of a transit is observed with clear aperture, part with obscured. The comparison of the two results gives the difference of personal equation for a definite change of magnitude. It appears that all the four regular observers record the times of faint stars later than bright, the difference per magnitude being about $0^{s}\cdot01$.

Altazimuth.—The total number of observations made in the year ending May 10, 1891, is as follows:—

Azimuths of the moon and stars	253
Azimuths of Mark I.	123
Azimuths of Mark II.	193
Zenith distances of the moon	118
Zenith distances of Mark I.	124
Zenith distances of Mark II.	188

Reflex Zenith Tube.—Since the date of the last Report, 14 double observations of γ Draconis have been made and completely reduced to the end of 1890. M. Lœwy's recent work seems to show that the determination of the constant of aberration with this instrument is more trustworthy than had been supposed; though the circumstance that the observations give a negative parallax for γ Draconis suggests that there is some unexplained source of error.

Sir H. Grubb reports that the object-glass and tube of the 28-inch refractor are now practically ready for mounting; but the Astronomer-Royal proposes to delay this operation until the completion of the new dome on the south-east tower mentioned in the first section of this Report.

Work with the 13-inch photographic refractor was seriously delayed by the accident to the driving-clock, and, later, by the illness of Mr. Criswick; but 81 stellar photographs have been taken, all of which must be regarded as more or less experimental. Ferrous oxalate development was used throughout, and all the plates were photographically impressed with the *réseau* kindly supplied by Prof. Vogel. The exposures have varied from a few seconds to about an hour; and trails have been taken both on the equator and near the pole to test the adjustment for orientation. Several different kinds of plates have been used, including Cramer, Seed, Paget, Star, Mawson and Swan, and Ilford; and on the whole the choice seems to lie between the Star and the Ilford plates.

Spectroscopic and Photographic Observations.—For determination of motions of approach or recession of stars, 286 measures have been made of the displacement of the F line in the spectra of 31 stars, and 14 of the *b* line in the spectra of 6 stars, besides comparisons with the spectra of Mars, the moon, the sun, or the sky, as a check on the general accuracy of the results. The series of observations with the 12¾-inch refractor is now practically completed, and the results are under discussion. An examination of those for the 21 stars most frequently observed shows that there is a systematic error depending on the hour angle, thus necessitating a correction for the position of the spectroscope at the observation.

In the year ending May 10, 1891, photographs of the sun have been taken at Greenwich on 224 days, and of these, 483 have been selected for preservation, besides 18 photographs with double images of the sun for determination of zero of position.

Magnetic Observations.—The following are the principal results for the magnetic elements for 1890:—

Mean declination ...	17° 28′·6 W.
Mean horizontal force	3·9546 (in British units). 1·8234 (in metric units).
Mean dip	67° 21′ 19″ (by 9-inch needles). 67 22 53 (by 6-inch needles). 67 24 24 (by 3-inch needles).

Meteorological Observations.—The continuous registration of meteorological phenomena has been maintained without interruption, except for four days in February when the old thermograph and shed in the magnetic ground were dismounted, and the new thermograph and shed were transferred from the South Ground to the position formerly occupied by the old instrument, to make way for the new buildings in the South Ground.

The mean temperature of the year 1890 was 48°·6, being 0°·6 below the average of the preceding 49 years. The highest air temperature in the shade was 82°·8 on August 5, and the lowest 13°·1 on March 4. This latter is the lowest temperature registered in March since 1841, being the same us that recorded on March 13, 1845. The mean monthly temperature in 1890 was below the average in all months excepting January, March, May, and September. In December it was below the average by 10°·0, and in January above by 5°·2.

The mean daily motion of the air in 1890 was 272 miles, being 10 miles below the average of the preceding 23 years. The greatest daily motion was 837 miles on January 26, and the least 32 miles on August 6. The greatest pressure registered was 14·5 pounds on the square foot on January 26.

During the year 1890 Osler's anemometer showed an excess of about three revolutions of the vane in the positive direction N., E., S., W., N., excluding the turnings which are evidently accidental.

The number of hours of bright sunshine recorded during 1890 by the Campbell-Stokes sunshine instrument was 1255, which is about 35 hours below the average of the preceding 13 years, after making allowance for difference of the indications with the Campbell and Campbell-Stokes instruments respectively. The aggregate number of hours during which the sun was above the horizon was 4454, so that the mean proportion of sunshine for the year was 0·282, constant sunshine being represented by 1.

The rainfall in 1890 was 21·9 inches, being 2·7 inches below the average of the preceding 49 years.

The winter of 1890–91 was remarkable for a long period of exceptionally cold weather which commenced on November 25, 1890. From this day till January 22 the mean temperature on every day except January 13 was below the average. The temperature was continuously below 32° on November 27, 28, December 10 to 19, 22, 23, 25, 28 to 30, January 2, 6 to 8, 10, 11, 17 to 19. The greatest defects from the average of 20 years were on November 28 (– 19°·1), December 22 (– 20°·7), and January 10 (– 19°·3). The lowest temperatures recorded during the three months were 18°·3 on November 28, 13°·4 on December 22, and 12°·0 on January 10. The mean temperature of December 1890 was 29°·8, or 10°·0 below the average of the preceding 49 years, the coldest December on record since 1841 previous to 1890 being that of 1879, whose mean temperature was 32°·4. In this same month, December 1890, only 2^{h}·4 of sunshine were recorded.

Chronometers, Time Signals, and Longitude Operations.—The number of chronometers and deck watches now being tested at the Observatory is 169 (113 box chronometers, 20 pocket chronometers, and 36 deck watches). The annual competitive trial of chronometers commences on July 4, and the trial of deck watches on October 24.

The time-balls at Greenwich, Deal, and Devonport are next referred to.

The reductions for the longitude Paris–Greenwich are now completed and ready for publication. In reference to the discrepancy between the results of the French and English observers, mentioned in the last Report, Commandant Defforges visited Greenwich in June 1890, and went carefully through the reductions with Mr. Turner and Mr. Lewis. No mistake was found in the work, but several questions of some importance were raised. The results of the discussion and of subsequent correspondence are summed up in two papers by Mr. Turner and one by Colonel Bassot and Commandant Defforges, in the *Monthly Notices of the Royal Astronomical Society*, vol. li. pp. 155, 407, and 413 respectively. As the matter now stands, the English definitive result for the difference of longitude between the Greenwich transit-circle and Cassini's meridian is 9^{m} 20^{s}·86, while the French result (not yet published) is about 0^{s}·15 greater, a discordance which, though only about half of that found in the preliminary discussion, is still so large, that there seems to be no alternative but to repeat the work with special precautions suggested by the experience gained.

The proposal to determine the longitude of Montreal as the base station for the Geodetic Survey having been sanctioned by the Admiralty last December, arrangements have been made in concert with Prof. McLeod, of the McGill College Observatory, Montreal, for a determination of the longitudes Montreal–Canso–Waterville–Greenwich, the termini of the cable, Canso and Waterville, being occupied as longitude and not merely as transmitting stations, a course which seems advisable in view of the great geodetic importance of these points. The necessary funds have been voted, and the Commercial Cable Company have generously granted the use of their cable.

The determination of the longitude of Washington has been deferred for the present.

During the past year, Lieutenants Heming, Monro, and Smyth, R.N., and Captain Haynes, R.E., have at various times been instructed in transit-observing. Mr. S. Hirayama, of the Tokio Observatory, was at work for some weeks studying the general organization of the Observatory.

THE CLASSIFICATION OF THE TUNICATA IN RELATION TO EVOLUTION.

THE detailed classification of the Tunicata, and especially of the so-called "Compound Ascidians," has usually been found a matter of special difficulty by systematists, and each successive investigator has discovered grounds for modifying in important respects the grouping of genera and families established by his predecessors. A glance at the systems of Giard, Della Valle, von Drasche, and Lahille, all of recent date (*i.e.* post-Darwinian, and since the introduction of modern methods and the recognition of the Tunicata as Chordata), shows the notable want of agreement between competent authorities. There is probably a special reason for this exceptional diversity of opinion, and I believe the cause is to be found in the course of evolution or phylogeny of the group, and especially in the complex relations between the Compound forms and the other Tunicata.

In fact, if the matter be regarded from the point of view of the consistent evolutionist, the special difficulties vanish, the complicated relationships between groups (which can only be represented by dendritic diagrams, or even in some cases by networks) become explicable and natural, the great diversity in value of the assemblages of forms known as "genera" and "species" is simply what would be expected, and the differences between the various classificatory systems (allowing for a few errors which have been corrected by later investigations) can be accounted for, and the conflicting opinions of the authors reconciled. But, on the other hand, if the subject be approached from the standpoint of the pure systematist, whose object is to divide and subdivide into clearly defined groups of approximately equal value, and to recognize only "good" genera and species, nothing but confusion results; it becomes practically impossible to distinguish and arrange naturally the groups of Simple and Compound Ascidians; and some of the most interesting and instructive points, such as the gradation of varieties into species and species into genera, and the individual variations in specific characters, are altogether lost sight of.

These views were expressed partly in my Reports on the *Challenger* Tunicata, but further work since—on some very extensive collections from Australian seas and on the Ascidians of our own coasts—has convinced me that the only rational explanation of the protean forms and labyrinthine inter-relations of the Ascidians is to be found in regarding the group as one in process of evolution, where many of the species, genera, &c., have not yet become markedly differentiated by the elimination of intermediate forms, and where the animals are so much at the mercy of their environment that a special premium is set upon useful characters (if, indeed, there are any "specific" characters which are not useful), and where, consequently, the relations between modification of structure and conditions of existence brought about by the action of natural selection are exceptionally evident. Adopting, then, this view, the following difficult subjects of dispute, and probably others with which I am not concerned at present, can be, I think, satisfactorily explained: (1) the connection of the Simple with the Compound Ascidians, and the classification of the latter; (2) the value of some modifications of the branchial sac; (3) the position of the Polystyelidæ; (4) the relations between the sub-families and genera of the Cynthiidæ; and (5) the numerous "species" of the genus *Botryllus*.

(1) If the attempt is made (as in most classifications) to regard the Compound Ascidians as a group distinct from the Ascidiæ Simplices, and forming either a parallel or a divergent line in regard to the latter, one meets at once with the serious difficulty that the Compound Ascidians show affinities with the Simple at several distinct points. Three investigators approaching the Compound Ascidians after the previous study of certain Simple Ascidians—say, the first fresh from *Ciona*, *Ecteinascidia*, and *Clavelina*, the second from *Perophora*, and the third from *Styela* and *Polycarpa*—could each make out a good case for the view that his new subjects were most closely connected with the genera he had just been working at. The first could demonstrate the undoubted relations, in external form and in structure of branchial sac, between *Clavelina* and *Chondrostachys*, *Colella* and the other Distomidæ; the second might point to the similarity (on which I personally lay no stress) of *Perophora* and the Botryllidæ, in the relations of alimentary canal to branchial sac; and the third could show the close similarity between the Styelinæ and the Compound forms *Synstyela*, *Goodsiria*, and *Chorizocormus* in nearly every detail of internal structure: and all three would be partly right, and therefore unlikely to agree upon any one system of classification.

But when the attempt is made seriously to form a conception of the past history or evolution of the forms in question, it becomes obvious that the Compound Ascidians are not a natural, but an artificial group. That is, they are not the whole surviving descendants of a single group of ancestors, but are polyphyletic in origin, being derived from several distinct lines of ancestry which have arisen independently from different kinds of Simple Ascidians, and have since acquired the common characteristic of being able to reproduce by gemmation so as to form compact colonies in which the members (ascidiozooids) are embedded in a common test or investing mass. We know with as much certainty as we know anything in such phylogenetic inquiries that the ancestral Tunicates were not colonies, and that reproduction by gemmation was not a primitive character. This property has, then, been acquired secondarily by some ancestral Simple Ascidians, and may very possibly have been acquired more than once (though this is not at all necessary for my theory of the polyphyletic origin). It follows from this view (which I have expressed before, but now feel more certain of from recent work), that if we are to retain the group Ascidiæ Compositæ, or Synascidiæ, in our system, we must represent it as linked on to the Ascidiæ Simplices, at three points at least, and we must not attempt to arrange the families and genera in a series diverging from any one of these points alone; or if we do, we need not be surprised when we arrive at obviously unnatural arrangements which are in conflict with the classifications of our fellow-workers.

On the other hand, we might abolish the group Ascidiæ Compositæ altogether as a sub-order of Ascidiaceæ, on the ground that it is not a natural group (*i.e.* a compact set of descendants from a common ancestor—a single branch of the genealogical tree).

But if we adopt this course with the Compound Ascidians, the same argument might be used in connection with other polyphyletic groups throughout the animal kingdom. They should all be broken up, it might be urged, as being artificial assemblages. And that would be a perfectly logical and definite position to take up, and one for which a good deal could be said, but before adopting it zoologists should remember that it involves a loss as well as a gain. If it gives "the system" a certain precision, and an advance of a step or two towards the goal of a completely natural classification, it at the same time destroys the recognition of characteristics which certain forms possess in common. In whatever manner they have been obtained, there is no doubt that Compound Ascidians of the present day possess certain features by which they can be identified as Compound Ascidians, and this fact is surely worthy of recognition in our "system." My own opinion, then, is that the group Ascidiæ Compositæ should still be retained, but that its polyphyletic origin and multiple connection with the Ascidiæ Simplices should be carefully borne in mind when drawing up any scheme of classification, or discussing affinities.

(2) Some of the ideas noted above, and others to be discussed below, took definite form lately in reading a recently published memoir by M. Fernand Lahille,[1] in which, while giving a number of important original observations on the anatomy and bionomics of the Ascidians (and especially of the Compound forms) of the French coasts, the author introduces what I cannot help thinking in some respects an unfortunate attempt to remodel the classification of the Tunicata on lines which he communicated a few years ago to the French Association (Congrès de Toulouse, 1887), and now elaborates in detail. He regards the branchial sac as the most important organ in the Tunicata, and so it is in some respects; but that is not sufficient reason for regarding its modifications in structure as the sole characteristics of the primary groups. For example, the Appendicularians, instead of being called Larvacea or Copelata, and characterized by the presence of a tail containing the urochord, are placed in a group "Atremata," defined by the absence of stigmata in the branchial sac. The openings in question (stigmata) are not even such important structures as the primary branchial clefts (gill-slits), but are merely the secondary slits placing the cavity of the branchial sac in communication with the peribranchial or atrial cavity, and are of nothing like such high morphological value as the presence or absence of a urochord, and of the two primitive atriopores, and the other well-known characteristics employed in former classifications as distinguishing the Appendiculariidæ. Some of the Thaliacea are placed by Lahille in a group (Hemitremata) of primary importance, by themselves, because they have the stigmata rudimentary or imperfectly formed, while the other Thaliacea are united with all the remaining Tunicata, because they are supposed to be alike in having complete stigmata.[2]

Then, again, an altogether fictitious value is given by Lahille to the presence of internal longitudinal bars in the branchial sac, especially since he shows (as had been done by former writers) that these bars develop as outgrowths from the connecting ducts, and that intermediate conditions can be found in which the bars can neither be said to be absent nor present. He describes this condition in his new species *Perophora banyulensis*, and it is also present in *P. viridis*, Verrill, and in various other Simple Ascidians, as has been shown in the *Challenger* Reports and elsewhere.

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[1] "Recherches sur les Tuniciers des Côtes de France" (Toulouse, 1890).

[2] Which, however, is not really the case. The apertures in the walls of the branchial sac in Lahille's "Eutremata" are not always homologous structures. In the genus *Culeolus*, for example, there are no true stigmata.

Such cases, although rather perplexing to the systematist, are perfectly natural from an evolutionist's point of view, and they certainly make one regard with some suspicion large groups founded upon any such one character. Consequently, Lahille's order "Stolidobranchiata," characterized solely by the presence of a particular kind of internal longitudinal bar in the branchial sac, is, in my opinion, a most unnatural assemblage of the families Polystyelidæ, Cynthiidæ, Molgulidæ, and Botryllidæ, which cannot be retained. It is not safe to trust to the modifications of structure of one organ in the detailed classification of a group, and it is especially unsafe where that organ is, as in the case of the branchial sac, of great physiological importance, and so is liable to be considerably modified in accordance with the mode of life in forms which are otherwise closely related. Morphological characters of less functional importance are more likely to be retained unaltered, and so indicate real genetic affinity.

Surely Lahille does not seriously mean to contend that the internal longitudinal bars in the branchial sac of the Botryllidæ, Cynthiidæ, &c., are different in any morphological sense from the similar bars found in other Ascidians, such as the Ascidiidæ. Although they may be slightly different[1] in their relations to the wall of the sac in these two groups, being attached throughout their length in *Botryllus* in place of only at the angles of the meshes as in *Ascidia*, and are therefore somewhat different in their development (ontogeny), there can scarcely be any doubt that in their origin (phylogeny) all such bars in the branchial sac are alike, and are therefore homologous structures.

(3) It follows from what has been said above in regard to the origin of the Compound Ascidians, that even though the group Polystyelidæ is placed (as was the case in the *Challenger* Report) in the Ascidiæ Compositæ, it is not thereby widely separated from its relations amongst the Simple Ascidians. If the sub-order Ascidiæ Compositæ is retained, then the Polystyelidæ must go in it, since they form definite permanent colonies with the ascidiozooids embedded in a common test; but of course these forms are very similar in many respects to *Styela* and *Polycarpa*—that being one of the points of contact between Compound and Simple Ascidians—and therefore I can agree fully with all that Lacaze-Duthiers and Delage say in favour of that relationship. The matter stands simply thus:—If Ascidiæ Compositæ is retained, the Polystyelidæ must be placed in it at the nearest point to *Polycarpa* amongst Ascidiæ Simplices; while if Ascidiæ Compositæ is abolished, the Polystyelidæ will form a family or a sub-family (it matters little which) alongside the Styelinæ under Ascidiæ Simplices. To go further, and break up even the genera of the Polystyelidæ, placing the species beside those Cynthiidæ they resemble most in the structure of the branchial sac, would be to give no value at all to the property of reproduction by gemmation and the formation of colonies.

(4) It has long been recognized that there are two groups of forms in the family Cynthiidæ, those which centre around *Styela* and those related to *Cynthia*, and when the remarkable stalked forms, such as *Boltenia* and the deep-sea genus *Culeolus*, had been added, I defined these three groups as sub-families under the names Styelinæ, Cynthinæ, and Bolteninæ. Leaving the last out of the question, we have the two former distinguished amongst other characters by the fact that the Styelinæ have never more than eight folds in the branchial sac, and have simple tentacles, while the Cynthinæ have always more than eight folds, and compound tentacles.

A few years ago these seemed well-established characters to which there were no exceptions. Last year, however, Lacaze-Duthiers and Delage published a preliminary account of a *Cynthia* from the French coasts, with only eight folds (as in Styelinæ) in its branchial sac; while Traustedt has discovered that the *Cynthia tesselata* of Forbes has four folds on the right side of the branchial sac and three on the left (like some Styelinæ), although the tentacles are compound (as in Cynthinæ); and I find that long ago Alder described the reverse case in *Cynthia tuberosa*, Macg., where there are twelve folds in the branchial sac (Cynthinæ), although the tentacles are simple (Styelinæ). Thus the two links required to unite the characters[1] of Styelinæ and Cynthinæ have been found, which is perfectly natural and satisfactory to the evolutionist, and the question for the systematist now is, Must these two sub-families be united? I think not. I believe that they are natural groups, and that they are really as widely separated from one another in their typical members as we ever supposed them to be, although not so completely isolated from one another by the extinction of intermediate forms.

If these interesting links, to which attention has just been drawn, and which are apparently not common nor widely distributed forms, had become extinct a few years ago, the Styelinæ and Cynthinæ would without question be justly regarded as widely separated groups. And the present position is merely that a few forms are known which if not bridging over at least lie as stepping-stones in the gap; while the vast majority of the species in question are clearly distinguishable by easily recognized characters into two definite sets. This last fact has an importance which entitles it to recognition. I am far from wishing to ignore the importance of such intermediate forms; in fact I am more likely, I fancy, to regard them with undue interest; but after all they are single species, minute twigs of the great branch under consideration, while long series of typical Styelinæ and Cynthinæ—the many species of *Styela* and of *Polycarpa*, of *Cynthia* and of *Microcosmus*—can be divided into two groups by their tentacles and their branchial folds, and I believe we are justified in giving expression to this natural grouping by retaining the two sub-families in our system of classification. It need not lead to any difficulties: the intermediate forms can be placed as an appendage to the sub-family taken first. We cannot now pretend to draw hard and fast lines round all our groups, a serial or a tabular classification will always give erroneous impressions, and in a phylogenetic arrangement the linking forms will appear in their proper places as little twigs between the two great branches.

(5) The genus *Botryllus* seems to contain an endless series of forms which might be (and many of which have been) described as separate species. Giard, twenty years ago, pointed out the great variability of the species in this genus, and described many varieties and local conditions, but the supply is not yet exhausted, and one is almost tempted to conclude that no satisfactory position can be taken up anywhere between the two extremes of either (1) regarding the whole genus (or even the family Botryllidæ) as an enormous protean species, or (2) describing nearly every colony as a separate species.

From the point of view of the systematist or speciographer who wants "good" and well-defined species, this group of Ascidians must be an abomination, but to the student of evolution it is full of interest. Here, if anywhere, characters can be seen varying in all directions and to almost all degrees, some variations becoming fixed while others remain indefinite. I am at present examining (with the help of my former student, Miss A. E. Warham, B.Sc.) the anatomical characters of a number of colonies of various *Botrylli* with the view of finding which characters, if any, can be relied on in distinguishing species or "forms," and I have just seen a series of ascidiozooids of *Botryllus smaragdus* in which the branchial tentacles, usually regarded as important features in the diagnosis of species, present all variations between eight and sixteen. Every one of the numbers 8, 9, 10, 11, 12, 13, 14, 15, and 16, is represented by one or more ascidiozooids, although 8 and 16 are those most commonly found. Also several definite arrangements, such as 2 large pigmented tentacles and 6 small, 3 large pigmented and 13 small, are present, and are connected by all possible gradations. Then, again, we find that the smaller set of these tentacles may be all alike, or may be of two sizes placed longer and shorter alternately, or they may be 2 shorter and 4 longer, or 2 shorter and 5 longer, or 3 shorter and 5 longer, or 4 shorter and 5 longer, or 6 shorter and 5 longer, and so on through the variations. Two or three of the extreme forms, if examined by themselves, might easily be regarded as distinct species.

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[1] Even this difference is not constant. In some Botryllidæ, and I think in all Polystyelidæ and many Cynthiidæ, the relations of the bars in the adult are precisely as in *Ascidia*, *Ciona*, and *Ecteinascidia*.

[1] These are the chief characters, but there are others, such as the condition of the stomach and digestive glands.

I have heard it said, and I fancy it may be often thought, that since evolution has changed our conception of a species, the modern biologist need not concern himself with the description and nomenclature and delimitation of those assemblages of variable forms which are known as varieties and species. But to take such a course would be a great mistake. The theory of evolution has given taxonomy and speciography an additional and a very real interest. Now that we know just how much and how little the term species indicates, it has become of great importance that species and varieties should be re-studied from the evolutionary standpoint, that the relations of allied forms should be carefully investigated, the limits of their variation determined, and the effect of their environment ascertained. The Botryllidæ form a specially interesting group for such an investigation.

Many of these more general remarks will no doubt apply to other groups of organisms with as much force as to the Tunicata, but some of the instances discussed above may seem points of mere detail of no great general interest. I believe, however, that they are typical cases illustrating difficulties which may confront any specialist in the course of his endeavour to attain to that important object of biological investigation—a natural or genetic classification of animals and plants.

February. W. A. HERDMAN.

PHOTO-STELLAR SPECTRA.[1]

PROF. PICKERING, while retaining the four types of stellar spectra, finds that so many stars show an intermediate stage of development, that, in the Draper Catalogue, letters are substituted for the types. Thus, letters A to D denote stars of the first type; E to L, stars of the second type; M, stars of Type III.; while N is reserved for fourth type stars. It seemed of some interest to compare the photographic results with those obtained directly with the spectroscope. For the first and second types, the observations of Vogel ("Spect. Beob.," − 1° to + 20°) were used. The stars in the first four hours of R.A. which occur in both works were examined and tabulated, those being rejected where there was any uncertainty as to type in Vogel's observations. The following table shows the results thus obtained:—

VOGEL. Eye observation. Class.	PICKERING. Photographic observation. Letter.						
	A	B	E	F	H	I	K
I.	68	1	25	18	15	1	1
I. !	35	1	4	—	—	—	—
II.	4	—	5	—	28	—	1
II. !	—	—	—	—	—	—	—
II. !!	—	—	—	—	2	—	2

To show the differences in type, the following table has been drawn up:—

VOGEL. Stars. Number and Type.	PICKERING.	
	Type I.	Type II.
169 of I. ...	105 ...	64
42 of II. ...	4 ...	38

These tables show that, in the case of Type I., nearly half the stars observed with the eye are really Type II. according to the photographs; in the case of Type I.!, four out of the forty, although having a clearly pronounced first type spectrum to the eye, are really second type stars according to the photographs. In the case of the second type, four stars out of forty-two are really first type.

For the third type stars, Dunér ("Sur les Étoiles," &c.) was consulted, and the following results were obtained:—

DUNÉR. Eye observation. Type.	PICKERING. Photographic observation. Letter.						
	A	E	F	H	I	K	M
III.	—	—	—	19	2	—	8
III. !	—	—	—	24	2	1	22
III. !!	3	—	—	16	1	1	24
III. !!!	—	1	1	5	—	—	12

This table may be condensed as follows:—

DUNÉR. Type.	PICKERING. Type.			
	I.	II.	III.	Total.
III. to III. ! ...	— ...	48 ...	30 ...	78
III. !! to III. !!! ...	3 ...	23 ...	12 ...	38
Total ...	3	71	42	116

The photographs therefore show that only 36 per cent. are third type at all. In order to account for this very remarkable result, the words of Prof. Pickering may be quoted:—"The difference between this (the third) type and the second is much less marked in the photographic than in the visible portion of the spectrum. The most noticeable difference is that, in spectra of the third type, the intensity suddenly changes at the wave-length 476·2. Rays of greater wave-length than this are fainter than those that are shorter."

It will be seen that three stars of the third type appear as first type stars on the photograph. These are:—

(1) LL. 3717, 1h. 55m. $-\ 9^\circ\ 0'\cdot4$, Dunér III. !!: "Les bandes 2–9 sont fortement développées, très larges et sombres."

(2) D.M. $+\ 17^\circ\cdot1479$, 6h. 56m. $+\ 17^\circ\ 53'\cdot8$, Dunér III. !!: "Les bandes 2–8, et peut-être 9, sont visibles; elles sont très larges et fort obscures autant dans le vert-bleu que dans le rouge."

(3) τ^4 Serpentis, 15h. 31m. $+\ 15^\circ\ 25'\cdot9$, Dunér III. !!: "Les bandes sont larges et fortes, surtout dans le vert et dans le bleu."

Prof. Pickering states, in the preface, that when the brightness exceeds 6·5 it is difficult to classify the spectrum with certainty. The photographic magnitudes of these stars are 6·65, 6·45, 6·44 respectively.

As regards the fourth type, it is stated (p. 3) that "the letter N is reserved for spectra of the fourth type, although no star of this type is bright enough to appear in the Draper Catalogue, owing to the red colour of all such stars." This seems to be a mistake, as three fourth type stars are found in the Draper Catalogue. They are :—

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[1] "Note on the Classification of Star Spectra in vol. xxvii. *Harvard Annals*, and on some Stars with Bright Lines."

Name.	R.A.	Decl.	Pickering's letter.	Photo. mag.	Dunér.
	h. m.	° ′			
D.M. + 17°1973	8 49	+17 36	H	6·65	IV. !!!
D.M. +68·617	10 38	+67 56	A ?	6·50	IV. !!!
D.M. + 76·734	19 25	+76 22	E	7·08	IV. !!!

These stars each occur on one plate only.

The photographs show that the following stars have bright lines in their spectra :—

Known variable stars : *ε* Aurigæ, *α* Orionis, *ζ* Geminorum, *α* Herculis, *β* Pegasi.

Suspected variable stars : *α* Cassiopeiæ, 66 Ceti, *ρ* Persei, *α* Tauri, *δ* Canis, *β* Geminorum, *α* Boötis, *β* Ursæ Minoris, *β* Cygni, *γ* Cephei.

Other stars showing bright lines, not hitherto detected, are : *τ* Ceti, *γ* Andromedæ, *κ* Persei, *α* Persei, *ν* Persei, 80 Tauri, *ζ* Aurigæ, *ζ* Cancri, σ^1 Ursæ Majoris, *o* Leonis,, *γ* Leonis, *ξ* Ursæ Majoris, 43 Comæ, *o* Boötis, *γ* Scorpii, *β* Coronæ, *ζ* Herculis, *η* Herculis, *μ* Herculis.

T. E. ESPIN.

SOME ASPECTS OF STAS'S WORK.

FOR the last thirty years Stas's work has set the standard of excellence in all that relates to atomic weight determination. The literature of the subject teems with references to his classic memoirs, which have come to be regarded by chemists in the light of canonical books. Admiration of the almost magical accuracy of Stas's results seems somewhat to have diverted attention from the rare philosophical insight displayed in the *plan* of his researches. Yet it is not too much to say that, while we owe the *conception* of the atomic theory to Dalton, Stas first placed the theory on a sound experimental basis.

It was in the year 1843 that Dumas and Stas's value for the atomic weight of carbon recalled attention to the hypothesis of Prout which had hitherto met with little favour on the Continent. The subsequent work of Dumas and of de Marignac led these chemists to support the hypothesis in a modified form. In 1860 appeared the first series of Stas's researches, " Sur les Rapport reciproques des Poids atomiques." In the introduction to his paper the author stated his conviction that these researches furnished proof, as complete as the nature of the subject admitted, that the hypothesis of Prout was a pure delusion —that there was, in fact, no common divisor between the atomic weights of the elements. In reviewing the work of Stas, de Marignac admitted the impossibility of reconciling the concordant results obtained by Stas and himself with even the modified form of Prout's hypothesis. Yet he regarded the dictum quoted above as too absolute in character. It was by no means established, he contended, that the constituents even of stable compounds are present *exactly* in the proportion of the atomic weights. De Marignac's criticism struck at the very basis of the atomic theory but this by no means deprived it of its weight. The laws of chemical combination are the experimental basis of the atomic theory, and Stas admitted that these laws had never been proved as " lois mathématiques." Writing in 1865, in the introduction to his " Nouvelles Recherches," he remarks that some of the fundamental ideas of chemistry, which are generally taken as having been proved, are as a matter of fact far from being so. He considers that the constancy of composition of chemical compounds has been experimentally established, but points out that this does not constitute a proof of the law of constant proportions, the law, viz., which states that the particular proportions in which two elements are combined in a certain compound is a *constant* proportion in all the compounds which contain those elements. This had never been proved, yet it was only in this way that the position of the atomic weights as constants of nature could be established. The so-called *law* of multiple proportions Stas referred to as an *hypothesis* of Dalton, pointing out that the very rough analyses on which Dalton relied—of which the error is frequently more than 10 per cent. —as well as the results obtained by Wollaston and by Gay-Lussac, were at most capable of establishing a "loi limité." The state of science at the time demanded a thorough re-examination of the basis of the atomic theory. Stas realized this need, and took upon himself the burden of the task. The conception and plan of the " Nouvelles Recherches sur les Lois des Proportions Chimiques " show the mind of a great thinker not less clearly than the results of the work exhibit the skill of a master in the art of experiment. The " Nouvelles Recherches " contains a verification as " loi mathématique " of the law of conservation of mass, in the complete synthesis of silver iodide, and the complete analysis of silver iodate. The constancy of composition of chemical compounds was subjected to a crucial test in the experiments on ammonium chloride, and the constant proportion between the combining weights of elements in different compounds was tested in the conversion of silver iodate, bromate, and chlorate, to the corresponding haloid salts. The law of equivalent proportions was verified by the concordant results obtained for the atomic weights of silver and of the alkali metals determined as functions of those of iodine, of bromine, and of chlorine respectively, oxygen forming the common standard. One cannot help regretting that the law of multiple proportions was not also made the subject of investigation. The most suitable examples occur among gaseous substances, and the operations of gas analysis were foreign to the methods of manipulation employed by Stas. The complete analysis of nitrous oxide was indeed contemplated in order to determine directly the atomic weight of nitrogen as a function of that of oxygen, but the idea was abandoned owing to the difficulty of constructing the necessary apparatus.

The work on the laws of combination furnished fresh materials for the examination of Prout's hypothesis. Stas's comments on the origin of this hypothesis possess a high degree of philosophic interest. The remarks to which we more particularly refer are the following :—" Lorsqu'on remonte à l'origine de l'hypothèse (de Prout) on s'aperçoit immédiatement qu'elle doit sa source à un préjugé ou, si l'on veut, à un opinion préconçue, concernant la simplicité des lois de la nature. Pendant longtemps les chimistes comme les physiciens, dès l'instant qu'ils ont vu certains faits se reproduire avec une *apparence* de régularité, ont cru à l'existence d'une loi naturelle susceptible d'être exprimée par une relation mathématique *simple*. . . . C'est à cette tendance, d'ailleurs très-naturelle, qu'on doit l'hypothèse de Prout." Dalton's enunciation of the law of multiple proportions is relegated by Stas to the same category as a generalization on insufficient data. Mendeleeff has remarked (Faraday Lecture, 1889) that the periodic law has shown that the masses of the atoms increase *per saltum*, in a manner which " is clearly connected in some way with Dalton's law of multiple proportions." Dalton was more fortunate than Prout. The combining proportions are expressible by a simple mathematical law, whilst the atomic weights are only to be represented by a complicated formula which may have some such form as that proposed by Carnelley.

The " Nouvelles Recherches " appeared in 1865. The first paper on the periodic system was read before the Russian Chemical Society in the spring of 1869. It is curious to reflect that the foundations of the atomic theory had hardly been made sure by Stas ere they were called upon to bear the magnificent structure raised by Mendeleeff.

V. C.

NOTES.

WE print elsewhere the proceedings of the important deputation to the Board of Trade on the subject of the Institute of Preventive Medicine. There can be no doubt that, after the statement made by the Minister, the registration of the Society will shortly be an accomplished fact; a few words in the deed of registration or a few minutes of reference between the Board of Trade and the Home Office are all that is needed to safeguard Sir Michael Hicks-Beach's official scruples. The importance of the deputation, however, will not be limited to this: it shows again, as in the case of the Art Gallery, that men of science are no longer willing to be snubbed by men in office.

THE annual meeting for the election of Fellows was held at the Royal Society's rooms, in Burlington House, on Thursday last, when the following gentlemen were elected into the Society:—William Anderson; Prof. Frederick Orpen Bower; Sir John Conroy, Bart.; Prof. Daniel John Cunningham; Dr. George Mercer Dawson; Edwin Bailey Elliott; Prof. Percy Faraday Frankland; Percy C. Gilchrist; Dr. William Dobinson Halliburton; Oliver Heaviside; John Edward Marr; Ludwig Mond; William Napier Shaw; Prof. Silvanus Phillips Thompson; Captain Thomas Henry Tizard, R.N.

MR. GEORGE HOLT, of Liverpool, last week sent the Treasurer of the University College there a cheque for ten thousand pounds as endowment for a Chair of Physiology, and candidates for the appointment are forthwith to be advertised for. It is only a few weeks since Mr. Brunner, M.P., sent a similar cheque to endow a Chair of Political Economy. The latter post has been offered to and accepted by Mr. E. C. K. Gonner.

THE Prince of Wales has fixed 4 o'clock on Wednesday, June 17, for the delivery by Lord Rayleigh of the first of the two lectures at the Royal Institution in connection with the centenary of the birth of Michael Faraday; and Friday evening, June 26, at 9 o'clock, has been appointed for the second of these lectures, which will be given by Prof. Dewar.

STUDENTS of geology were sorry to hear of the death of Dr. P. M. Duncan, F.R.S. He died on May 29 in his sixty-seventh year. Dr. Duncan was Professor of Geology at King's College, London, and was intimately connected with the Geological Society, of which he was President in 1876 and 1877. He was also a member of the Linnean Society.

MR. G. V. POORE, the Government Inspector, who has recently drawn up a report upon experiments performed on living animals during the year 1890, states that during the many visits he has paid to places licensed for the performance of such experiments, it has never fallen to his lot to see a single animal which appeared to be in bodily pain.

WE are glad to be able to announce that Mr. J. Graham-Kerr, of the University of Edinburgh, Naturalist to the Pilcomayo Expedition, has returned safely to this country, and has succeeded in bringing with him a portion of his natural history collections. As is well known, the *Bolivia*, in which Captain Page and his expedition ascended the Pilcomayo, was stranded in that river, in April 1890, in the middle of the Gran Chaco. After the death of Captain Page, which occurred while he was returning in a canoe down the Pilcomayo to get medical assistance, the *Bolivia* remained stuck fast nearly in one spot until March of this year, when Mr. Kerr, finding the vessel still immovable, and no prospects whatever of a rise in the river, decided to come away as best he could. After a very rough journey he reached Asuncion on mule-back, bringing as many of his light things as possible, and arrived in this country last week. Some very interesting letters of Mr. Kerr's, describing the natural history of the Gran Chaco, will be found in the two numbers of the *Ibis* for January and April last.

UNDER the will of Dr. Fothergill (1821), funds were bequeathed to the Society of Arts for the offer of medals for subjects, in the first instance, relating to the prevention of fire. A Society's Gold Medal, or £20, is now offered for the best invention having for its object the prevention or extinction of fires in theatres or other places of public amusement.

MESSRS. NEWTON AND CO. have been appointed philosophical instrument makers to the Royal Institution of Great Britain, an appointment which we believe has not been held by any firm for some years.

MR. JOHN T. BRUNNER, M.P., has been elected President of the Sunday Society in succession to Prof. G. J. Romanes. Mr. Brunner will deliver his presidential address at the Society's public annual meeting on June 27.

THE Société Botanique de France recently held its annual meeting in the little town of Collioure, near Perpignan, on the Mediterranean coast. After the meeting many excursions were made in the neighbourhood, which is interesting to botanists.

ON behalf of Prof. E. C. Stirling, of the University of Adelaide, South Australia, Prof. Newton communicated to the Zoological Society of London, at its meeting last week, a figure of the new Australian Marsupial, originally described by Dr. Stirling in this journal in 1888 (NATURE, vol. xxxviii. p. 588), together with some notes on this extraordinary animal. *Notoryctes typhlops*, as Dr. Stirling now proposes to call it, is a small mole-like animal belonging to the order of Marsupials, of which it forms an entirely new type. A general description of it has already been given, as above referred to, but Prof. Stirling now adds that the Marsupial bones are exceedingly small nodules, and escaped his notice at first. Four or five of the cervical vertebræ are fused, and there is a keeled sternum, an enormously thick and short first rib, which serves a purpose of buttressing the sternum in lieu of coracoids, and a bird-like pelvis. The penis is in the uro-genital canal, and the testes are external in front of it. The eyes are mere spots underneath the skin. The four specimens as yet received of *Notoryctes typhlops* were obtained in the centre of Australia, on the telegraph line between Adelaide and Port Darwin. The animal is said to burrow in the sand with great rapidity. A full description of it, it is understood, has appeared in the Transactions of the Royal Society of South Australia, but no copy of this journal has as yet reached England.

MM. GRÉHANT and Quinquaud conclude from some recent experiments on dogs that under the influence of alcohol muscular strength is much diminished.

PROF. JOHN M. COULTER, the well-known botanist, has been elected President of the State University of Indiana, located at Bloomington; and Dr. Douglas H. Campbell has been appointed Associate Professor of Botany at the new Stanford University of California.

ACCORDING to the *Botanical Gazette*, Mr. Thomas Meehan, of Philadelphia, is about to establish, in conjunction with his sons, a new journal of gardening and botanical miscellany. It will be called *Meehan's Monthly*, and the first number will appear on July 1.

WE learn from the *Journal of Botany* that Mr. Worthington G. Smith is preparing for the public gallery of the Botanical Department of the British Museum a series of 96 tables illustrating the British Fungi. Every species of the Hymenomycetes will be figured in its natural colours, the drawings being taken from Mr. Smith's own series already in the Museum, with others from original figures lent by Mr. Plowright and others.

THE number of *Neptunia* for April 30 gives a brief description of the following stations for the study of natural history:—

A marine zoological station has been founded at Endoume, near Marseilles, by Prof. Marion, especially for the study of the fishes of the Mediterranean. M. Alphonse Biosson is about to establish at his own expense a zoological station at Point-de-Grave, Gironde, with the especial object of promoting the investigation of the ornithology and entomology of the district. A marine station for physiology has been opened at Tamaris, near Toulon, under the direction of Dr. R. Dubois, Professor of Physiology in the Faculty of Sciences at Lyons.

The Berlin Academy of Sciences has recently made the following grants:—£100 to Dr. Fleischmann, of Erlangen, for researches in development; £30 towards the cost of publication of Dr. Krabbe's work, "Development-History and Morphology of the polymorphous Lichen Genus *Cladomia*"; £60 to Dr. Hartwig, of Bamberg Observatory, towards a series of observations on variation of the earth's axis; and £40 to Dr. Schmidt, of Halle, for researches on the light reflected from transparent bodies.

The following are subjects for prize competition, recently proposed by the Belgian Academy of Sciences:—Advancement of our knowledge of the relation of phenomena of solution to phenomena of combinations; discussion, on the basis of new experiments, of works relating to the kinetic theory of gases; perfection of the theory of approximative integration, both as regards rigour of methods and facility of application; researches on the embryonal development of a mammal belonging to an order the embryogeny of which has not yet been investigated; determination, by means of palæontology and stratigraphy, of the relations between formations referred by Dumont to his Laekenian and Tongrian marine systems; new researches on the formation of polar bodies of animals. The prizes are gold medals, of the value of 1000, 800, and 600 francs. Papers to be written in French, Flemish, or Latin, and sent to the Secretary before August 1, 1892.

Messrs. Richard Frères have issued an illustrated catalogue of measuring, controlling, and self-registering instruments for scientific and industrial purposes. A descriptive and illustrated list of instruments has also been published by the Cambridge Scientific Instrument Company.

The series of lectures annually given in the gardens of the Royal Botanic Society of London upon subjects connected with botany came to an end on Friday last, when Prof. Stewart, F.R.S., President of the Linnean Society, addressed a large number of visitors and students upon "The Relationship between Plants and Animals." The subject, he said, was one of much interest, as affording an explanation of the origin of many abnormal forms of vegetable growth. This is specially the case in tropical countries, where the struggle for existence is more intense than in colder climes; there the relationship is almost vital, some plants providing food, others shelter, to various kinds of ants, while these pugnacious insects, in turn, protect the plant from damage, by attacking any living thing which approaches it. One plant, known as the bull's-horn acacia, of Central America, provides a species of ant not only with food and drink, in the shape of tiny egg-like bodies upon the leaves—of which the ants are very fond—and a sweet fluid in special cavities on the stalk, but, in addition, furnishes a home in the hollow spines with which it is armed, these, when punctured by the ants, swelling out into perfect miniature bull's horns. In return the ants protect it from its enemies.

A series of experiments with regard to evaporation from free water surfaces and from earth saturated with water, in sun and in shade, has been recently made by Signor Battelli (*Il Nuovo Cimento*). He used three large tubs or vats, two holding water, and the third earth on a grating, to which water was admitted from a pipe entering the bottom. One water-tub and the earth-tub stood a few yards apart on the north side of a high wall; the other water-tub was in the open, and embedded in the ground. Signor Battelli's results are these:—The quantity of water evaporated from moist earth is in general greater than that from a free stagnant water surface, when the air temperature rises; but less, when the latter falls. With increasing wind-velocity, evaporation increases more rapidly from the water surface. The moister the air, the greater (other things equal) seems to be the ratio of the water evaporated from the moist earth to that from the stagnant water surface. The evaporation of a water surface exposed to the sun's rays is greater than that of a shaded one, not only by day, but in the following night. With rising temperature, the ratio between the water quantities from these two surfaces increases somewhat more quickly; with rising wind-velocity, this ratio diminishes.

The *Photographic Journal* of May 22 prints a paper by M. Léon Vidal, on photographic methods of obtaining polychromatic impressions. One of the writer's objects is to show that typographic and lithographic printers ought to find in photography "one of their principal auxiliaries." By its aid, he says, their work might be executed "more cheaply, more thoroughly, and more artistically."

On Sunday, June 7, there was a series of severe earthquake shocks in Italy. The centre of the seismic movement seems to have been in the province of Verona, but the disturbance was felt over a wide area. At Verona three strong shocks, preceded by a subterranean noise like the roaring of artillery, are reported to have occurred at 2 o'clock a.m. The inhabitants rushed in terror from their houses to seek safety in the open streets and squares. One of the assistant mistresses at a boarding-school died of fright. A number of chimneys were thrown down by the oscillation. Still more violent were the effects of the seismic disturbance at other places in the province of Verona, especially at Tregnago and Badia-Calavena. Shocks more or less severe were experienced at Brescia, Belluno, Ravenna, Parma, Modena, and Ferrara. The Central Meteorological Bureau reports that the earthquake was very strongly felt at Florence, where it awoke several people from their sleep. The disturbance also extended to Rome, as was shown by the seismograph, the time at which the shock was felt in Rome being 6 minutes and 40 seconds after 2 a.m. In Verona and the surrounding districts slight shocks continued to be felt on Monday and Tuesday. A large stream of lava issued on Monday from the new crater of Mount Vesuvius at the base of the central cone. Signor Palmieri, the Director of the Vesuvian Observatory, holds that this flow is directly connected with the earthquake shocks in the north, and points out that seismic disturbances in Italy generally stop when the eruption of Vesuvius begins.

In the Report of the Meteorological Service of the Dominion of Canada for the year ending December 31, 1887, just issued, it is stated that nearly eleven hundred warnings of approaching storms were issued by the Service during the year, and that of these warnings 972 were verified, being 88·9 per cent.

We have the pleasure of recording the issue of the first volume of the Publications of the Vatican Observatory, containing astronomical and meteorological observations for the last nine months of 1890. This Observatory was first established by Pope Gregory XIII. for astronomical purposes, and was used for regular meteorological observations from 1800–1821. After passing through several vicissitudes, a proposal was made, about the time of the Vatican Jubilee Exhibition in 1888, to reorganize the Observatory, and the present Pope accordingly re-established it on a sound basis, and it is now furnished with the best instruments procurable, both for direct observations and continuous registration in meteorology, astronomy, magnetism, and earthquake phenomena. It is proposed to carry on various researches, and to issue further volumes from time to time, as soon as sufficient materials are accumulated. The Director is Padre Denza, the founder of the Italian Meteorological Society, and Superintendent of the Observatory at Moncalieri.

CONSIDERING the question of determination of the evaporating power of a climate, Dr. Ule distinguishes (*Met. Zeits.*) between the intensity and the speed of evaporation. The latter can be well determined with an instrument like Wild's evaporimeter, and Dr. Ule sets forth, in a table, the monthly data of this for Chemnitz, compared with those of absolute humidity, "saturation deficit," and relative humidity. The agreement of the last with the evaporimeter figures is much better than that of the two others; still, there is considerable discrepance, and this is not explained (the author shows) by variations in wind-intensity. On the other hand, the data of the psychrometer show a remarkable parallelism with those of the evaporimeter, and by taking wind-variations into account the agreement is increased. Thus, from psychrometer-differences and wind variations, the evaporative power of a climate may be correctly estimated where an evaporimeter is wanting. Dr. Ule offers a new formula for estimating the layer of water evaporated in a given time, and tests it with two German climates, and one Australian.

IN an interesting paper on technical education in agriculture, reprinted from the Journal of the Royal Agricultural Society, Dr. W. Fream refers incidentally to the value of mathematical studies for the agriculturist. Dr. Fream's professorial experience at agricultural colleges has convinced him that a lad who is fairly competent in mathematical studies is "a good medium to work upon." "Those interested in the welfare of any young agriculturist should take care," he says, "that in his school days the study of mathematics is not ignored. The time devoted to acquiring proficiency in arithmetic, geometry, mensuration, and the elements of algebra and trigonometry—the latter really indispensable in the case of surveying—will never be regretted."

THOSE who are interested in questions relating to physical education will find much to please them in an excellent paper, in the June number of *Physique*, on natural history in public schools, by the Rev. T. A. Preston, late President of the Marlborough College Natural History Society. Many boys are not much attracted by games, and it seems hard that in such cases any sort of compulsion should be used. Why not have various alternative ways of securing exercise, any one of which might be chosen? Mr. Preston shows with great force, and in a very interesting manner, with how much advantage the study of natural history might in some instances be substituted for cricket and football. Boys out for a field excursion take a great deal more exercise, he maintains, than is ever taken at cricket. "With those who are keen naturalists," he says, "the mere exercise taken in any one day (not in an excursion) is often such that it might almost be said to require moderating. I have no hesitation in saying that, if exercise alone is to be considered, a field naturalist will take far more than any one at games."

MR. W. R. HILLIER, of the Indian Civil Service, has written a very curious monograph on the manners and customs of the Shan States. When a Shan becomes a father it is considered highly undesirable that he should drive pigs, carry the dead, bore holes, fill in holes in the ground, or indulge in mockery. "If either sex," writes Mr. Hillier, "die without marrying, the body, before burial, is banged against a stump, which is at the time considered as representing the husband or wife,"—a ceremony which is supposed to guard against the danger of unrequited affection in the next stage of existence. Marriage is simplicity itself. A young man takes a fancy to a young lady, and if the liking is reciprocated, she straightway accompanies him to his house as his wife. Next day the young man's parents meet the parents of the young lady, and after informing them of what has taken place, beg that "they may be forgiven for the intrusion," and ask that a day be fixed for the wedding. This request being granted—and apparently a refusal is not contemplated—the young lady returns to her parents. Divorce is easy also, the man merely giving his wife a letter permitting her to remarry, and the wife merely being required to pay an unwilling husband thirty rupees for release from an uncongenial mate. As to food the Shan is not an epicure, eating everything that is eatable; and indeed it is considered quite becoming, if he only be of high rank, to devour an enemy. This privilege, however, is accorded only to Bohs, or chiefs. The Shan theory of the cosmogony is that "the earth came out of the depths by means of white ants."

SOME further explorations have lately been made on the Upper Irrawaddy. Major Hobday, of the Indian Survey Department, with an escort of fifty Goorkhas, succeeded in getting as far north as latitude 26° 15′ up the Malika, or right branch of the river. Here the local tribes began to show opposition, and the party could not without fighting their way have proceeded further. The point reached was, however, only fifty miles south of that which Colonel Woodthorpe gained a few years ago in his explorations from the far north of Assam. This small gap will probably be crossed when the next attempt is made, as by that time the wild tribes will have learned from their neighbours that British officers have only friendly intentions towards them. Finding his progress barred to the north, Major Hobday turned due eastwards, with the intention of striking the Meka, which is supposed to be the main stream of the Irrawaddy. After exploring the course of this river for some distance, he will journey back through the hills along the Yunnan border, reaching Bhamo by land. He will thus be able to map a considerable extent of country.

AN interesting synthesis of troilite, the crystallized monosulphide of iron, FeS, which is so frequently found in meteorites and yet is never found in terrestrial rocks, is described by Dr. Richard Lorenz, of Göttingen, in the current number of the *Berichte*. A stream of dry sulphuretted hydrogen gas was led over a bundle of iron wire contained in a combustion-tube heated in a furnace. As soon as the wire became heated to dull redness, it became quite changed, becoming completely covered with innumerable brilliant little crystals. These crystals possessed a bright silver-white lustre when first obtained, but after a short time reflected a pale-green coloured light. On standing for some days, the crystals further changed in colour to blue and afterwards to brown, without the least change in the form being apparent. Under the microscope they appear to consist of well-formed six-sided tables of a bright steel-gray lustre. Prof. Groth, the eminent crystallographer, who has examined them, pronounces them to be hemimorphic hexagonal in form, isomorphous with wurtzite, the hexagonal variety of zinc sulphide. Any kind of iron may be substituted for the wire; whatever the variety employed, it always becomes covered with a crust of these crystals when heated in a stream of sulphuretted hydrogen, the only precaution necessary being to prevent the temperature from rising to the melting-point of monosulphide of iron. The crystals are readily detached from the iron, and upon analysis yield numbers very near the theoretical ones required by FeS. The largest and best developed individual crystals of troilite are obtained by diluting the sulphuretted hydrogen with an inert gas. Wurtzite, sulphide of zinc, ZnS, may also be readily artificially obtained in a similar manner by passing sulphuretted hydrogen over zinc heated to whiteness in a porcelain tube in a Schlösing furnace. When the tube, which is allowed to cool in the stream of gas, is broken, immediately beyond the portion which has been heated in the furnace a beautiful sublimate of crystals of wurtzite is found. They consist of well-developed hexagonal prisms, somewhat transparent and of a yellow colour, exhibiting, according to Prof. Groth, their hemimorphic nature in a most decided manner. In a similar way also Dr. Lorenz has artificially prepared greenockite, sulphide of cadmium, CdS. This synthesis is perhaps the easiest of all to effect, and it may readily be conducted in an ordinary combustion-tube. The metallic cadmium is placed in a porcelain boat, and commences to react with the sulphuretted hydrogen at a temperature just below its boiling-point. As soon as this temperature is attained, the porcelain boat and the portion of the tube beyond it become covered with magnificent long yellow skewer-like crystals of greenockite, which Prof. Groth finds to be of two kinds, hexagonal prisms isomorphous with troilite and wurtzite, and a new form of greenockite consisting of monoclinic crystals. Dr. Lorenz has further artificially prepared millerite, the sulphide of nickel, NiS, by the same method, obtaining in this case very minute but undoubtedly hexagonal crystals isomorphous with the three other sulphides above described.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mr. Walter Fraser; a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Colonel Beresford; a Great Black-headed Gull (*Larus ichthyaëtus*) from the Persian Gulf, four Macqueen's Bustards (*Houbara macqueeni* ♂ ♂ ♀ ♀) from Western Asia, three Chaplin Crows (*Corvus capellanus*) from Persia, presented by Mr. B. T. Ffinch, C.M.Z.S.; a Diamond Snake (*Morelia spilotes*) from New South Wales, presented by Mr. J. Hellberg; a Common Viper (*Vipera berus*) from Hampshire, presented by Mr. W. H. B. Pain; two Piapecs (*Ptilostomus senegalensis*) from West Africa, purchased; a Collared Fruit Bat (*Cynonycteris collaris*), four North African Jackals (*Canis anthus*), two Partridge Bronze-wing Pigeons (*Geophaps scripta*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE SPECTRA OF DOUBLE STARS.—A note on "The Discovery of Double Stars by means of their Spectra" is contributed by Prof. E. C. Pickering to *Astronomische Nachrichten*, No. 3034. When the components of a close binary system have similar spectra, relative orbital motion in the line of sight may cause a periodic doubling of the lines. But if the spectra be not similar any lines common to both ought to be conspicuously strong, and, provided the components have not equal and opposite velocities in the line of sight, ought also to be displaced with reference to other lines. Thus, if one component of a close binary system has a Group V. spectrum, like our sun, and the other a Group IV. spectrum, in which strongly marked hydrogen lines is the main feature, the resulting spectrum will have a composite character, and careful measurements should show that the position of the hydrogen line is periodically displaced when compared with the lines characteristic of the solar-type spectrum. α Canis Majoris is the brightest star having this composite spectrum, and the wave-length of the hydrogen line G, derived from a comparison with three lines of greater and three lines of smaller wave-length, was 434·09, which exceeds that derived from the solar spectrum by 0·03. Similar measures of the hydrogen line *h* gave a wave-length of 410·22, which also exceeds that in the solar spectrum by 0·03. From this displacement it would appear that if the phenomenon is due to the relative motion of a faint component, it is receding at the rate of 20 kilometres per second, as compared with the bright component. An examination shows that the following stars have the composite spectrum referred to: γ Andromedæ, H.P. 650, ε Boötis, α Scorpii, and β Cygni, all of which are known to be double; also π Persei, ζ Aurigæ, δ Sagittarii, 31 Cygni, and β Capricorni. In the cases of the last two, the spectra of the distant companions are distinctly separated from those of the chief stars. Although the strong hydrogen lines in the spectra investigated may be due to the presence of a faint companion, their intensity may also be due to many other causes. Thus, the strong hydrogen lines in the solar spectrum are not due to the integration of the spectrum of the sun and that of a companion. It is necessary, therefore, to determine whether the displacement is subject to a periodic variation or not, in order to test this method of discovering close binaries.

THE PERSEID RADIANT.—At the St. Petersburg Academy of Sciences, on April 22, M. Bredichin concluded, from the meteor observations made at Pulkova by ten astronomers in August 1890, "le courant des aérolithes n'est pas délimité par un point ou un petit rond, mais présente une surface considérable parsemée de radiants."

THE FLORA OF DIAMOND ISLAND.

DIAMOND ISLAND is situated at the mouth of the Bassein River, in the Indian Ocean, about five miles from Pagoda Point and eight miles from Cape Negrais, and in about 16° N. lat. It is of sandstone formation, somewhat exceeds a square mile in area, being about twice as long as broad, and the central part is a kind of plateau 60 feet or so above the level of the sea. With the exception of a small clearing for a telegraph station, the island is densely wooded down to the sea, but there is no mangrove belt on any part of the sandy coast, unless it be considered as represented by a few patches of *Avicennia officinalis*. Thus is the island described, though in greater detail, by Dr. D. Prain, Curator of the Herbarium of the Royal Botanic Garden, Calcutta, who has visited the island in H.M. Indian Marine Survey steamer *Investigator*, commanded by R. F. Hoskyn, R.N. Dr. Prain has published an elaborate analytical account of the flora in the Journal of the Asiatic Society of Bengal. He collected eighty-six species of flowering plants, three ferns, and four funguses, among which there was not a single novelty. The enumeration includes a number of cultivated plants, among them the coco-nut palm; but these are all of recent introduction. It is supposed that the island was not previously inhabited, and therefore that the vegetation of the dense wood overspreading the island is quite natural. The most interesting fact brought out is the evident affinity with the somewhat distant Andaman flora, pointing to a former connection. The Report is also valuable to the student of plant-distribution for the details it contains of the habitats and relative frequency of the component species of the vegetation.

W. BOTTING HEMSLEY.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Lord Walsingham, F.R.S., the High Steward elect, has issued a letter of thanks to the Senate, in which he promises to maintain the highest traditions of "our beloved University."

W. M. Hicks, F.R.S., late Fellow of St. John's College, and Principal of Firth College, Sheffield, has been approved for the degree of Doctor in Science. Dr. Hicks is the author of many important memoirs in mathematical physics, and of an approved text-book of dynamics.

Prof. Newton has been appointed a Manager of the Balfour Studentship Fund for five years.

A. H. L. Newstead, Scholar of Christ's, and E. W. MacBride, Scholar of St. John's, and President of the Union Society, have been nominated for research work at the Naples Zoological Station.

The Syndicate appointed for the purpose have selected a site for the Sedgwick Memorial Museum on the old Botanic Garden area, with a frontage to Downing Street. The proposed Museum will lie between the new Chemical Laboratory and the old Anatomical School, and complete one quadrangle of the new Museums group.

The following distinguished persons are proposed recipients of honorary degrees on June 16:—Lord Walsingham, F.R.S., the Marquis of Dufferin and Ava, K.P., G.C.B., Prof. Rudolf von Gneist, of Berlin, Sir Alfred Lyall, K.C.B., Sir Archibald Geikie, F.R.S., Antonin Dvořák, Prof. Karl Weierstrass, of Berlin, A. H. Taine, member of the French Academy, Dr.

Elias Metschnikoff, Director of the Paris Pasteur Institute, Prof. W. H. Flower, C.B., F.R.S., and Mr. W. E. H. Lecky.

Delegates from the seventh International Congress of Hygiene and Demography will be received by the Vice-Chancellor in the Senate House on Saturday, August 15.

The Museums Association hold their annual meeting in July in the buildings lately erected for the departments of Anatomy and Physiology.

Prof. Foster is appointed by the University a Member of Council of the Marine Biological Association.

The reference to the Syndicate on the question of Greek in the Previous Examination has been enlarged to include Latin also, and will be decided on by the Senate early in the October term. An animated discussion on the question took place in the Arts School, in which the claims of modern (non-classical) education for consideration by the University were strongly put forward by men of the highest classical distinction.

Mr. J. N. Keynes, the Secretary for the Local Examinations, has been approved for the degree of Doctor in Science.

An election to an Isaac Newton Studentship will take place in October. The value is £200 a year for three years from April 15, 1891. Candidates are to send their names and testimonials to the Vice-Chancellor between October 1 and 10.

It is proposed to affiliate the University to the University of Adelaide, South Australia.

The General Board of Studies propose that Dr. Ruhemann, hitherto Assistant to Prof. Dewar, shall be appointed a University Lecturer in Organic Chemistry.

A room in the new Physiological Laboratory is to be set aside for Psychophysics, and a grant of £50 for instruments is recommended by the General Board.

The Annual Report of the University Observatory contains a good record of work done and in progress. Prof. Adams is to be congratulated on the satisfactory way in which, notwithstanding his long and severe illness, the Observatory has been conducted.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 14.—"On the Theory of Electrodynamics." By J. Larmor, Fellow of St. John's College, Cambridge. Communicated by Prof. J. J. Thomson, F.R.S.

The electrical ideas of Clerk Maxwell, which were cultivated partly in relation to mechanical models of electrodynamic action, led him to the general principle that electrical currents always flow round complete circuits.

When this principle of circuital currents is postulated, the theory of electrodynamics is reduced to the Ampère-Neumann theory of complete circuits, of which the truth has been fully established. It leads, as shown by Maxwell, to the propagation of electrical action in dielectric media by waves of transverse electric displacement, which have the intimate relations to waves of light that are now well known.

The problem of determining how far these remarkable conclusions will still hold good when a more general view of the nature of dielectric polarization is assumed was considered by von Helmholtz in a series of memoirs.

The most general conception of the polarization of a medium which has been formed is the Poisson theory of magnetization. The magnetized element, whether actually produced by the orientation of polar molecules, or otherwise, may be mathematically considered to be formed by the displacement of a quantity of ideal magnetic matter from its negative to its positive pole, thereby producing defect at the one end, and excess at the other end. The element is defined magnetically by its moment, which is the product of the displaced quantity and the distance through which it is displaced. The displacement per unit volume, measured by this product, is equal to the magnetic moment per unit volume, whether the magnetized molecules fill up the whole of that volume or are a system of discrete particles with unoccupied space between them.

In the electric analogue we replace ideal magnetic matter by ideal electric matter; the displacement thus measured constitutes the electric displacement, and its rate of change per unit time represents the displacement current in the dielectric. We have to consider whether a displacement current of this type suffices to make all electric currents circuital; and it will be sufficient and convenient to examine the case of a condenser which is charged through a wire connecting its two plates. In the first place, this notion of electric displacement leads to the same distribution of potential between the plates as the ordinary one, adopted by Maxwell; for in the theory of induced magnetism there occurs a vector quantity of circuital character, the magnetic induction of Maxwell, of which the components are $-\mu(dV/dx)$, $-\mu(dV/dy)$, $-\mu(dV/dz)$, and which, therefore, leads to the characteristic equation of the potential

$$\frac{d}{dx}\left(\mu\frac{dV}{dx}\right)+\frac{d}{dy}\left(\mu\frac{dV}{dy}\right)+\frac{d}{dz}\left(\mu\frac{dV}{dz}\right)=0,$$

corresponding to the one given above. If the displacement in the dielectric is $-\kappa(dV/dx)$, $-\kappa(dV/dy)$, $-\kappa(dV/dz)$, then

$$\mu = 1 + 4\pi\kappa.$$

The displacement in a unit cube may, of course, be considered as a displacement across the opposite faces of the cube.

Now, considering the case of a plane condenser, let F be the electric force in the dielectric between the plates; then the displacement is κF. Let σ be the surface density of the charge conducted to a plate; then the effective electrification along that plate will be of surface density $\sigma' = \sigma - \kappa F$; therefore, by Coulomb's principle,

$$\begin{aligned} F &= 4\pi\sigma' \\ &= 4\pi(\sigma - \kappa F); \end{aligned}$$

so that

$$\sigma = \frac{\mu}{4\pi}F = \kappa F + \frac{1}{4\pi}F.$$

Thus the current is not circuital, but there is an excess of the surface density conducted to the surface over the displacement current from the surface, which is equal to $F/4\pi$.

The specific inductive capacity, as determined by static experiments on capacity, is here measured by μ, the coefficient in the expression for σ.

In addition to this discontinuity at the face of a condenser plate, the induction in the mass of the dielectric will not be circuital unless the electric force is itself circuital, which it is not in the general form of the electrodynamic theory.

The most general type of electrodynamic relations which is consistent with the established theory of complete circuits, is discussed on the basis of von Helmholtz's work, but with avoidance of certain restricting conditions introduced by him, the chief conclusion being as follows:

In a complete circuit the one thing essential to the established theory is that the electric force integrated round the circuit should be equal to the time rate of change of the magnetic induction through it, and, therefore, have an ascertainable value, though its distribution round the circuit is a subject of hypothesis. The conclusion that waves of transverse displacement will be propagated in a dielectric with velocity $K1^{-\frac{1}{2}}$ will hold good if we assume any form whatever for the electric force which does not violate this one relation, and also assume an electrostatic polarization of the medium, equal at each point to the electric force multiplied by a constant $K_1/4\pi$.

The increased generality which can be imparted to the theory merely leads to various modes of propagation of a condensational wave.

If K_2 denote the specific inductive capacity of the medium, measured in static units, this polarization constant K_1 is equal to $K_2 - 1$; and the velocity of the transverse waves is the ratio of the electric units of quantity in a medium of unit inductive capacity multiplied by the static value of $K_1^{-\frac{1}{2}}$. The correspondence of the refractive index for the simpler media with $K_2^{-\frac{1}{2}}$, as well as direct measures of the relative velocities of electric waves in other media, give for the value of this velocity the same ratio multiplied by $K_2^{-\frac{1}{2}}$. These values can be reconciled only by the limiting form of the theory of polarization which is equivalent to Maxwell's theory.

May 28.—"On the Anatomy and Physiology of *Protopterus annectens*." By W. N. Parker, Ph.D., F.Z.S., Professor of Biology in University College, Cardiff. Communicated by W. H. Flower, F.R.S.

The work which has resulted in the present paper was begun in Freiburg in the summer of 1888, when the author was fortunate enough, owing to the generosity of Prof. Wiedersheim, to obtain a number of fresh specimens for examination. As so many interesting points presented themselves at an early stage in the research, a short preliminary notice, without illustrations, was published in the following autumn (*Berichte d. Naturforsch. Gesellschaft zu Freiburg i. Br.*, vol. iv. 1888; see also NATURE, vol. xxxix. p. 19) This notice merely forms the basis of the present paper, in which the whole subject has been worked out in greater detail. A number of new facts are recorded, some of the author's earlier conclusions modified, and the paper illustrated with 11 plates containing 71 figures.

With the exception of certain special details, the structure of the skeleton and of the nervous and muscular systems are not described, the paper consisting mainly of an account of other organs which have not received so much attention from previous observers, and of a comparison of *Protopterus* with the other genera of Dipnoi, so far as their structure is known, as well as with other Ichthyopsida.

A number of details with regard to the habits of *Protopterus* in captivity are given.

The paired extremities show no connection with the cheiropterygium, and, in spite of their considerable nerve-supply, are evidently greatly degenerated structures as regards their free portions. Sensory organs are not present on them, and they therefore cannot have a tactile function. Their distal ends, like the apex of the tail, are very variable, and can undoubtedly be reproduced when lost by accident. The tail is almost certainly not primarily diphycercal, and shows signs of a possible origin from a heterocercal form.

The epidermis on the whole most nearly resembles that of Perennibranchiate Amphibians, and gives rise to simple multicellular glands, as well as to very numerous closely-packed goblet-cells, which produce the gluey secretion as well as the main substance of the capsule which surrounds the animal during the torpid state.

The integumentary sense-organs are similar to those of fishes and larval Amphibians. The relations of the sensory organs of the trunk are similar to those seen in young stages of Fishes and in Amphibian larvæ, while in the case of the head they resemble those which are typical for adult Fishes. End-buds, similar in structure to the taste-buds of Fishes and Amphibians, are present on the tongue and oral epithelium.

As regards its general structure, the olfactory organ most nearly resembles that of Elasmobranchs, but the presence of posterior nostrils raises it to a higher level. The position of the anterior nostrils beneath the upper lip is probably to be accounted for as an adaptation in connection with the torpid state. Four straight and two oblique muscles are present. The sclerotic is fibrous in young animals, and islands of cartilage first appear at the points of insertion of eye-muscles, and then gradually extend so as to chondrify the whole. The eye resembles that of Amphibians; a *processus falciformis* and *campanula Halleri* are absent, and no ciliary muscles were observed, though possibly present; almost all the pigment of the eye is ectodermic.

No specialized glands are present in connection with the greatly folded epithelium of the oral cavity. The lips contain no muscles. The tongue, as well as the palate, is covered with blunt conical papillæ, on which the taste-buds are situated. A horny cap is developed over each tooth from the overlying epithelium, which apparently becomes cut through by the sharp edges and points of the teeth, and which probably corresponds to the *cuticula dentis*. The thyroid and thymus are described.

A ventral, as well as a fenestrated dorsal, mesentery is present supporting the intestine. The author compares the so-called urinary bladder ("cloacal cæcum") with the "processus digitiformis" of Elasmobranchs. A spleen and pancreas are present, embedded in the thin walls of the stomach, and extending on to the proximal part of the intestine; they are covered externally by sparse muscular fibres as well as by the peritoneum. The relations of the pancreas therefore most nearly resemble those met with in Ganoids and certain Teleosteans. The pancreas is deeply pigmented, and its ducts open into the bile-duct. The pigmented walls of the intestine and the spiral valve are very thick, owing to the abundance of lymphoid tissue contained within them. With the exception of the bursa entiana, the internal walls of which are raised up into a number of deeply pigmented oblique folds, the whole of the mucous membrane of the stomach and intestine is perfectly smooth, and there is no indication of any differentiated gastric or intestinal glands.

Cilia are present on the epithelium throughout the stomach and intestine. A layer of small-celled lymphoid tissue directly underlies the epithelium. In the spleen and lymphoid organs of the intestine two kinds of tissue are present. Large migratory cells are present in both kinds of tissue, many of which inclose yellowish granules. Gradations between these and rounded cells of a deeper yellow or brown colour can apparently be made out, and cells appearing to be intermediate forms between these and the ordinary black, branched pigment cells can also be seen. It seems probable that the yellow granules mentioned above are due to the disintegration of red corpuscles, which are ingested by leucocytes, and then undergo some change, whereby the latter gradually pass into the condition of black pigment cells, which migrate through the epithelium, and are so got rid of. The muscular layers are very thin.

The question as to the mode of digestion and absorption of the food in *Protopterus* is discussed.

The branchial apparatus shows signs of considerable reduction. The pulmonary apparatus, on the whole, more nearly resembles the air-bladder and its duct of certain Ganoids than the lungs and laryngo-tracheal chamber of Amphibians. The pulmonary branches of the vague cross one another at the base of the lungs.

The blood is remarkable for the large size of its elements, which is only exceeded in the case of *Proteus* and *Siren*, as well as for the large proportion of white corpuscles in comparison with the red ones. Two forms of the former are described. The chief points of interest with regard to the blood-vessels are: (1) the paired pulmonary artery, the left supplying the ventral, and the right the dorsal, aspect of the lungs; (2) the single post-caval and persistent left posterior cardinal vein; and (3) the single caudal vein, giving rise to a right and a left renal portal.

No external sexual differences could be observed, and amongst the specimens examined, females were the more abundant. The urino-genital organs are surrounded by masses of tissue resembling the large-celled lymphoid tissue of the gut, but differing from the latter in becoming largely converted into adipose tissue. The kidneys probably represent the mesonephros, and their duct the Wolffian duct; nephrostomes are absent.

In unripe males, delicate Müllerian ducts are present. The sperm is conducted to the exterior by a duct, which is probably formed in connection with the testis, quite independently of the excretory apparatus. The seminal tubules are directly connected with it, and it opens into the base of the Müllerian duct, the rest of which apparently aborts completely. Unlike most of the tissue elements, which are very large, and closely resemble those of the Amphibia, the spermatozoa are very minute, and are remarkable in possessing *two* vibratile flagella attached to the carrot-shaped "head." The generative organs of the female bear a striking resemblance to those of Amphibians. The oviduct corresponds to the Müllerian duct; the epithelium covering its internal folds shows signs of degeneration similar to those which have recently been described amongst Urodeles.

An account of the mode of life of *Protopterus* during the torpid period is given. The cocoon is provided with a "lid," perforated by a hollow funnel-shaped tube, which passes between the lips of the animal, and thus forms a passage for the respiratory current. The source of nutriment during the summer sleep lies in the adipose tissue in connection with the gonads and kidneys and alongside the notochord in the tail, as well as in the lateral muscles, some of which, especially in the caudal region, undergo a granular degeneration. Very probably the latter is the precursor of the fatty degeneration, and, in all probability, leucocytes are the active transporting agents of the degenerated material. This assumption would help to explain the large development of lymphoid tissue in the body of the animal.

The systematic position of the Dipnoi is briefly discussed in the light of the new facts brought forward in the present paper. Although the Dipnoi present many points of resemblance to Fishes on the one hand, and to the lower Amphibians on the other, their connection with any living forms of either class is probably a very distant one, and it is inadvisable to classify them amongst the Fishes. Owing to the absence of ontological evidence, and to the incompleteness of our knowledge of the palæontological history of the Dipnoi, it is impossible to construct a genealogical tree which will show, with any approach to accuracy, the probable connection between them and other Ichthyopsidan types. The most that can be said at present, with anything like certainty, is that the Dipnoi are the isolated survivors of an exceedingly ancient group, which was probably related to the ancestors of existing Fishes and Amphibians. Amongst the former, the connection seems to be closest to the

Elasmobranchs, more particularly to the Chimæroids on the one hand, and to such an ancient Selachian type as *Chlamydoselache* on the other; but, at the same time, the Ganoids probably arose from the common ancestral stock not very far off. Though retaining many primitive characters, the Dipnoi, and more especially *Protopterus* and *Lepidosiren*, are in some respects highly specialized, the specialization being largely due to a change of habit.

"Method of Indexing Finger Marks." By Francis Galton, F.R.S.

Sufficient proof was adduced by me in a memoir read November 27, 1890, before the Royal Society (Phil. Trans., B, 1891), of the extraordinary persistence of the papillary ridges on the inner surface of the hands throughout life. It was shown that the impression in ink upon paper of each finger tip, contained on the average from twenty-five to thirty distinct points of reference, every one of which, with the rarest exception, appeared to be absolutely persistent. Consequently that it was possible to affirm with practical certainty whether or no any two submitted impressions were made by the fingers of the same person.

In the present memoir I shall explain the way in which finger prints may be indexed and referred to after the fashion of a dictionary, and on the same general principle as that devised by A. Bertillon with respect to anthropometric measures, whose ingenious method is now in regular use on a very large scale in the criminal administration of France and elsewhere. I desire to show how vastly the practical efficiency of any such method as that of A. Bertillon admits of being increased by taking finger prints into account in the way to be described.

It must not, however, be supposed that the use of indexing finger marks is limited to the above purpose, the power of doing so being equally needed for racial and hereditary inquiries. I do not dwell upon these applications now, simply because I am engaged in making them, and the results are not yet ready to be published. I ought, however, to mention that a great increase of experience has fully confirmed my earlier views, that finger marks are singularly appropriate subjects of anthropometric study owing to many distinct reasons. The impressions are easily to be made by anyone who has the proper appliances at hand. They are as durable as any other printed matter, and they occupy very little space. The patterns are usually sharp and clear, and their *minutiæ* are independent of age and growth. They are necessarily trustworthy, and no reluctance is shown in permitting them to be taken, which can be founded either upon personal vanity or upon an unwillingness to communicate undesirable family peculiarities.

Without caring to dwell on many of my earlier failures to index the finger prints in a satisfactory way, my description shall be confined to that which has proved to be a success. It is based on a small variety of conspicuous differences of pattern in each of many digits, and not upon the numerous minute peculiarities of a single digit. My conclusions are principally based on a study of the impressions of all 10 digits of 289 different persons, but the tables given in the memoir refer only to the first 100 on my list. These are sufficiently numerous to serve as a fair sample of what we might always expect to find, while they are not too cumbrous to print and to discuss in full detail.

Though I have spoken and shall speak only of impressions, it is not really necessary in forming an index to make any impression at all. All the entries that appear in it may be derived directly from the fingers themselves.

I rely, for the purpose of indexing, on the three elementary divisions of primaries, whorls, and loops. They are severally expressed by the numerals 1 and 2, 3 and 4, 5 and 6. The reason of this double numeration is that most of the patterns have a definite axis. Those that are formed by ridges which proceed from only one side of the finger, lie in a sloping direction across its axis, the slope being directed according to the side from which the supply of ridges proceeds. All normal slopes, or those that are (roughly) parallel to a line drawn from the tip of the forefinger to the base of the little finger, as well as all the patterns that have no definite axis, are expressed by the odd numerals, 1, 3, or 5. All abnormal slopes are expressed by the even numerals 2, 4, or 6. It cannot be too strongly insisted that the words right and left are ambiguous, and must not be used here.

The forefingers are the most variable of all the digits in respect to their patterns, their slopes being almost as frequently abnormal as not; the third fingers rank next; the little finger ranks last, as its pattern is a loop in nine cases out of ten. I therefore found it convenient not to index the fingers in their natural order, but so that the sequence of the numerals which express the patterns on the digits should be divided into two groups of three numerals, and two groups of two numerals, as 355, 455, 55, 35. The first group 355 referring to the first, second, and third fingers of the left hand; the second group 455 to the first, second, and third fingers of the right hand; the third group 55 to the thumb and fourth finger of the left hand; the fourth group 35 to the thumb and fourth finger of the right hand. The index is arranged in the numerical sequence of these sets of numbers.

Before translating the patterns into numerals, I find it an excellent plan to draw symbolic pictures of the several patterns in the order in which they appear in the impression, or in the fingers themselves, as the case may be, confining myself to a limited number of symbols [a list of those which have thus far sufficed is given in the memoir; 5 of them are symmetrical symbols, and 9 are tailed and duplicated for the reasons given above, one of each pair being inclined to the right, and the other to the left. The total number of these hieroglyphs is consequently 23]. A little violence has of course to be used now and then, in fitting some unusual pattern to one of these symbols. But we are familiar with such processes in ordinary spelling, where the same letter does duty for different sounds, as *a* in the words *as*, *ask*, *ale*, and *all*. The merits of this process are many. It facilitates a leisurely revision of first determinations; it affords an adequate record of the character of each pattern; it prevents mistakes between normal and abnormal slopes; it prevents confusion when changing the sequence of the entries from the order of the impressions to that used in the index; and, lastly, it affords considerable help to a yet further subdivision of the patterns.

In making a large and complete index, the symbols would, of course, be cast as movable types, and be printed with the letterpress.

It appears from the 100 cases that are printed in the memoir that there were 83 different varieties of index numbers when all 10 digits are used. Consequently the average number of references required to pick out a single well-defined case from among these 100 would be equal to 100 divided by 83—that is, to about 1¼. I do not expect from my own reiterated experiences that there would be much trouble due to transitional cases, after a standard collection of doubtful forms had been collected and numbered, so as to insure that different persons should follow a common standard. I find much uniformity in my own judgment.

Owing to the large effect of correlation, an index based on all the 10 digits is not much superior in efficiency to one that is based on six—namely, upon the first three fingers of both hands. In the 100 different sets there are, as already said, 83 varieties of pattern in the one case, and there are 65 in the other, which roughly accords with the relative efficiency of 5 to 4. It is, therefore, a fair question whether it is worth while to impress all the 10 digits. The chief advantage of doing so is to add to the volume of evidence, and to supply data which mutilation, or bad scars, or obliteration due to some exceptional cause might render of value. The three fingers of both hands are more than twice as useful for an index as those of one hand only; again, the three fingers of one hand are nearly twice as useful as two only. I may mention that for my present inquiries into racial and hereditary patterns I am, for various reasons, dealing only with the first three fingers of the right hand, and slightly rolling the forefinger, so as to obtain a full impression of its pattern on the side of the thumb.

When searching through a large number of prints that bear the same index number, in order to find a duplicate of a particular specimen, it is a very expeditious method to fix on some one well-marked characteristic of a minute kind, such as an island, or inclosure, or a couple of adjacent bifurcations, that may present itself in any one of the fingers, and in making the search to use a lens or lenses of low power, fixed at the end of an arm, and to confine the attention solely to looking for that one characteristic. The cards on which the finger marks have been made, may then be passed successively under the lens with great rapidity.

[It is proposed to exhibit specimens illustrative of this and of the previous memoir, together with appliances for taking impressions from the fingers, at the approaching *soirée* of the Royal Society.]

Physical Society, May 22.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Mr. C. J. Woodward exhibited Dr. Schobben's form of lantern stereoscope. This instrument consists of a double lantern, by which the two pictures of a stereoscopic slide are projected on a screen. The two pictures are coloured complementary tints by placing pieces of red and green glass in front of the lenses, and each observer views the overlapping images through spectacles, the eye-glasses of which are also coloured red and green. The stereoscopic effect is very striking. Mr. Boys stated that he had tried to obtain a similar result with the aid of polarized light, by viewing two polarized images through Nicol prisms. No effect was obtained, owing to elliptic polarization produced by the screen, but he thought that if a dead-gold screen had been used instead of an ordinary one, the effect might have been observed.—Prof. Perry, F.R.S., showed a new form of steam-engine indicator. A galvanometer mirror is fixed excentrically to a steel disk, forming one side of a chamber communicating with the cylinder. The pressure of the steam bulges out the disk, and causes the mirror to deflect a ray of light thrown on it in the ordinary way. A rotation of the mirror at right angles to the former is imparted by the movement of the piston-rod. The ray of light traces out the diagram on a screen suitably placed, and the complete figure is continuously visible, owing to the persistence of impressions. This indicator possesses advantages over other forms, in being free from errors due to friction or oscillations of the springs, and the alteration of their elasticity due to temperature changes. The errors of ordinary indicators are considerable at high speeds, owing to the ripples introduced into the indicator diagram. If the natural period of the springs is one-twentieth of the time of a revolution, the diagram is fairly free from ripples, but if it is as much as one-tenth, no amount of friction in the indicator will prevent ripples forming. In the new indicator, the natural period of the disk can be made very short. The steel disks are easily removable, and can be proportioned to suit different pressures and speeds. For experimental and teaching purposes it is advantageous to see at once the alterations in the diagram caused by changes of load, pressure, &c. Several diagrams were exhibited to the meeting. In reply to Prof. Carus Wilson, Prof. Perry stated that the deflection was proportional to the pressure in the cylinder within the limits any particular disk was intended for. Mr. Addenbrooke thought the instrument an important improvement on its predecessors, and considered it would prove of great service to electrical engineers. Mr. Swinburne said a peculiar merit of the indicator was that it could be permanently attached to an engine like an ordinary pressure-gauge. He suggested the use of a small telescope instead of the ray method. The President thought that the instrument could be modified so as to be useful for analyzing the shape of the curves representing alternating currents.—On Blakesley's method of measuring power in transformers, by Prof. Perry, F.R.S. This paper refers to the supposed error in Mr. Blakesley's formula due to the fact that transformers show magnetic leakage. The proofs of the formula hitherto given have been obtained by treating the equations in the manner first adopted by Dr. Hopkinson. On this system the reactions of the primary and secondary currents are represented by the equations—

$$V = R_1C_1 + P\frac{dN}{dt}, \quad 0 = R_2C_2 + S\frac{dN}{dt},$$

where P and N are respectively the turns on the primary and secondary coils, and N is the magnetic flux between the coils. Here it is assumed that there is no magnetic leakage, and the author thinks that on this account the method is inferior to the original method of Maxwell, in which the induced electromotive forces are expressed in terms of coefficients of self and mutual induction. On the assumption that there are no eddy currents, Maxwell's equations are—

$$V = R_1C_1 + L_1\dot{C}_1 + M\dot{C}_2; \quad 0 = R_2C_2 + MC_1 + L_2\dot{C}_2,$$

in which although L_1, M, and L_2 may not be constant, it may be assumed that they are respectively proportional to P^2, PS, and S^2, if there is no magnetic leakage; and if the amount of magnetic leakage bears a constant proportion to the whole flux, the three quantities may still be assumed proportional to each other, although M^2 is less than L_1L_2. From these equations we obtain

$$VC = R_1C_1^2 - R_2\frac{M}{L_2}C_1C_2 + \frac{L_1L_2 - M^2}{L_2}C_1\dot{C}_1.$$

Hopkinson puts down the last term as zero, but owing to the very rapid rate at which C_1 changes, the last term is very important, even though M may be but a small percentage less than $\sqrt{L_1L_2}$. On integrating this equation, the first two terms on the right-hand side yield Blakesley's formula, and the last term vanishes in the integral, because, however great the magnetic leakage may be,

$$\int C_1\dot{C}_1 dt = 0,$$

when taken over a period because the functions are periodic. Mr. Blakesley's formula thus appears to hold, whatever the magnetic leakage. The paper contains several tables of calculations showing the effect of magnetic leakage on the value of the terms in the equation. Mr. Blakesley said he doubted the correctness of the assumption that the value of M was the same in the two equations, and thought that the result arrived at must be incorrect. Dr. Sumpner did not doubt that if the coefficients could be considered constant, the formula was true whatever the leakage, but he did not consider that the action of transformers justified such an assumption. If the formula were true, it would also hold if there were eddy currents, as these would merely produce the effect of additional secondary coils. He had analyzed Blakesley's method by using a modification of the Hopkinson equations, and had shown that the power as estimated by Blakesley's formula had to be lessened by the fraction represented by the expression

$$X = \frac{\int x\dot{A}_p \int A_s dtdt}{e + \int A_pA_s dt},$$

where A_p and A_s are the instantaneous values of the primary and secondary currents, and x is such that $N_p = N_s(1 + x)$ where N_p and N_s are the fluxes of magnetism through the primary and secondary coils at the same instant; e is a negligibly small quantity compared with the rest of the denominator. In obtaining this factor no assumptions whatever had been made, and it was easy to see that if A_p and A_s could be assumed sine functions, and x a constant, the value of the factor X became x simply. In only one case did X reduce to zero, and that was when x was a sine function of the same period as A_p and A_s. He believed that in actual transformers x was approximately constant. Mr. Swinburne pointed out that the split dynamometer was merely a watt-meter, and stated that he had transformers which, owing to magnetic leakage, would indicate an efficiency of over 100 per cent. if tested by Blakesley's method. If this method gives an efficiency of 96 per cent., and leakage causes a drop of 2 per cent. in E.M.F., the real efficiency is only about 94 per cent. He thought that the assumption that the currents followed a sine law was equivalent to supposing that there was no loss in hysteresis. The President said that no one would be more glad than himself to find that Mr. Blakesley's method was accurate, but he could not agree with Prof. Perry that Dr. Hopkinson was wrong in abandoning the academical method of Maxwell. Prof. Perry replied to the various points raised in the discussion.—A paper on current and potential difference analogies in the methods of measuring power, by Prof. W. E. Ayrton and Dr. Sumpner, was postponed.

Royal Microscopical Society, May 20.—Dr. R. Braithwaite, President, in the chair.—The President said he regretted to have to announce the deaths of two of their Honorary Fellows, Dr. Carl von Naegeli, of Munich, and Prof. J. Leidy, of Philadelphia.—Mr. C. L. Curties exhibited a new form of Mayall's mechanical stage, recently manufactured by Zeiss, which gave upwards of an inch motion each way, and merely required to be clamped on the pillar of the microscope when wanted for use.—Mr. Watson exhibited and described a microscope which he had recently made specially to meet the wants of Dr. Van Heurck, of Antwerp. Mr. Mayall, after criticizing the design, for which, he understood, Dr. Van Heurck was responsible, concluded by expressing his regret that Dr. Van Heurck's specification should have resulted in the production of the microscope exhibited. Mr. E. M. Nelson and the Rev. Dr. Dallinger also criticized the instrument adversely.—Mr. Grenfell exhibited the photograph of a small organism, the nature of which he had been as yet unable to determine; some zoologists and botanists to whom he had shown it were unable to say whether it was vegetable or animal in its nature. He also wished to mention that in the Botanical Gardens, Regent's Park, there were considerable numbers of a free-swimming infusorian known as *Tintullus*. It was remarkable for its chitinous lorica.

Claparède mentioned its having been found at Berlin; but hitherto it had only seemed to have been found in sea-water.—Prof. Bell said they had received a communication from Mr. J. B. Rosseter describing the development of *Tænia lanceolata* from the duck, the cysticercoid form of which had not been previously known. He (Mr. Rosseter) had fed the ducks with some of the *Cypris* known to be infested with the parasite, and after some weeks opened the ducks and found the tapeworm mentioned. It was interesting to get the life-history of another tapeworm worked out.—Mr. E. M. Nelson read a note on the subject of lateral development in photography, and a paper on the use of monochromatic light in microscopy, and exhibited the model of a new and simple apparatus for obtaining the same by means of a glass prism. Mr. Nelson also described a new projection microscope fitted with a special condenser made of three flint lenses so as to embrace the whole cone of 82°. The novelty about it was the system of collecting the light, by which a beam of 4¼ inches was brought down to 1¼ inches, and by passing through two lenses placed in the water-trough, a beam of parallel rays of great intensity was obtained for use in projecting the image upon the screen. Afterwards Mr. Nelson gave an exhibition on the screen.

Geological Society, May 27.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read:—On the lower jaws of *Procoptodon*, by R. Lydekker. After reviewing Sir R. Owen's writings upon the large extinct Kangaroos for which he established the genus *Procoptodon* in 1874, the author describes two mandibular rami from the clay beds of Miall Creek in the neighbourhood of Bingera, N.S.W., which belong to this genus, and from their characters and a comparison of them with the lower jaws in the British Museum, he maintains that this part of the skull indicates two very distinct species of the genus, for which he retains the names *P. rapha*, Ow., and *P. goliah*, Ow., though it is possible that the types of those two species are really specifically identical, in which case the name *P pusio*, Ow., might have to be adopted for one of the species described.—On some recently exposed sections in the Glacial deposits at Hendon, by Henry Hicks, F.R.S. In this paper the author brings forward evidence obtained from sections exposed in gravel-pits and deep cuttings made for the purpose of laying down the main sewers, to show that Glacial deposits had been spread out to a much wider extent over the Hendon plateau than had hitherto been supposed, and that they had reached down the slopes to below the Ordnance-datum line of 200 feet. He further mentions that there is evidence to show that these deposits have extended in a south and south-west direction across the Brent and Silk Valleys, and now occur on most of the heights in the parishes of Kingsbury and Willesden. As the sands, gravels, and Boulder-clay which cover the Hendon plateau and the neighbouring heights are found to rest on an undulating floor of London Clay, and to follow the contours of the hills and valleys, the author considers that it is clear that the main physical features of this portion of North-west Middlesex were moulded at a very early stage in the Glacial period, and before the so-called Middle sands and gravels and overlying Upper Boulder-clay with Northern erratics were deposited. He believes that at this time there could have been no barrier of any importance to prevent these deposits from extending into the Thames Valley, and that the evidence clearly points to the conclusion that the implement-bearing deposits on the higher horizons in the Thames Valley should be classed as of contemporaneous age with the undoubted Glacial deposits at Hendon, Finchley, and on the slopes of the Brent Valley, which they so closely resemble. The author is therefore satisfied that man lived in the neighbourhood of the Thames Valley in the early part of the Glacial period; probably, he thinks, in pre-Glacial times. This paper led to a discussion, in which Mr. H. B. Woodward, Mr. J. A. Brown, Dr. Hinde, Mr. Monckton, and the author took part.

Entomological Society, June 3.—Mr. Frederick Du Cane-Godman, F.R.S., President, in the chair.—Mr. E. B. Poulton, F.R.S., exhibited living larvæ of *Endromis versicolora*, and commented on their habits.—Mr. W. F. H. Blandford called attention to the fact that the larvæ of *Liparis monacha* remained in small groups on the bark of the tree for about a week after emerging from the eggs, and that this fact was taken advantage of by the German foresters to destroy them. Also that he had himself verified the statement that uric acid can be detected in the Malpighian tubes of insects. Mr. McLachlan, F.R.S., agreed that the demonstration that the Malpighian tubes were of the nature of renal organs was now satisfactory.—Mr. C. J. Gahan exhibited two species of Coleoptera that he considered to possess a mimetic resemblance.—Mr. Tutt exhibited a hybrid between *Amphidasis prodromaria* and *A. betularia*, obtained by Dr. Chapman. Mr. Stainton, F.R.S., commented on the fact that the two insects appeared at different times; and Mr. Tutt stated that the *A. betularia* had been subjected to forcing, so as to cause it to emerge at the same time as *A. prodromaria*.—Mr. Tutt also exhibited forms of *Caradrina*, some of which he said were considered distinct on the Continent, though they were not recognized as such in this country, viz. *Caradrina taraxaci* (*blanda*), *C. superstes*, Tr., from Sligo, and *C. superstes*, H. S., considered as synonymous with *superstes*, Tr., but apparently more closely allied to *C. ambigua*.—Mr. Bristowe exhibited varieties of *Arctia menthastri*, some of which had been fed on mulberry and others on walnut; no difference was observed in the variation.—Mr. G. Elisha exhibited larvæ in their cases of *Coleophora vibicigerella* and *C. maritimella*.—Mr. A. G. Butler communicated a paper entitled "Additional Notes on the Synonymy of the Genera of Noctuid Moths."

Zoological Society, June 2.—Prof. W. H. Flower, C.B., F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of May 1891, calling special attention to a female Water-buck Antelope (*Cobus ellipsiprymnus*) from British East Africa, presented by Mr. George S. Mackenzie, and to three Blanford's Rats (*Mus blanfordi*) from Kadapa, Madras, received in exchange, new to the collection.—Mr. Sclater made some remarks on the animals which he had noticed during a recent visit to the Zoological Gardens of Paris, Ghent, Antwerp, Rotterdam, Amsterdam, and the Hague.—Prof. Newton, F.R.S., exhibited (on behalf of Prof. Stirling, of the University of Adelaide), a drawing, being the first received in Europe, representing the remarkable new Australian Mammal lately described by Prof. Stirling as *Notoryctes typhlops*, which was stated to be the Mole-type of the order Marsupialia.—The Secretary exhibited (on behalf of Mr. F. E. Blaauw) specimens of the Long-tailed Tit, shot in Holland, and sent to this country for the purpose of ascertaining whether they belonged to the British form (*Acredula rosea*) or the white-headed Continental form (*A. caudata*).—Mr. F. Finn exhibited a hybrid Duck bred in the Society's Gardens, believed to be bred between a male Chilian Pintail (*Dafila spinicauda*) and a female Summer-Duck (*Æx sponsa*).—A communication was read from Dr. O. F. von Moellendorff containing a revised list of the Land and Freshwater Shells of Perak, with descriptions of some new species.—A communication was read from Dr. G. E. Dobson, F.R.S., containing a sketch of the derivation and distribution of the Mammals of the order Insectivora found in the New World.—Mr. G. A. Boulenger read a report on Reptiles, Batrachians, and Fishes of which specimens had been collected for the West Indian Exploration Committee in some of the Lesser Antilles, and deposited in the British Museum.—A communication was read from Mr. Hamilton H. Druce containing an account of the Butterflies of the family Lycænidæ obtained by Mr. C. M. Woodford in the Solomon Islands.

CAMBRIDGE.

Philosophical Society, May 18.—Prof. Liveing, Vice-President, in the chair.—The following communications were made:—On parasitic Mollusca, by Mr. A. H. Cooke.—Mr. W. Bateson exhibited and explained models of double supernumerary appendages in insects; and also a mechanical method of demonstrating the system upon which the symmetry of such appendages is usually arranged.—On the nature of the excretory processes in marine Polyzoa, by Mr. S. F. Harmer. This communication was the result of an occupation of a University table at the Zoological Station at Naples during the Easter Vacation of 1891. Observations were made on the manner in which various artificial pigments were excreted in *Bugula* and in *Flustra*, on the lines adopted by Kowalevsky (*Biolog. Centralblatt*, ix., 1889–1890, pp. 33, &c.) for other invertebrates. The general result of the experiments was to show that excretion is not performed by organs comparable with nephridia, but that this process is carried on by free mesoderm cells, and to some extent by the connective tissue and by the walls of the alimentary canal. Evidence was obtained to show that the periodic loss of the alimentary canals leading to the formation of the "brown bodies" may be regarded as, to some extent, an excretory process.

PARIS.

Academy of Sciences, June 1.—M. Duchartre in the chair.—Calorimetric researches on humic acid derived from sugar, by MM. Berthelot and André. The experiments show that humic acid is a polybasic acid which may be caused to unite with three equivalents of potash to form insoluble salts: one salt described is monobasic, stable, and formed with the evolution of 18 calories, an amount comparable with that evolved when alkaline salts are formed by the action of strong acids. Many other properties of this acid are given.—Analysis of the light diffused by the sky, by M. A. Crova. The observations recorded extend from December 1889 to the same month in 1890. From the results it appears that the blue colour of the sky is most intense in the months of December, January, March, and September; and shows minima in July, August, and November. Roughly, the blue colour is deepest in the winter, and palest in summer; spring and autumn give sensibly the same results. A comparison of the intensities at different hours of the day indicates that a maximum blue coloration occurs in the morning and a minimum at the hottest hour in the day.—On Abelian equations, by M. A. Pellet.—On a new method of determining the vertical motion of aërostats, by M. André Duboin. The methods usually employed by balloonists to determine their state of vertical motion are by means of a barometer, or by throwing out light bits of paper and observing whether they ascend or descend relatively to the balloon. The author has devised an apparatus having the same object, on the principal of Kretz's differential manometer, and claims for it a sensibility 150 times greater than the ordinary mercurial barometer.—New models of copper oxide batteries, by M. F. de Lalande. A 35 per cent. solution of potash is the liquid employed. In it dips a conglomerate of copper oxide and sand covered with a thin porous layer of metallic copper, and one or two zinc plates. A cell thus constituted is shown to be practically constant for three or four days, and is said to work for years without getting out of order.—Determination of molecular weights at the critical point, by M. Philippe A. Guye. Using Van der Waals's formula, the author deduces $d = 1146\frac{\delta\theta}{\pi(1670 + \theta)}$, where d is the critical density with respect to air, δ the critical density with respect to water—that is, the weight of the substance in grams occupying a volume of one cubic centimetre at the critical state—θ the absolute temperature, and π the pressure in atmospheres. It is then shown that the values of d obtained by means of this empirical formula are equal to the molecular weights of the substances investigated divided by 28·87.—Research on the separation of metals from platinum, and in particular of palladium and rhodium in the presence of common metals, by MM. A. Joly and E. Leidié. The platinum or palladium are converted into soluble nitrites by the addition of potassium nitrite, and are thus separated from other metals.—On the specific heats of some solutions, by M. W. Timofeiew. Alcoholic solutions of bichloride of mercury and cadmium iodide were used. It is shown that the difference between the molecular specific heat of the solution and solvent is sensibly equal in the case of both salts, the mean value being 52. Taking this value to represent the molecular specific heat of the salt in solution and the specific heats of the alcohols employed to be expressed by the formulæ,

$$C^{\text{methyl}} = 0{\cdot}56755 + 0{\cdot}001633t\text{ ; }C^{\text{ethyl}} = 0{\cdot}53574 + 0{\cdot}002132t\text{ ;}$$

it is shown that the observed and calculated specific heats of the solutions are very nearly the same in each case.—On the oxidation products of uric acid, by M. C. Matignon. The heats of formation and combustion of the principal derivatives of uric acid are considered.—On the employment of ammonium selenite for the identification of alkaloids, by M. A. J. Ferreira da Silva. The use of ammonium sulphoselenite for the detection of morphine and codeine was suggested by M. Lafon in 1885. The author shows how the method can be extended to other alkaloids.—On the development of the liver of Nudibranchiates, by M. H. Fischer. The research shows that the liver of Nudibranchiates is formed to a great extent by the left hepatic lobe of the embryo, and that the hepatic organs in two very different groups of Mollusca, the Lamellibranchiates and Nudibranchiates, are homologous productions.—The *Isaria*, a parasite of the larva of the cockchafer, by M. Alfred Giard.—The genera of the group of Clusia, and in particular the genus *Tovomita*, by M. J. Vesque.—On some supporting elements of the leaves of Dicotyledons, by M. E. Pée-Laby.—Diffusion of three distinct forms of titanium oxide in the Cretaceous strata of Northern France, by M. L. Cayeux. A microscopic examination of the residue after treating chalk with an acid leads the author to believe he has recognized the three crystalline forms of titanium oxide—rutile, anatase, and brookite.—The lichens on mulberry-trees, and their influence on silk culture, by M. G. Hallauer.—On the employment of carbon bisulphide against aërial parasites, by M. H. Quantin.

AMSTERDAM.

Royal Academy of Sciences, May 30.—Prof. Van de Sande Bakhuyzen in the chair.—Dr. Bakhuis Roozeboom treated of the solubility of mixed crystals of isomorphous substances. Admitting the absolute homogeneity of such crystals, according to the recent researches of Retgers, it is possible to deduce, by thermodynamical reasoning, that, when p and T are taken as constants, equilibrium is obtained when to a proportion a of the components in the mixed crystals, correspond two definite values C_1 and C_2 for the concentrations of the components in the aqueous solution. From the analogy between the said equilibrium and that between a liquid and a gaseous phase, composed of two substances, are further deduced some general conclusions as to the behaviour of solutions of two isomorphous salts when they are evaporated. This is done both for the case in which the mixed crystals form a continuate series, and that in which they present a hiatus. The latter was found in studying the solubility of $KClO_3$ and $TlClO_3$. Solid mixtures were obtained at 10° and 1 atm. from 0–36·3 and from 97·9–100 per cent. $KClO_3$. In the evaporation of any one solution, it tends to a composition, which is necessary to deposit the two kinds of crystals between which the hiatus exists.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Bacteria and their Products: Dr. Sims Woodhead (Scott).—Einleitung in die Theoretische Physik; Zweite umg. und verm. auflage; V. von Lang (Braunschweig, Viewig).—A Manual of Forestry, vol. ii.: Dr. W. Schlich (Bradbury).—The British Noctuæ and their Varieties, vol. i.: J. W. Tutt (Sonnenschein).—Practical Work in Organic Chemistry: P. W. Streatfeild (Spon).—Katalog der Bibliothek der Deutschen Seewarte zu Hamburg (Hamburg).—Catalogue of the Fossil Birds in the British Museum (Natural History): R. Lydekker (London).—The Solar Parallax and its Related Constants: W. Harkness (Washington).—Favourite Foreign Birds: W. T. Greene (Gill).—Anthropogeographie: Dr. F. Ratzel (Stuttgart, Engelhorn).—Notes on Building Construction, Part 2, new edition (Longmans).—Poachers and Poaching: J. Watson (Hall).—Modern Cremation, 2nd edition: Sir Henry Thompson (Paul).—Nature and Treatment of Diphtheria, 3rd edition: R. W. Parker (Lewis).—Riddles of the Sphinx: A Troglodyte (Sonnenschein).—Housing of the Poor: F. H. Millington (Cassell).—British Cage-Birds, Part 14: R. L. Wallace (Gill).—A Manual of Orchidaceous Plants, Part 7 (Veitch).—Tasmanian Official Record, 1891 (Hobart).—The Birds of Manitoba: E. E. Thompson (Washington).—Scientific Results of the Second Yarkand Mission; Coleoptera (Calcutta).—Smithsonian Report, 1889 (Washington).—Internationales Archiv für Ethnographie, Band 4, Heft 3 (Paul).—The Asclepiad, No. 30, vol. viii.: B. W. Richardson (Longmans).—Photographic Reporter, June (Hazell).

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THURSDAY, JUNE 18, 1891.

EGYPTIAN IRRIGATION.

THE "Note" on the above subject by Sir Colin Moncrieff, which we publish this week, will prove pleasant reading to all who have the welfare of Egypt at heart. To those who have known that country intimately in the past, the brief record of progress in irrigation since the British occupation will recall the horrors of the *corvée*, and the torturing of the wretched peasantry by tyrannical farmers of the taxes; and to engineers the record will imply, not only that all those atrocities have been abolished, but further that some of the most difficult and important engineering problems of recent times have been successfully solved by Sir Colin Moncrieff and the able staff under his control. Nothing is exaggerated, but we have in the "Note" a plain and modest statement of the quiet and unostentatious execution of works the mere discussion of the difficulties of which had occupied the time of the predecessors of Sir Colin for the previous quarter of a century without anything useful resulting.

It will be only necessary to refer to one or two matters to establish this proposition. In paragraph 10, Sir Colin announces that the Barrage has been completed, and placed in a condition to fulfil its original purpose, for the sum of about £460,000. Contrast this with the published statement of M. Linant, a former engineer-in-chief of the Egyptian Government, who, in 1872, expressed a doubt whether it would not cost more to repair the existing Barrage than to build an entirely new one, and further says: "If, at the time when the Barrage was commenced, steam-engines had been what they are to-day, one would certainly have advised Mehemet Ali to abandon his project of a Barrage for the establishment of pumping-machinery." Even at that time, M. Linant thought it was not too late to consider whether it would not be better to abandon the idea of repairing the Barrage; and to assist in the determination of the question he submitted an estimate of the cost of pumping, amounting to £465,000 per annum, which, he thought, the cultivators could well afford to pay.

We have already stated that Sir Colin Moncrieff has effected the same result by a single expenditure of £460,000 instead of by an annually recurring one of £465,000. By means of the completed Barrage the whole summer supply of the Nile is thrown on to the lands, so obviously there is no work for pumps, and the vast cost of the same is saved to the country. Although national feeling runs high in France, we cannot but think that French engineers will rejoice that the world-renowned Barrage of the Nile, the design of which by M. Mongel was approved of by the Council of the Ponts et Chaussées in 1842, and carried into execution during the ensuing ten years, has at last, after thirty years' practical inutility and failure, been finally completed by their worthy compeers and successors in Egypt—the British engineers—whose experience, gained in the great irrigation works of our Indian Empire, has been as zealously utilized in securing the success of a great French work as it would have been in carrying out a new one of their own design.

One other matter in Sir Colin Moncrieff's "Note" may be referred to—namely, the drainage recently effected No doubt, the fact enforced upon Indian engineers by numberless experiences—that high-level perennial irrigation must be accompanied by drainage works if soil and people are to remain in a healthy condition—was not well known to the French projectors of summer irrigation works in Egypt; and, as a consequence, whereas magnificent canals carrying 5000 cubic feet and upwards per second were constructed, no corresponding means were provided for draining the superfluous and often saline water off the lands. Sir Colin tells us that the mileage of the drains at present is not less than 1500. When we consider that, in addition to these vast works of improved irrigation and drainage, a steady reclamation of marsh-land has been going on, we have reason as a nation to be proud of the good work which our countrymen have carried on in Egypt; as, whatever may happen in the future, the fact of the British occupation will, from its successful applications of science, be indelibly stamped upon the face of the country for all time, and its memory will for other reasons live honourably in the traditions of the emancipated and much-enduring fellaheen.

PHYSIOLOGICAL PSYCHOLOGY.

Leitfaden der physiologischen Psychologie. In 14 Vorlesungen. Von Dr. Th. Ziehen, Docent in Jena. (Jena: Gustav Fischer, 1891.)

THIS little volume will be welcome to a good many students of psychology, both in Germany and beyond. Anyone who has had to look up the newer researches in experimental psychology in Germany knows the serious difficulty of gaining easy access to them. They are scattered over a whole heterogeneous mass of serial and other publications. Now we have to look into an avowedly psychological journal or brochure, but more frequently still into physiological works, and not infrequently into journals for psychiatry. The explanation is obvious. Psychology, in passing into the objective and experimental phase, is broadening its base to an almost perplexing extent, and is encroaching more especially on the domain of physiology. Hence the need of a volume like the present work, which aims at giving the beginner a conspectus of the psychological field. We want such a book badly in English, the only available one, that of Prof. Ladd, being at once incomplete on certain sides, and in part too metaphysical. Meantime we can recommend Dr. Ziehen's "Vorlesungen" as exceedingly well adapted to give the student a clear idea of the scope and the methods of the new science of physiological psychology.

Dr. Ziehen marks off physiological from what he calls transcendental psychology by the differentia that it deals with psychical processes as attached to cerebral functions. Psychophysic, the branch of investigation opened up by Weber and Fechner, he includes under physiological psychology as that part which aims at exact measurement. This seems to be a satisfactory way of mapping out the ground. The standpoint of the physiological psychologist is indicated in the assumption that every psychical process must be thought of as having a concomitant physiological process. This, too, though it would exclude such a "physiological psychology" as that of Ladd, seems a reasonable way of viewing the matter. Further, the author proceeds to set forth the typical form of psycho-physical process as reflex, and he considers that every known development of the psychical phase must be capable of being viewed as an incident in such a reflex process. Here Dr. Ziehen meets the real difficulty in psychology, and, as we see, meets it boldly. From the physiological point of view we are bound to take the reflex as our starting-point, and to view the most intricate plexus of cerebral processes as merely an expansion of the intermediate central stage of this reflex. But can the same mode of treatment be applied to the intricate interweavings which constitute our mental life? As mere events in time, synchronizing with neural events, they appear to be susceptible of being thus regarded, and this, as the author rightly contends, is precisely the way in which the physiological psychologist has to conceive of psychical phenomena.

Starting, then, with the reflex, of which the writer gives an admirable account by way of introduction, he proceeds to deal with the psychical process in its three successive stages, viz. sensation, answering to the afferent or sensory section of the nervous process; ideation, or, as he puts it, association, answering to the expanded form of the central section; and action, answering to the efferent or motor section. The account of sensation is fairly full, and up to date. Perhaps the treatment of the relation of stimulus to sensation (Weber's law) is proportionately too long. The discussion of the interpretation of the facts is original and interesting. The author does not, like most recent physiologists, view the logarithmic ratio of stimulus to sensation as a purely physiological relation, due to what Mr. James has recently called the friction of the nerve-machine, but connects it with a more general psycho-physical law formulated by Hering, viz. "that the purity, distinctness, or clearness of a sensation or idea depends on the relation in which the weight of the same, *i.e.* the magnitude of the corresponding psycho-physical process, stands to the collective weight of all simultaneously present sensations and ideas, *i.e.* to the sum of the magnitudes of all the corresponding psycho-physical processes." At the same time the author is far from clear when he speaks of the conscious comparison of intensities as an "associative activity." This is an example of a tendency among the younger physiological psychologists to force psychical processes into a physiological framework. Comparison cannot, as Münsterberg's ingenious but futile attempts plainly show, be regarded as *merely* an associative process, though of course it depends on association, and in this way can be correlated with a nervous process. It must, however, be said in justice to Dr. Ziehen, that he is commendably free from the common tendency of physiologists to ignore psychical distinctions. Thus it is quite refreshing to find a physiologist contending that black and grey are positive sensations, having each its distinctive quality, like white or blue. It may be added that special interest is given to the exposition of sensation, as of the other psychical phenomena, by the frequent bringing in of the biological point of view, and the suggestion how, by the process of natural selection, particular psycho-physical arrangements have been brought about and rendered permanent.

The account of sensation is supplemented by a chapter on the affective or emotional tone of sensations, *i.e.* their pleasurable or painful aspect. Here, again, we have frequent references to the Darwinian theory, as when it is suggested, *à propos* of the fact that extreme intensities of skin-sensation, pressure, heat and cold, lose their distinctive sensational quality, and become purely affective phenomena or pains, that this arrangement has come about owing to the circumstance that in the evolution of the zoological series "intensive mechanical and caloric stimuli constitute the earliest, the most frequent, the most direct, and the greatest danger to the animal organism" (p. 85). The author refers the whole of the difference in affective tone among colours, and among combinations of musical sound, to association. This seems to be going too far. The contrast between the exhilarating effect of the warm colours, and the quiet effect of those at the violet end of the spectrum, seems to be connected in part with the difference in the underlying nervous processes; and this is certainly true, as Helmholtz has shown, with respect to the emotional aspect of certain accords, *e.g.* the major and minor triads.

Coming now to the account of the idea (image and concept), we note that Dr. Ziehen differs from the majority of contemporary psychologists in assigning a separate cortical element to the sensation and to the idea. These different cells (the author, in spite of Lewes's attacks on the cell-superstition, talks of the individual cell as the seat of a sensation) are closely connected, and in this way the after-effect of sensations in memory, as also the reflex effects of ideas in exciting sensations, as in hallucinations, are accounted for. The writer elaborates his peculiar anatomical hypothesis in an ingenious way. He seems to admit, however, that it is a pure hypothesis, for the facts of "mental blindness" referred to are not apparently put forward as a proof; and it may be added that rightly viewed they do not seem even to suggest the hypothesis. One may add that it appears to lack the only possible justification of such a hypothesis, viz. that it simplifies the interpretation of the facts. The other supposition, that the sensation and the idea involve the same group of central elements (not the same single cell), is more reasonable in itself, and seems to offer a readier explanation of most of the phenomena.

The account of the psycho-physical process in association is less clear and instructive than most of the exposition. The author follows Münsterberg a good way at least—in reducing all association to one form, viz. contiguous, and more particularly simultaneous, association. But the diagrammatic representation of the processes strikes one as needlessly complicated by the hypothesis of separate ideational nerve-cells. Much better is the account at the close of this lecture of the way in which the different psycho-physical factors co-operate and modify one another in the actual concrete processes of reproduction. Dr. Ziehen is particularly happy in explaining the great variability of the sequences of our ideas from moment to moment. The account of the ideational stage is completed by a discussion of the relation of association to judgment and reasoning—which is a little hasty, and ignores some of the main difficulties of the subject, of attention and the voluntary control of the thought-process, and of the abnormal modifications of ideation in mental disease, sleep, and hypnosis.

The unfolding of the third and final stage, voluntary action, with which the volume concludes, offers little that is noteworthy. The author adopts the new and growingly fashionable view that all our active consciousness, sense of muscular effort, and so forth, is the result of afferent nerve processes, and he proceeds, much in the manner of Münsterberg, to resolve all volitional processes into complexes of sensations and ideas, more particularly ideas of movement. This seems to lead logically to the denial of any distinctive active or volitional psychosis answering to ideational or emotional psychosis; and Dr. Ziehen is not afraid to express this denial, and fortifies his position by the debatable statement that psychiatry, while acknowledging a special variety of intellectual and of emotional disturbance, knows no such thing as a distinct volitionary disturbance. It is to be added that the exposition concludes with a particularly good discussion of the final results of psycho-physical research. The author here shows himself a genuine psychologist, and while insisting upon the invariable concomitance of a physiological factor in psychical phenomena, is so far from regarding the psychical as a non-essential and negligible accompaniment of the material process, that he closes in a quite Kantian strain by reminding us that the psychical chain is that which is known primarily and immediately, and which as such must always possess more reality for us. J. S.

ACHIEVEMENTS IN ENGINEERING.

Achievements in Engineering. By L. F. Vernon-Harcourt, M.Inst.C.E. (London: Seeley and Co., Limited, 1891.)

THE object of this book is to describe some of the principal engineering works carried out during the last fifty years at home and abroad. The author has avoided technical phraseology to a great extent, thus making a very interesting subject as clear as may be to the general reader. Much subject-matter has been gleaned from many sources, and these are amply enumerated in the preface.

The London Metropolitan Railways and the New York elevated railways are described in chapter i. The growth of the Metropolitan system is very interesting, and is traced from the opening of the first section from Paddington to Farringdon Street in 1863 to the completion of the "Inner Circle" from the Mansion House to Aldgate in 1884. The author states that when the Metropolitan Railway was first designed, it was intended that the traffic should be worked by smokeless, hot-water locomotives not burning fuel, as it was supposed that the trains would be small, and that "foreign" locomotives would not travel over the line to any important extent. This, however, was not carried out, and locomotives of the ordinary type were adopted. The ventilation therefore proved defective, and even to this day improvement is greatly needed in many sections. The bad atmosphere is, of course, due to the locomotives in use, and the emission of steam considerably adds to the nuisance. Locomotive engineering is surely able to cope with this trouble. The dead weight of the trains might be considerably reduced with advantage, and the engines designed with ample condensing arrangements, even if the latter had to be attached to the engine as a separate vehicle. The boilers should, of course, be large enough to steam well with the ordinary blower, so that all the exhaust might be condensed.

The Metropolitan Railway represents an engineering achievement novel in many respects and made under circumstances of peculiar difficulty. On the other hand, the New York elevated railways illustrate how the American engineers solved a similar problem in a very different manner. Owing to the cost of "burrowing underground," as the author aptly describes it, they rejected the underground scheme, and for the same reason a railway on an arched viaduct was also considered undesirable. The railways have been carried along the streets, raised above the street traffic on girders resting upon wrought iron lattice columns standing at convenient places on the line of the curb of the pavements. An illustration is given representing a street in New York and the elevated railways running on each side. No payment has been made for placing these columns along the streets, and no compensation has been paid for damages to residential property fronting the railways. The author estimates the depreciation in value, due to the presence of the railway, as not less than 50 per cent. The cost per mile will therefore be considerably less than in the case of the London Metropolitan Railway, in which case all these items were heavily paid for. The London railway cost about £575,000 per mile, whereas the New York elevated railways only cost about £81,000 per mile.

Chapter ii. describes railways across the Alps, the Rocky Mountains, and the Andes. On p. 30 we find an interesting diagram representing the gradients and altitudes of the heavy portions of these lines, from which it is evident that the lines in North and South America are at higher elevations and are more subject to snow than the highest of the Alpine railways, and more severe gradients are to be found. Take, for instance, the heavy gradient on the Mexican Railway, rising 6400 feet in 54 miles, the maximum gradient being 1 in 25. This portion of the line is worked by Fairlie engines, which the author attempts to describe on p. 56.

The author in describing the Festiniog Railway says that the traffic is worked up the long incline by "duplex bogie engines, introduced in 1869, having two engines, united by a tender common to the two, and hinged at the centre." He goes on to say that these are called Fairlie engines, after the name of their designer. The Fairlie engines as used on the Mexican Railway certainly do not agree with this description, nor does this description agree with the usually accepted type of engine known as the "Fairlie." The Fairlie engine consists of a special type of boiler carried on bogies, one at each end. These bogies have either four or six wheels, as the case may be; each bogie is fitted with steam cylinders and gear complete, and all the wheels are coupled. The boiler has a smoke-box at each end, and is fitted with fire-boxes in the centre, being fired from the side. The steam pipes from the boiler to the "steam" bogies are flexible, to allow the bogies to take the curves. The water is carried in side tanks, and the fuel on the top of the boiler and at the side. The author will observe that there is no central pivot and no tender; the engine is a tank engine; and that the whole of its weight is good for adhesion. The Fairlie engines at work on the Mexican Railway weigh in order about 92 tons. The total wheel base is 32 feet 5 inches, and the rigid wheel base of the bogie is 8 feet 3 inches.

Chapter iii. includes narrow gauge railways, as well as the Fell, Rigi, Pilatus, and Abt mountain railways. The use of a narrow gauge railway in place of the standard gauge is due to questions of cost of construction by diminishing the width of the line, and also enabling sharper curves to be adopted. Narrow gauge railways now in use were years ago of ample capacity for the traffic then available, but are now a continual source of trouble where the traffic has increased beyond their capacity. In some cases, where an increase of gauge is impossible owing to the cost, the rolling stock has to be designed to suit the abnormal requirements, and the locomotives recently designed have to be made to suit the conditions, and are working under adverse conditions from a locomotive engineer's point of view. The cost of a break of gauge is a serious matter, involving as it does the transshipment of passengers and goods, as well as two classes of rolling stock. In India, for instance, the metre gauge has given place to the broad gauge of 5 feet 6 inches in many cases, in order to obtain through communication without break of gauge. The author gives an excellent description of the various mountain railways named, and they are without doubt monuments of engineering daring and skill.

In chapter iv. an excellent description is given of the piercing of the Alps. To the rivalry of European Powers, each anxious to command a route, are due the several Alpine tunnels; from the design and execution of the Mont Cenis tunnel to the more recent schemes west of the St. Gothard. Had the author told us a little more about the difficulties encountered, he would have added considerably to the interest.

Tunnels under the Alps naturally give place to subaqueous tunnels in the sequence of subject-matter in the volume. The Mersey and Severn tunnels are described, and the tremendous difficulties encountered in the execution of the latter undertaking are pointed out. We also find a description of several subaqueous tunnels in the States, including the Sarnia tunnel recently opened under the St. Clair river, to connect the Grand Trunk Railway of Canada at Sarnia with the United States Railways at Port Huron. The chapter closes with an account of the proposed Channel Tunnel.

The progress and principles of modern bridge construction are treated in chapter vi. This gives a good account of the great advance made during the last fifty years in this important branch of engineering. Wrought-iron gradually superseded cast-iron in bridge construction, and steel has again superseded it. The manufacture of steel has now reached a stage in which there are no uncertainties in its quality. The earliest instance of the adoption of steel for a bridge is the St. Louis Bridge, over the Mississippi, constructed in 1867–74, and the most recent example is, of course, the cantilever bridge, with two spans of 1700 feet, over the Firth of Forth. The author gives the great Indian bridge over the Rori branch of the River Indus, at Sukkur, very little notice, and does scant justice to this "achievement in engineering," certainly a monument to its designer. Designed by Sir Alexander M. Rendel, K.C.I.E., M.Inst.C.E., and built by, and erected on the works of Messrs. Westwood, Baillie, and Co, of London, this bridge was taken to pieces and shipped to India, where it was re-erected. The chapter closes with an account of the proposed bridge over the Channel.

Submarine mining and blasting are treated in the chapter that follows. This chapter is interesting mainly owing to a detailed description of the operations for improving the entrance to New York Harbour by the removal of the obstructions at Hell Gate and Hallett's reef. With reference to the explosion at the latter site, it is interesting to observe that the earth-wave produced was carefully recorded at various places, and the rate of transmission of the shock was found to be more rapid and more uniform when the shock passed northwards through rock, than when it passed through drift in an easterly direction. In travelling through drift, it reached Goat Island, a distance of 145 miles, in 59 seconds, and Harvard College Observatory, 182⅜ miles, in 3 minutes 40 seconds; and in travelling through rock, it reached West Point, 42½ miles distant, in 11 seconds, and Litchfield Observatory, 174⅛ miles away, in 45½ seconds.

Chapters ix. to xv. deal with that branch of engineering which may be roughly included under the title of "Harbours and Docks." In a previous work by the author, bearing this title, and reviewed in these columns, this subject was amply dealt with, and it will now be sufficient to state that the present chapters are well up to the standard of excellence of his previous work. We find an interesting description of the Manchester Ship Canal works in these chapters—a work rapidly nearing completion, and one which, if successful, will be the forerunner of many similar works in this country. An illustration is given, showing the progress of the works forming the Eastham Locks, viewed from the Eastham end. This illustration gives a very good idea of the magnitude of the undertaking. Another Manchester undertaking occupies considerable space in this work, viz. the Manchester waterworks, and more particularly the Thirlmere scheme. The author tells us that the eventual maximum daily supply of 50 million gallons of water will be conveyed to Manchester by an aqueduct, or conduit, about 100 miles long. Another similar undertaking is also discussed; in the Liverpool Vyrnwy scheme we find how engineers have solved the difficulty of getting a pure water supply for that city.

The volume concludes with an account of the Eddystone Lighthouse and the Eiffel Tower.

The frontispiece is a portrait of Robert Stephenson, a very appropriate one for such a work. His name will ever be associated with the development of railways, as the author remarks; and he might also have pointed out that the railway has been in many cases the reason for many "achievements in engineering" being called into existence.

Taken as a whole, this work is a very interesting one. It is well written, and the author may be congratulated on having succeeded in his endeavour to describe briefly some of the principal engineering works carried out, at home and abroad, within the last fifty years. The book is well printed, and the illustrations are excellent, although there might perhaps have been more of them, considering that the general reader has to be provided for. N. J. L.

GEOLOGICAL EXCURSIONS.

Geologists' Association: a Record of Excursions made between 1860 *and* 1890. Edited by Thomas Vincent Holmes, F.G.S., and C. Davies Sherborn, F.G.S. (London: E. Stanford, 1891.)

THE Geologists' Association began its useful career of work more than thirty years since. It has stimulated—more, perhaps, than any other body—a real interest in geology among those who live in and about London, because it has enabled students, still near the outset of their work, not only to meet for mutual help and encouragement, but also to be aided by those of repute in science. Of its meetings, not the least pleasant and useful are the excursions. At first these were made generally once a week, so long as weather permitted, and they occupied a Saturday afternoon or at most a single day. Then an occasional journey of longer duration was attempted; now it is usual to undertake excursions, lasting two or three days, at Easter and Whitsuntide, and one of a week or more during the summer holidays. Before each excursion a flysheet is issued to the members with a brief description of the geology of the locality, illustrated by diagrams and containing references to books and papers. Afterwards, a report of the excursion is inserted in the Proceedings of the Association. It was a happy thought to collect together in one volume these scattered notices, for they give succinct descriptions of almost all the localities of geological interest readily reached from London, so grouped as to be conveniently accessible. Thus the student, instead of having to compile for himself, from books or maps, a plan of campaign, whether for an afternoon or for a longer time, finds everything arranged ready to his hand, and is directed to the sections best worth visiting. These diagrams and reports possess a further value, that they frequently record sections which can be no longer examined, because they now either are overgrown by vegetation, or have been removed in quarrying. The work therefore is a geological guide-book of an exceptional and a very convenient character to a large district around London, and to several other localities of special interest in England.

The plan which has been followed in compiling the volume is stated in the preface. The excursions are grouped, as far as possible, within county boundaries; where more than one visit has been paid to any place, the editors have "either suppressed the shorter, and retained the fuller, or given from each account that which is not to be found elsewhere." The reports have been condensed by the excision of matters of general or merely temporary interest, and although references are made to all excursions up to the year 1890, no reports are given of later dates than 1884, because since 1885 it has been customary to print all these in the November number of the Proceedings, so that they can be easily consulted.

The thanks of the Association—indeed of a wider circle of geologists—are due to the editors for the pains which they have taken in discharging a very laborious duty. It seems almost ungracious to criticize, and to do it effectively would require encyclopædic knowledge; but we think that, though it may have been "impossible to send each report to the original reporter for revision," it would have been prudent to submit it to someone with a special knowledge of each district. These reports occasionally contain *obiter dicta*, or the crude speculations of members who are better acquainted with their own locality than with the principles of the science. Hence obsolete notions are preserved like flies in amber: these may perplex, but they cannot help the beginner. By way of testing the results of the editors' method, we have examined the reports of two or three districts with which we are specially familiar. The statement on p. 203 about the section at Roswell Pit, near Ely, is misleading. The natural interpretation of its words would be that the Kimeridge clay formed a part of the great erratic. This, in reality, consists of Cretaceous rocks, the Jurassic clay being *in situ*. On p. 216, the sentence "at the base, as at the top of the Gault," should have been "below the base, as above the top." Again, the clay beneath the neighbouring Upware limestone, now admitted to be Coral rag, cannot well be Ampthill clay, and we are not aware of any evidence in favour of this view. Again, the account of Charnwood Forest needs correction. At p. 463 a statement is quoted, which was published without due authority, and has been recalled by the author. On pp. 465 and 466 the suggestion that the Charnwood Forest rocks "ought to be called Laurentian" should have been cancelled. It was groundless, even as Laurentian was defined in 1875: it is absurd now. All reference to the "Archæan Petrology" of Prof. Ansted might well have been omitted. On p. 472, Peldar Tor is twice misprinted Peddar Tor. We know of no ground for the statement, on p. 473, that "the quartz [in the rocks of this neighbourhood] appears to be of subsequent formation." Doubtless similar defects could be pointed out by others; indeed, our own list is not quite exhausted, but we have no desire to carp at a book on which so much labour has been bestowed, and prefer to welcome it as a valuable addition to British geology, which will be indispensable to all students who live in the neighbourhood of the metropolis. T. G. B.

OUR BOOK SHELF.

Across East African Glaciers: An Account of the First Ascent of Kilimanjaro. By Dr. Hans Meyer. Translated from the German by E. H. S. Calder. (London: George Philip and Son, 1891.)

LONG before he thought of exploring any part of Africa, Dr. Meyer was an experienced and enthusiastic traveller. The idea of undertaking explorations in "the Dark Continent" was suggested to him by the fact that while the German colonial possessions in the west of Africa had been thoroughly investigated under Government supervision, and at the Government expense, those in the East had been left to the more limited resources of commercial companies. It occurred to Dr. Meyer that he might do good service to his countrymen by devoting himself to the task which the German Government seemed so unwilling to undertake. Accordingly, in 1886, he began to make preparations for the accomplishment of his plan and since that time he has organized no fewer than three important expeditions, in the third of which he succeeded in reaching the top of Kilimanjaro. It is this third expedition of which an account is given in the present work. The broad results of the journey were soon made known; but of course it is only from the explorer's full narrative that an adequate idea can be formed of the interest and importance of his achievements. The mountain mass of Kilimanjaro towers up to a height of nearly 20,000 feet, and Dr. Meyer describes well the feelings with which he saw it after his arduous march across the steppes. "It was a picture," he says, "full of contrasts—here the swelling heat of the equator, the naked negro, and the palm-trees of Taveta—yonder, arctic snow and ice, and an atmosphere of god-like repose, where once was the angry turmoil of a fiery volcano." The story of the ascent is told most vividly, and there are few readers who will not sympathize with the delight with which he speaks of the moment when he set foot on the culminating peak. Although the record of his experiences at Kilimanjaro forms the centre of the book, he has much to say about what he saw both on his way to the mountain and on his way back; and in appendices various writers present classifications of his collections, and the conclusions at which they have arrived in working out his astronomical and meteorological data. The book is admirably translated, and its value is greatly increased by illustrations and maps.

Chemistry in Space. From Prof. T. H. van 't Hoff's "Dix Années dans l'Histoire d'une Théorie." Translated and Edited by J. E. Marsh, B.A. (Oxford: Clarendon Press, 1891.)

WE have already reviewed the monograph of which this is a translation (NATURE, vol. xxxvii. p. 121), and need not therefore, at present, say anything of the subject with which it deals. The translator has done his work carefully, and "the invaluable assistance and advice" of the author have enabled him to make his rendering "a considerable extension of the French edition." Mr. Marsh advises those to whom the question is new to leave the first chapter till the end, as it contains a translation of the earliest memoirs on the subject, and the ideas are incompletely developed, obscure, and sometimes erroneous.

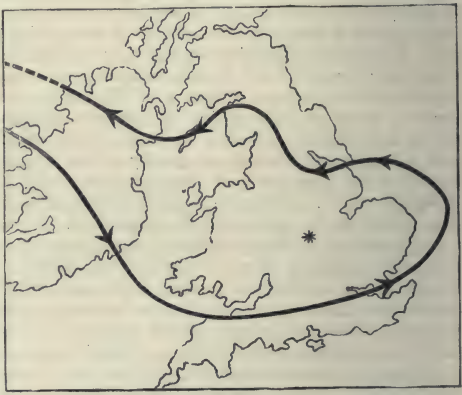
LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Erratic Track of a Barometric Depression.

THE singular course of the cyclonic system which has, during the week terminating on May 29, circulated round and across the British Isles, deserves more attention than can be thus early given to it. I wish here, with your permission, first, to describe the path of its centre as correctly as can be done with the data at present in my hands, mentioning at the same time the principal modifications of the isobars and of the weather in the neighbourhood of the centre; secondly, to mention some remarkable facts in relation to the upper currents as observed by myself in its neighbourhood; and finally, to indicate the nature of those questions an examination of which will, I believe, in the instance before me, prove to be of most scientific value.

(1) The accompanying chart shows the course of the centre of depression, so far as we have yet been able to follow its track, the arrow-heads marking the position at 6 p.m. of each day. At 8 a.m. of the 23rd, the centre appears to have lain about 60 miles to the west of Erris Head, with a barometrical pressure of a little below 29·4. By 6 p.m. it had advanced south-eastwards into Connaught with a velocity of 6·5 English miles per hour, and by 8 a.m. of the 24th to a little west of St. Anne's Head. During the above period the depression was elongating itself, the position of its major axis changing from N.W.-S.E. into W.-E. The weather in the meantime was becoming rainy in the English Channel and home counties, while continuing fair in the north. At 6 p.m. of the 24th the eastward elongation of the whole system had become very marked; and at this hour the centre lay over the mouth of the Thames, after a somewhat lengthened thunder-storm over London, Woolwich, &c. The velocity of transit during the twenty-four hours had been 22 miles per hour, and the path of the centre was beginning to curve towards the left. By the morning of the 25th the centre had advanced to N.N.E., and lay about 53° 2′ N. lat., 0° 24′ W. long., with wet and cloudy weather over our eastern and midland districts. By 6 p.m. of that day the centre had begun to move slightly to the westward, having moved during the twenty-four hours with a velocity of 10 miles per hour. By the morning of the 26th the centre was near the mouth of the Humber, rainfall continuing over the north-east and north midland counties; at 6 p.m. of that day the centre lay over north-west Lincoln, having moved only with a velocity of about 3·8 m. per hour. The centre now moved to the neighbourhood of the Solway, with a velocity of about 10 miles per hour, and on the evening of the 27th began to recurve again a little to the left, the system at the same time becoming more circular in form, and the central pressures slightly decreasing. During this day rain and cloud prevailed on the west of the system, while in its rear there were some scattered thunder and hail showers of the type prevalent in summer in the rear of cyclonic systems travelling to north-east. At 6 p.m. on the following day the central area had passed into Ulster, with a velocity of 5·5 miles per hour. The thunderstorms in the rear were on that day more pronounced. During the following night the centre travelled with increased velocity across Donegal to the Atlantic, and by 6 p.m. of the 29th the exterior isobars of the system had almost left our shores, finer weather setting in over Great Britain generally.



(2) The point marked with an asterisk on the chart marks the position of the writer during the progress of the depression, a position of vantage for the observation of upper currents, the value of which was much diminished by the predominant thickness of low cloud, and by the fact that there was little moonlight. Over the Midlands outlying threads of "cirro-filum" advanced with great velocity from north-north-west at noon of the 23rd, soon after which a great sheet of frozen veil-cloud rapidly overspread the sky, the exterior edge of which soon disappeared over the north-east horizon. A brilliant solar halo was completely eclipsed before 5 p.m. Meanwhile the lower cloud-current backed from south-west to south. At 7.32 p.m. there was a squall of wind from south-east with rain, and a "jump" in the barograph. About noon of the following day, when the centre was about 118 miles to the south-south-west a glimpse of the upper clouds was obtained; they were then moving from south. Further opportunities of observation were obtained in the

evening, which showed that the upper current had changed to south-east. No observations could be made during the two wet days which followed; but early in the morning of the 27th, when the centre was about 100 miles to the north, true cirri were observed moving slowly from north-east. These soon disappeared; but at 6 p.m. of the same day an important change took place, the bands of ice-cloud moving from south-south-west, from which point, or from a little west of it, the belts have continued to travel up to the time of my writing this, the lines being nearly parallel to the isobars, and to the general direction of the surface winds, and precisely resembling in character the stripes seen in most cases travelling from north-north-west when a depression, whose centre has passed a little to the north of the observer, has moved away to north-east.[1]

(3) In an elaborate paper in the Quart. Journ. of the R. Met. Soc. for October 1877, the writer pointed out that in the extreme left-hand segment of an approximately circular cyclone, moving in any direction in the northern temperate latitudes, the movements of the upper currents are by no means analogous to those in the right-hand segment.[2] In the case of cyclones travelling eastwards, the reason of this difference is, I think, now well understood. Owing to the great relative density of the lower atmosphere, attended with low barometric pressure, near the poles, the gradients for westerly currents are far more constant in the upper than in the lower strata of the atmosphere in the regions traversed by extra-tropical cyclones. Over a large number of these cyclones, therefore, many of the isobars in the upper regions of the atmosphere do not form closed curves, but curves somewhat resembling those which, at the earth's surface, accompanying what are popularly termed V-shaped depressions. It is a question of the utmost interest whether, during the periods in which depressions travel to the west, the distribution of gradients in the upper atmosphere is really for the time reversed, and, if so, what can be the causes of so remarkable a change. There is a further question correlated with the above, which deserves more attention than has been given to it. The writer long ago pointed out (Journ. Scot. Met. Soc., vol. iv. pp. 333–335) that in cases of depressions travelling westward across our islands, temperatures at the earth's surface are in general higher over Scandinavia than over France; and a considerable number of instances have occurred since 1875 which have confirmed this conclusion. But in most of these cases an anticyclone has lain to the north-east of us, so that the "gradient force" of the lower strata may have tended to send the depression westwards, in addition to the ascensional force, associated with condensation in the western segment, due to the indraught of relatively warm air from north and north-east. In the instance described in this paper pressure was not particularly high over Scandinavia, during the westward progress of the system, but temperature seems to have been higher, over Sweden at least, than in France.

W. CLEMENT LEY.

May 30.

The Crowing of the Jungle Cock.

IN NATURE (vol. xliii. p. 295) Mr. Henry O. Forbes has a letter commenting on a statement of Mr. Bartlett to the effect that the wild jungle cock does not crow, and testifying that he once heard one. In reply, in the next number of NATURE, it was suggested that the cock heard by Mr. Forbes was a hybrid.

I think that no one who has travelled in the jungles of Burma, during the dry season, can have any doubt that the jungle cock crows; for he cannot fail to have heard them many times.

It so happens that, just after reading Mr. Forbes's letter, I had occasion to travel among the hills which form the watershed between the Irrawaddy and the Sittong rivers. In one region here a large kind of bamboo was seeding, so that the jungle fowl were very numerous, and I heard them crowing in great numbers. I remember one place in particular: the Karens had prepared us a hut in which to sleep just outside of their village, which consisted, like nearly all the villages in these hills, of a single house, each family having its separate room in the common building. "At cock crowing" in the morning we had, close to us, the crowing of the village cocks, and on every side, far and near, the answering crows of multitudes of wild birds. I do not remember ever to have been treated to such a chanticleer concert before.

The idea that these wild cocks were all hybrids is inadmissible, because (1) they were so very numerous, and (2) the country is very sparsely peopled, the villages all being small and far apart, and the greater part of the country still covered with primæval forest.

The crow of the jungle cock is shrill, like that of the smallest breeds of domestic fowl, and is, perhaps, a little less prolonged than that of the average domestic cock; but it can hardly be distinguished from the crow of a small breed of fowl kept by the Karens, some individuals of which so closely resemble the wild fowl that they are used as decoys.

I have several times heard wild fowl cackle, and in this journey, while in the midst of a heavy forest, miles from any human habitation, we came upon a flock of wild fowl cackling, and could tell by the tones that both cocks and hens were cackling. One of the followers being sent with a gun to try and get a shot, some of the birds saw him and flew, whereupon one of the cocks gave the peculiar call which the domestic cock gives when a bird flies over him.

I might add that, among the numerous birds shot in this region, there was one hen which had a pair of spurs about half an inch long.

B. P. CROSS.

Rangoon, May 20.

Cordylophora lacustris.

IT is generally believed that this tube-dwelling Hydrozoa was originally a salt-water animal, and although now found a considerable distance from tidal water, it still dwells in rivers and canals more or less connected with tidal rivers. I have for many years found it in the Chester and Ellesmere Port Canal, growing principally on the shells of the fresh-water mussel, from two to three miles from the tidal river (the Dee). It seems to be a shade-loving animal, as I have always found it under the bridges, and from 4 to 6 feet beneath the surface of the water.

The tubes *only* remain during the winter and early spring, and the animal is fully developed in August and September. It is generally accompanied by *Fredericella sultana*.

THOMAS SHEPHEARD.

Kingsley Lodge, Chester, June 12.

Philosophical Instrument Makers.

I FIND in your paper of June 11 (p. 135) that Messrs. Newton and Co. have been appointed philosophical instrument makers to the Royal Institution of Great Britain. Allow me to state that they are not the only ones, and that I also was appointed on June 1 by the managers of the Royal Institution of Great Britain to be their philosophical instrument maker. I thought that in the interest of the public you should know this fact.

A. HILGER.

204 Stanhope Street, Hampstead Road, June 12.

The Earthquake of June 7.

THE earthquake of June 7, whose centre seems to have been in the province of Verona, was also perceptible at Basle. The seismometer of the Bernoullianum Observatory registered a horizontal shock at 1h. 47m. 29s. a. Basle mean time, which corresponds to 1h. 17m. 10s. Greenwich mean time.

At Thal, a village east of St. Gall, the shock was strong enough to be felt by several persons.

Basle, June 13. A. RIGGENBACH-BURCKHARDT.

NOTE ON EGYPTIAN IRRIGATION.

IN entering upon any account of Egyptian irrigation it is necessary, at first, to point out that it consists of two very broad subdivisions: (1) the irrigation effected by the Nile flood when there is rich muddy water in abundance for a land thrice as big as Egypt, and when everyone considers it his absolute right to have his fields flooded without the expense or trouble of raising the water artificially; and (2) the irrigation effected by the Nile at its lowest, in those hot months of May and June when the water surface is 20 feet below that of the field, and when it is only by the strictest economy that we can water an area not exceeding one-fourth of the whole of Egypt.

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[1] These stripes or cirro-filum are so abundant in the rear of most depressions, towards the termination of the inversion disturbances accompanying squalls or thunder-showers, in Europe and the Northern States that it is singularly unfortunate that the statement of an English meteorologist, to the effect that they do not exist, should have found its way into the first edition of Ferrel's "Popular Treatise on the Winds."

[2] See also Ferrel, "Pop. Treat.," § 180; "Modern Meteorology," p. 111 (diagram).

2. *The Irrigation of Old Egypt.*—The first irrigation is the ancient art of Egypt, the culture that, from the days of the Pharaohs, made this little valley the granary of Europe. The products are wheat, barley, beans, maize, and rice. These two last crops require special irrigation. For the growth of wheat, barley, and beans, it is enough to saturate the fields, during high flood, from August to October. The seed is scattered as the waters retreat, and the fields receive neither irrigation nor rain from that time till the harvest is gathered in at the end of April.

3. *Perennial Irrigation.*—The introduction of the second system is due to the sagacity of Mohamed Ali, who saw that the conditions of soil and climate were such as to favour the growth of cotton and sugar-cane, sub-tropical products greatly exceeding the value of cereals. But these crops require irrigation during the months when the Nile is at its lowest, hence a system of deep canals was necessary, and it was in trying to carry out this system in Lower Egypt that the Egyptians got into hopeless difficulties, for the canals got blocked with silt, and it was most difficult to clear them.

4. *The Barrage unused.*—The obvious remedy was to raise the water in the river, and divert it into the canals by a Barrage or dam at the apex of the Delta. Such a work was constructed, at a cost of about two millions sterling; but soon after its completion it cracked in a very alarming way, and, from 1867 to 1883, remained practically useless. The great network of canals continued to be cleared year after year to a depth of about 20 feet below the soil, and for half of each year the *corvée* was constantly employed on them.

5. *Pumping.*—The Egyptian Government had abandoned all hope of again using the Barrage. They had entered into a contract with a private company to irrigate Behera by a system of pumps, at an annual cost of from £50,000 to £60,000; and they were about to come to similar arrangements for the rest of the Delta, at an initial cost of £700,000, and an annual one of £250,000.

6. *Neglect of Drainage.*—Continuous irrigation like that of Lower Egypt requires to be accompanied by drainage, otherwise the land becomes soured and waterlogged. No attention was being paid to this subject in 1883.

7. *State of Upper Egypt.*—The first system of irrigation alluded to above continued to be practised in Upper Egypt. A few very costly bridges had been built to assist it, but little attention was being bestowed on it, and even in years of average Nile flood we found a loss of annual revenue amounting to about £38,000 taking place.

8. *Addition to Area of Egypt.*—Such was the state of affairs when we took charge of the irrigation in 1884. I am frequently asked whether, since then, there has not been a great addition to the cultivated area of Egypt. My reply is in the negative.

The question of extending cultivation into the desert is partly one of displacement of population, chiefly one of level, for above the point that the Nile flood can be brought to reach we must not look for an extension of cultivation. Some goes on—notably to the west of the province of Behera and in the Fayoum; but it is not on a very large scale.

9. *Reclamation of Marshes.*—An extension much more rapid, and of more importance, is in progress along all the north of the Delta, where land is being yearly reclaimed from marsh and lagoon by our drainage operations.

The cultivated and revenue-paying area of Egypt is about five millions of acres. The lagoons in the north cover an area of about 1,280,000 acres. I expect in a very few years to see at least half of this land reclaimed and cultivated.

10. *The Barrage repaired, and the Effect on Lower Egypt.*—What we have done, are doing, and propose to do, then, in future years is as follows:—

First. The Barrage has been completed, and placed in a condition to fulfil its original purpose. From upstream of it are derived three main trunk canals which irrigate the whole Delta, and three smaller canals which irrigate all the country north-east of Cairo and to the south of Zagazig; one of these takes water to Port Said and Suez. The outlay on the Barrage has been, since 1884, about £460,000.

Of the three trunk canals, that on the west had been neglected, and completely filled in with sand. It has been restored, and the system of pumps alluded to in paragraph 5 will, I hope, never be used again.

The canal supplying the East Delta (termed the Tewfikieh Canal) has been entirely made since 1886, at a cost of £372,000.

Practically, the whole summer supply of the Nile is diverted by the Barrage into these canals, and none flows out useless to the sea. The value of the work is this—that so long as there is water in the Nile it is under our control, and, however low the river may fall, the water will get on to the fields, and the great cotton crop will be secured. In former days, during low Nile, the canals were left high and dry, and what water there was flowed out to the sea, useless.

The Barrage has not much increased the area of cultivation, but it has very largely increased that of land bearing double crops—that is, the area producing cotton. It was in 1884 that, by employing temporary measures, we began to use the Barrage. Since then, the average annual yield of cotton has been 333,893 kantars (15,000 tons) greater than in the five years preceding 1884. This represents a value to the country of £835,000 a year, exclusive of the value of cotton-seeds.

11. *Provision for Navigation.*—Secondly. As the abstraction of water renders impossible the river navigation during four or five months every year, two main canals have been selected, one of them roughly parallel to each of the branches of the Nile, and fitted with locks and rendered navigable. This is not yet quite finished. When it is, it will enable laden boats to pass freely between Cairo and Alexandria on one side, and Cairo and Damietta on the other side, at all seasons of the year. Other locks have been built, and obstructions removed, so that navigation has had an impulse given to it throughout the whole Delta.

12. *Drainage Introduced.*—Thirdly. Year by year have been opened out new miles of drainage arteries, and in Behera, Gharbieh, Dakahlieh, Sharkieh in Lower Egypt, and in the Fayoum, large tracts have been reclaimed from salt-marsh, and now yield good crops. The Budget for the current year contains £140,000 for new drainage-works in Lower Egypt. No part of our work has been more appreciated than this, but, unfortunately, the defective system of revenue statistics makes it impossible to say what lands have been reclaimed. The mileage of drains is not less than 1500.

13. *Measures for Improving Irrigation of Upper Egypt.*—Fourthly. I have said, in paragraph 7, that there has been an annual loss of about £38,000 in average years, due to the Nile flood not attaining all the fields of Upper Egypt. In exceptional years this loss has been much greater. Thus, after the very deficient flood of 1877 it amounted to £1,111,880. After 1888 it was about £300,000. If such was the loss of revenue alone, it may be imagined what a heavy calamity was inflicted on the

cultivators. Colonel Ross, Inspector-General of Irrigation, has studied this subject most closely. Even in these deficient years there was water enough in the river if it could only be got on to the land. He has proved that, by a judicious system of canals, sluices, siphons, escapes, weirs, &c., it may be arranged that, even in the worst years, the whole Nile valley shall receive its share of mud-charged water. This involves the construction of no great work like the Barrage (the most expensive does not exceed £45,000), but of a great number of works costing from £5000 to £15,000 each, requiring very careful designing, and built often in remote spots, where construction of any kind is difficult.

These works have been going on now for more than a year. When finished, as I hope they will be in 1893, the whole outlay will be about £600,000. And then, I trust, the lands of Upper Egypt will yield their full crop, however defective may be the Nile flood.

14. *Agricultural Roads.*—Fifthly. A minor subject, and yet one of great value to the country, deserves notice here—namely, the introduction of agricultural roads, This reform is due to Riaz Pasha. Until two years ago. it would have been impossible to take a cart-load of agricultural produce from any one centre of population to another in the Delta. Comparatively few of the canals were adapted for boats, and the one means of transporting cotton to the railway stations or to the river was by camels, which, however well adapted for carrying burdens on the firm sand of the desert, are not suitable for the rich alluvial soil and the sloppy fields of the Nile valley. This is all being changed. The people have willingly accepted a tax never exceeding P.E. 4 or 5 per feddan for one year only, and, with the fund thus raised, a whole network of serviceable roads is being formed sufficiently adapted for this dry climate.

15. *Corvée Abolition.*—The above paragraphs describe generally the improvements that have been brought about in the last seven years. Second to none is the boon that has been conferred on Egypt in the abolition of the *corvée*. Previous to 1885, the whole of the earth-work in the clearance and repairs of canals and embankments was effected by the forced, unpaid, unfed labour of the peasantry. In 1884 this labour amounted to 85,000 men working for 160 days. We were told that this was quite a necessary state of things, that it would be impossible to maintain the irrigation-works otherwise, and that the Egyptian peasant, unlike that of any other country, would not work for wages, and must be forced. We estimated that to redeem this *corvée* and to pay for all this labour would cost £400,000. Nubar Pasha, in the face of the greatest financial difficulty and opposition, managed to give an annual grant of £250,000 for this object. Riaz Pasha, at the end of 1889, found means of granting the remaining £150,000, and in 1890, for the first time perhaps in all history, there was no *corvée* in Egypt.

16. *Canal Legislation.*—When we began work here, we were much hampered by the want of any canal legislation, there being no law corresponding to what is found in India, Italy, and elsewhere, treating of the many conflicting questions connected with irrigation. After three years' discussion, a very useful Canal Act now exists, and the only misfortune is that it is not binding on residents of foreign nationality.

17. *Storage of Nile Water.*—Lastly, as regards our programme for the future, there is abundance to do in carrying out, year by year, solid unpretending reforms; but, besides these, a very large question is coming to the front. The restoration of the Barrage placed at our disposal all the water of low Nile, but the increase in the area irrigated outruns the increase in the water available, and we have to look for means of storing the surplus volume of the flood, and utilizing it when the river is low.

There are two ways in which this may probably be done. The first, which is connected with the name of an ingenious American gentleman, Mr. Cope Whitehouse, is to divert a portion of the flood into a great natural depression existing west of the Nile valley, and there to form a storage reservoir, to be drawn upon as the water in the river decreases. This has been examined and found feasible, but the expense, probably 1½ millions sterling, is against it. The alternative project is to pond up water in the valley of the river itself above Assouan. This project is being studied at present. There can be hardly any further extension of the cotton cultivation if one or the other of these schemes is not executed. There is room enough in the country to employ both.

Colin Scott Moncrieff,
Under-Secretary of State, Public Works Department.

Cairo, March 5, 1891.

THE SECOND ORNITHOLOGICAL CONGRESS.

A FULL report of the proceedings of this important Congress can only be obtained when the official *Comptes rendus* are published, for the officers of one section were unable to attend the meetings of the other sections owing to the fact that all four sections sat at one and the same time. This is the only complaint we have to make concerning the recent proceedings, but as it affects the future of these useful reunions, we feel compelled to make our protest, because, by the simultaneous session of all the sections of a Congress, no man, however interested in the subjects under discussion, can hear all that he wishes to hear; the visitor has to choose between two meetings, both of which probably possess for him an equal interest. It must be obvious to everyone who had the privilege of attending the second Ornithological Congress that a great gathering of specialists such as that which took place last month must require more time than three days to discuss such varied problems as were placed before them at the recent meeting.

The city of Budapest was happily chosen as the meeting-place of the Congress, and it may well be questioned whether there is any country in the world that could have offered so many attractions to the ornithologist as Hungary. The hospitality of the Hungarians is proverbial, the accommodation in the beautiful capital is unlimited, and access thereto is easy. After an enjoyable trip down the Danube from Vienna, the travellers found themselves at the opening *conversazione* of the Congress, which was celebrated in the Grand Hotel "Hungaria." Here the Hungarian Committee had assembled with all the members of the Congress to welcome the guests, and the inaugural banquet served as a pleasant medium for the introduction of the strangers. On May 17 the first general meeting of the Congress took place in the sumptuous theatre of the Hungarian National Museum. After some words of welcome from the Burgomaster of Budapest, the officers for the Congress were chosen as follows:—Honorary Presidents: Count Bethlen, Minister of Agriculture; Count A. Csáky, Minister of Public Instruction; Mr. B. Kállay, Minister of Finance. Presidents: Prof. Victor Fatio (Geneva) and Mr. Otto Herman, M.P. Vice-Presidents: Dr. Rudolph Blasius (Brunswick), Prof. S. Brusina (Agram), Prof. R. Collett (Christiania), Mr. J. de Csató (Budapest), Dr. Otto Finsch (Bremen), Major Alex. von Homeyer (Greifswald), Dr. A. B. Meyer (Dresden), Dr. E. von Middendorf (Livonia), Dr. Emil Oustalet (Paris), Dr. Bowdler Sharpe (British Museum), Mr. E. von Szalay (Budapest), Victor Ritter Tschusi von Schmidhoffen (Hallein). General Secretary: Dr. G. von Horváth. Secretaries: Mr. E. Chernel von Chernelháza, Dr. A. Lendl, Dr. L. Lorenz von Liburnau, Dr. A. Lovassy, Dr. J. von Madarász, Mr. O. Reiser, Prof. G. Szikla. Hon. Secretaries: Mr. E. de Gaál, Mr. B. de Lipthay, Mr. J. d'Ottlik. Quæstor: Mr. J. von

Xántus. After preliminary reports, Major Alex. von Homeyer gave his reminiscences of travel in West Africa some years ago, and his imitations of the notes of African birds were strikingly rendered. Four different sections of the Congress were appointed, the names of the different delegates from foreign countries were read out, as well as letters of apology for their absence from several naturalists, Prof. Fürbringer, Baron de Selys Longchamps, and others.

The officers of the different sections were constituted as follows:—(1) Systematic Section: Presidents, Dr. Bowdler Sharpe (London) and Prof. Claus (Vienna); Vice-Presidents, Dr. A. Reichenow (Berlin) and Mr. C. G. Danford (Siebenburgen). (2) Biology and Oology: President, Dr. Rudolph Blasius. (3) Avigeographia: President, Dr. Palacky (Prag). (4) Economic Ornithology: President, Major Alex. von Homeyer.

On the afternoon of May 17 many of the members of the Congress ascended the Blocksberg, to enjoy a view of the city of Budapest and the Danube flowing below—a view not to be surpassed in beauty and interest in any country.

On Monday, May 18, the Systematic Section met in the lecture-theatre of the Polytechnicum, which was placed at the disposal of the Congress by Prof. Szabo, whose work is well known and appreciated in Great Britain. Papers were read by Prof. Klug, on some points in the anatomy of the stomach in birds, and by Dr. Bowdler Sharpe on the classification of birds, the latter lecture being illustrated by several large diagrams and a wax model of the phylogenetic tree, in which Prof. Fürbringer traces the evolution of birds from a reptilian stock. The remainder of the work of the Systematic Section consisted in the passing of the rules of nomenclature, as put forward by a committee consisting of Prof. Möbius, Dr. A. Reichenow, Count von Berlepoch, Dr. A. B. Meyer, and Dr. W. Blasius. The recommendations of this committee were adopted almost in their entirety by the meeting, after a two-days' discussion, notwithstanding some protests of Dr. Sharpe, and Mr. Büttikofer of the Royal Museum of Leyden, who found themselves in a hopeless minority. The chief points carried were: the adoption of the 10th instead of the 12th edition of the "Systema Naturæ" of Linnæus, the recognition of trinomial names in certain cases, and the adoption of names, even faulty in construction or misspelt, with all the consequences. The tone of the report, however, is so moderate, and exhibits so much consideration for the methods of other ornithologists, that it ought to be possible now to arrive at a definite conclusion for European usage at least; and then it would be easy to assimilate the American and European methods of nomenclature.

In the afternoon of the 18th, the Congress met in the Museum, and Dr. Otto Herman, M.P., gave an account of the distribution of birds in Hungary, and explained the collections which had been made specially for the Congress. These consisted of beautifully mounted cases of Hungarian birds with nests and natural surroundings: some very rare species were included in the collection, which was the work of four ornithologists—Dr. O. Herman, M.P., Dr. Julius von Madarász, Mr. Chernel, and Prof. Szikla. These gentlemen had each occupied a station in different parts of Hungary, and had not only collected the series of birds exhibited, but had also made exact observations on migration and distribution. The Hungarian National Museum is a very fine building, and contains a collection which fairly surprised most of the visitors, the series of native birds being especially complete. Large groups of Laemmergeiers, Sea Eagles, Ospreys, &c., with their nests, eggs, and young birds, are to be seen in the Bird-galleries, and these are principally the work of a well-known Hungarian ornithologist, Dr. J. von Madarász. The collection of Mammalia also comprises some great rarities, and the whole Museum teems with specimens procured by the veteran explorer, Mr. J. von Xántus, whose labours in Lower California and Central America, as well as in Borneo and the Sunda Islands, are also widely known. The Museum likewise contains a fine series of insects, especially Coleoptera, which were shown with much natural pride by Dr. Frivaldszky, who is responsible for the beautiful arrangement of the latter groups. The afternoon closed with an adjournment to the Hungarian Academy of Sciences, where Prof. Robert Collett read a paper on Arctic Bird-life before a crowded audience; and the evening concluded with a banquet at the "Archiduc Stephan" Hotel.

On Tuesday the debate on nomenclature was continued; and in the afternoon the Congress assembled on St. Margaret's Island, which forms a most delightful summer retreat for the inhabitants of Budapest, with its dozens of nightingales, its ruined cloisters, and its sulphur springs.

On Wednesday, the 20th, the general meeting of the Congress was held to receive the reports of the different sections and committees, and the business was concluded. A farewell banquet took place in the evening, and the second Ornithological Congress came to an end.

Next day the members were scattered in different directions—some to their homes, some to join one of the pre-arranged excursions. These were three in number—one to the Hanság marshes and Fertö, a second to the Platten-See, and a third to the districts of the Drave. Of the first excursion, in which the writer took part, he can only say that, under the direction of Dr. von Madarász, the members of the Congress who accompanied it underwent a never-to-be-forgotten experience. The species of birds observed were mostly those unknown to an English naturalist, and the hospitality dispensed by Prince Esterhazy, Baron von Berg, and Count Széchenyi, is not likely to disappear from the memory of those who had the good fortune to partake of it.

THE IMPERIAL PHYSICAL AND TECHNICAL INSTITUTION AT BERLIN.

THE Imperial Physical and Technical Institution which was founded in 1887 at Charlottenburg, near Berlin, under the auspices of the German Government, has now been for some time in active operation, and recently there has been issued by the executive Director, Dr. L. Loewenherz, a Report on the work of the Institution up to the end of last year.

It may be remembered that the Institution has two main objects in view: first, that of physical and technical research appropriate to the practical development of manufacture—researches for instance as to the qualities of metals and materials and as to methods of construction and measurement; the second object being that of fundamental research in theoretical problems in physics, and the testing of all kinds of measuring apparatus applicable for use in science, art, and manufacture. It appears to undertake, therefore, investigations and verifications similar to those undertaken in this country by the Board of Trade, or at the Kew Observatory; and, in France, by the Bureau International des Poids et Mesures. Its staff includes (exclusive of the clerical staff) a President, nominated by the Reichstag; a Director, with a Committee of seven members; seven scientific officers in the department of research; four technical assistants, and several mechanics and machinists.

From time to time, as new methods of testing are adopted, or as fresh work is undertaken, explanatory papers are issued by the responsible officers of the Institution (printed by Julius Springer, Berlin); and the following papers have, amongst others, been already issued:—Karl Scheel, H. F. Wiebe, and Alfr. Böttcher, on meteorological measurements; Dr. K. Feussner and Dr. St. Lindeck, on electrical measurements; Dr. O. Lummer and Dr. E. Brodhun, on optical measurements, including photometry; Dr. F. Foerster and Dr. F. Milius, on chemical analysis of glass.

We gather from the Director's Report above referred to, that the Institution has provided itself with fundamental standards of length and mass; with primary thermometers and barometers; with electrical standards of resistance, current, and pressure; and with apparatus for testing the flashing point of petroleum and inflammable liquids. Its metrological work for the public has included the proving of clinical thermometers, pyrometers, aneroid barometers, manometers, alcohol thermometers for low temperature, and thermometers for chemical research.

In October 1888, the official testing of thermometers was transferred from the Normal Aichungs Commission at Berlin to the Imperial Institution, and all thermometers are still tested on the basis of the regulations laid down by the Commission on November 10, 1885; excepting that, in place of basing the errors of scientific thermometers on a mercurial thermometer, thermometer readings are now reduced to the more accurate scale of the air-thermometer or hydrogen-thermometer.

The use of thermometers for determining pressures, or altitudes, &c., on the occasion of journeys of exploration, &c., seems of late to have increased, for many such have been already presented for examination at the Institution. If the thermometers are made of Jena glass (or of other hard thermometer glass), it would appear to be possible to ascertain pressures with but little trouble to ± 0·25 millimetre. The necessity for using proper glass is shown in an experiment carried out at the Institution with two thermometers, Nos. 42 and 43, made of ordinary Thuringian and crystal glass. On September 7, 1888, the corrections of these thermometers at 87° C. were found to be—

No. 42, $-0^\circ{\cdot}05$; No. 43, $-0^\circ{\cdot}24$ C.

The thermometers were then heated for 15 minutes to a temperature of 100° C.; they were then allowed to cool, and subsequently retested on September 10, when their errors were found to be—

No. 42, $+0^\circ{\cdot}08$; No. 43, $-0^\circ{\cdot}09$ C.

Such variation in the reading of a thermometer after its exposure to a high temperature would unfit it for use in the exact determination of pressures or altitudes.

With reference to the testing of various sorts of glass Dr. F. Milius points out that Weber's process, generally made use of, and which consists in exposing the body to be examined to an atmosphere of muriatic acid vapour for a space of twenty-four hours, is not always trustworthy. Thus, according to the quality of the glass, it appears to be covered more or less, after exposure to the acid vapour, by a thick rime (or hoar frost); and that although the experienced observer finds Weber's method tolerably certain, yet the less experienced observer may sometimes be left in doubt, particularly where rough surfaces are treated, as to whether the rime exists or not; Dr. Milius therefore proposes an optical form of test other than that of the muriatic acid test, as is explained at length in his paper.

Dr. Milius, in conjunction with Dr. F. Foerster, has also investigated the solubility, in water, of potash and soda glass, particularly with reference to Schott's experiments as to the capacity of potash water-glass for absorbing water without losing its vitreous quality. This latter fact can be ascertained by keeping pulverized water-glass under water, when, as in the case of hydraulic cement, a hardening of the paste begins to take place. This process is connected with a development of heat; in the case of water-glass in which there was one atom of potash to three of silicic acid it was observed at the Institution that within a quarter of an hour the moistened matter had been heated 10° Centigrade, and it became hard in one day; if the proportion of silicic acid is larger, the glass requires from two to three days for solidification. Their researches appear to show that for purposes connected with mercurial electrical standards, the glass used should be very little soluble in water and acids; hard glass, for instance, which had a base of soda, and not potash, being little hygrometric.

In the important field of electrical measurements, the Institution appears also to be doing good work. It is preparing to undertake the verification of all kinds of apparatus; including voltmeters, ammeters, meters for the measurement of power and efficiency, galvanometers, and resistance coils.

In the field of practical photometry we have to compare the intensities of different sources of light as experienced by the eye; but unfortunately we have not, even for commercial purposes, any satisfactory method by which intercomparisons may be made between the relative intensities of coal-gas, electric and oil lights respectively. In practical photometry much is being done in this country by Abney, Vernon-Harcourt, Chaney, and others, as well as by Lummer, Brodhun, and others in Germany, but as yet no standard photometer has been produced. The standard light is still also the ancient "sperm-candle," and the method of comparison is still the old-fashioned "grease-spot" Bunsen photometer more or less modified. The German authorities appear to be fully alive to the necessity of improvement in this field of technical research; and have investigated M. Violle's incandescent platinum-standard of light, and also the Hefner lamp and Aubert's apparatus; and for electrical light purposes they have followed a form of standard glow lamp.

Among the papers above referred to, we notice also one by Dr. Loewenherz, on the testing of tuning-forks. The Institution undertakes the testing of tuning-forks, on payment of a small fee, the object of the examination being to ascertain the correctness of the height of the tone of the fork in terms of an international diapason; or the number of the vibrations of the fork per second, at the temperature of 15° Centigrade, the pitch of the note A being fixed at 435 entire vibrations per second, or 870 half or single vibrations according to the French method of counting. Tuning-forks sent to the Institution for examination are required to be constructed in accordance with conditions laid down by the Institution. Unity of pitch is of fundamental importance in music and in the construction of musical instruments, and it is to be desired that some authoritative testing of tuning-forks might be similarly undertaken in this country.

In metallurgy the work of the chemical laboratories of the Institution does not appear to be extensive; it has included more particularly analyses of the metals platinum, cadmium, and rhodium. In the Physical Laboratory, measuring instruments of precision for workshop use, such as speed and power indicators, screw-thread gauges, have also been examined by the Institution; and its geodetical work has included the verification of instruments of precision for General Schreiber, of the Imperial Prussian Land Survey. The department has undertaken also the verification of polariscopes, lenses, prisms, and other optical instruments, to a limited extent.

The above observations may serve to show that the Institution is alike prepared to verify a standard—as a measurer of electrical resistance—with the utmost accuracy, or to test an instrument for common purposes—as a gas meter. How far the Institution may be self-supporting is not stated in the Director's Report; but as the demands for verification work of this kind are largely voluntary, it would appear to be evident that the excellent staff of the Institution could not be maintained unless it received valuable support from the State.

CRYSTALLIZATION.[1]

THERE is something very fascinating about crystals. It is not merely the intrinsic beauty of their forms, their picturesque grouping, and the play of light upon their faces, but there is a feeling of wonder at the power of Nature, which causes substances, in passing from the fluid to the solid state, to assume regular shapes bounded by plane faces, each substance with its own set of forms, and its faces arranged with characteristic symmetry: some, like alum, in perfect octahedra; others, like blue vitriol, in shapes which are regularly oblique. It is this power of Nature which is the subject of this discourse. I hope to show that crystalline forms, with all their regularity and symmetry, are the outcome of the accepted principles of mechanics. I shall invoke no peculiar force, but only such as we are already familiar with in other facts of Nature. I shall call in only the same force that produces the rise of a liquid in a capillary tube and the surface-tension at the boundary of two substances which do not mix. Whether this force be different from gravity I need not stop to inquire, for any attractive force which for small masses, such as we suppose the molecules of matter to be, is only sensible at insensible distances is sufficient for my purpose.

We know that the external forms of crystals are intimately connected with their internal structure. This is betrayed by the cleavages with which in mica and selenite everybody is familiar, and which extend to the minutest parts, as is seen in the tiny rhombs which form the dust of crushed calcite. It is better marked by the optical properties, single and double refraction, and the effects of crystals on polarized light. These familiar facts lead up to the thought that it is really the internal structure which determines the external form. As a starting-point for considering that structure, I assume that crystalline matter is made up of molecules, and that, whereas in the fluid state the molecules move about amongst themselves, in the solid state they have little freedom. They are always within the range of each other's influence, and do not change their relative places. Nevertheless, these molecules are in constant and very rapid motion. Not only will they communicate heat to colder bodies in contact with them, but they are always radiating, which means producing waves in the ether at the rate of many billions in a second. We are sure that they have a great deal of energy, and, if they cannot move far, they must have very rapid vibratory motions. It is reasonable to suppose that the parts of each molecule swing, backwards and forwards, through, or about, the centre of mass of the molecule. The average distances to which the parts swing will determine the average dimensions of the molecule, the average space it occupies.

Dalton fancied he had proved that the atoms of the chemical elements must be spherical, because there was no assignable cause why they should be longer in one dimension than another. I rather invert his argument. I see no reason why the excursions of the parts of a molecule from the centre of mass should be equal in all directions, and therefore assume, as the most general case, that these excursions are unequal in different directions. And, since the movements must be symmetrical with reference to the centre of mass of the molecule, they will in general be included within an ellipsoid, of which the centre is the centre of mass.

Here I may, perhaps, guard against a misconception. We chemists are familiar with the notion of complex molecules; and most of us figure to ourselves a molecule of common salt as consisting of an atom of sodium and one of chlorine held together by some sort of force, and it may be imagined that these atoms are the parts of the molecules which I have in mind. That, however, is not my notion. I am paradoxical enough to disbelieve altogether in the existence of either sodium or chlorine in common salt. Were my audience a less philosophical one I could imagine I heard the retort from many a lip: "Why, you can get sodium and chlorine out of it, and you can make it out of sodium and chlorine!" But no, you cannot get either sodium or chlorine out of common salt without first adding something which seems to me of the essence of the matter. You can get neither sodium nor chlorine from it without adding energy; nor can you make it out of these elements without subtracting energy. My point is that energy is of the essence of the molecule. Each kind of molecule has its own motion; and in this I think most physicists will agree with me. Chemists will agree with me in thinking that all the molecules of the same element, or compound, are alike in mass, and in the space they occupy at a given temperature and pressure. The only remaining assumption I make is that the form of the ellipsoid—the relative lengths of its axes—is *on the average* the same for all the molecules of the same substance. This implies that the distances of the excursions of the parts of the molecule depend on its constitution, and are, on the average, the same in similarly constituted molecules under similar circumstances.

I have come to the end of my postulates. I hope they are such as you will readily concede. I want you to conceive of each molecule as having its parts in extremely rapid vibration, so that it occupies a larger space than it would occupy if its parts were at rest; and that the excursions of the parts about the centre of mass are on the average, at a given temperature and pressure, comprised within a certain ellipsoid; that the dimensions of this ellipsoid are the same for all molecules of the same chemical constitution, but different for molecules of different kinds.

We have now to consider how these molecules will pack themselves on passing from the fluid state, in which they can and do move about amongst themselves, into the solid state, in which they have no sensible freedom. If they attract one another, according to any law, and for my purpose gravity will suffice, then the laws of energy require that for stable equilibrium the potential energy of the system shall be a minimum. This is the same, in the case we are considering, as saying that the molecules shall be packed in such a way that the distances between their centres of mass shall on the whole be the least possible; or, that as many of them as possible shall be packed into unit space. In order to see how this packing will take place, it will be easiest to consider first the particular case in which the axes of the ellipsoids are all equal—that is, when the ellipsoids happen to be spheres. The problem is then reduced to finding how to pack the greatest number of equal spherical balls into a given space. It is easy to reduce this to the problem of finding how the spheres can be arranged so that each one shall be touched by as many others as possible. In this way the cornered spaces between the balls, the unoccupied room, is reduced to a minimum. You can stack balls so that each is touched by twelve others, but not by more. At first sight it seems as if this might be done in two ways.

In the first place we may start with a square of balls, as in Fig. 1, where each is touched by four others. We may then place another (shaded in the figure) so as to rest on four, and place four more in adjacent holes to touch it, as indicated by the dotted circles. Above these four more may be placed in the openings *a b c d*, so as to touch it—making twelve in all. If the pile be completed, we shall get a four-sided pyramid, of which each side is an equilateral triangle, as represented in Fig. 2. It will be seen that, in these triangular faces, each ball (except, of course, those forming the edges) is touched by six others.

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[1] A Discourse delivered at the Royal Institution of Great Britain on Friday, May 15, 1891, by G. D. Liveing, F.R.S.

Again, if we start with such a triangle, as in Fig. 3, where each ball is touched by six others, we can place one ball —the shaded one—so as to rest on three others, and can then place six more round it and touching it, as indicated by the dotted circles. In three of the triangular holes between the shaded ball and the dotted balls touching it we can place three more, so as to touch the shaded ball—again twelve touching it in all. If we complete the pile, we shall get the triangular pyramid represented by Fig. 4, where each of the three sides is a right-angled triangle, while the base is an equilateral triangle. It will be seen that in the faces of this pyramid each ball (except those outside) is touched by four others. In fact, the arrangement in these faces is the same as in the base of the former pyramid; and the two arrangements are really identical in the interior, only one has to be turned over in order to bring it into parallelism with the other. Fig. 2 represents half a regular octahedron; Fig. 4 the corner of a cube. Ellipsoids, if they are all equal and similar to one another, can be packed in precisely the same way, so that each is touched by twelve others, provided their axes are kept parallel to each other —that is, if they are all oriented alike. This, then, by the laws of energy, will be the arrangement which the molecules will assume, in consequence of mutual attraction, in passing from a fluid to a solid state.

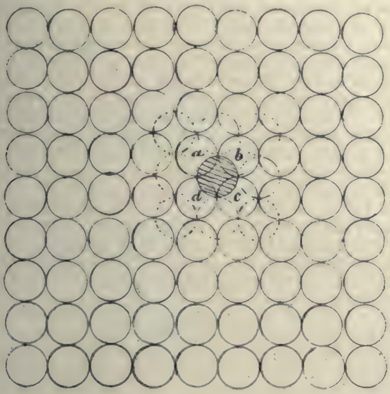

FIG. 1.

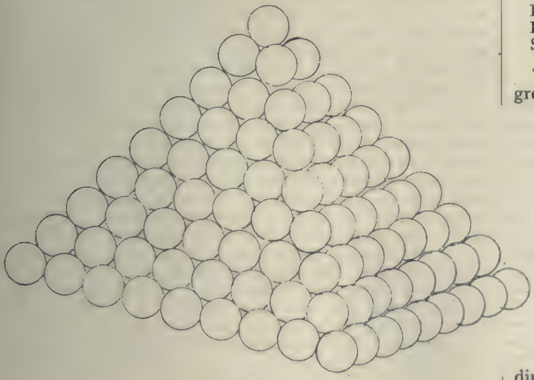

FIG. 2.

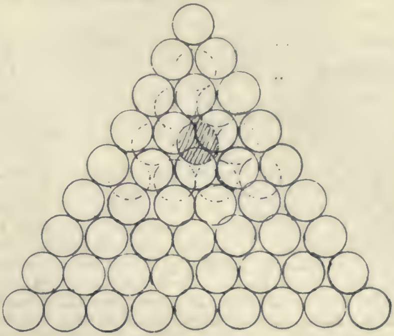

FIG. 3.

Next, let us see how the packing of the molecules will affect the external form. And here I bring in the surface-tension. We are familiar with the effects of this force in the case of liquids, and if we adopt the usually received theory of it, we must have a surface-tension at the boundary of a solid, as well as at the surface of a liquid. I know of no actual measures of the surface-tension of solids; but Quincke has given us the surface-tensions of a number of substances at temperatures near their points of solidification, in dynes per lineal centimetre, as follows :—

Platinum	1658	Antimony	244
Gold	983	Borax	212
Zinc	860	Sodium carbonate ...	206
Tin	587	Sodium chloride ...	114
Mercury	577	Water	86·2
Lead	448	Selenium	70·4
Silver	419	Sulphur	41·3
Bismuth	382	Phosphorus	41·1
Potassium	364	Wax	33·4
Sodium	253		

The surface-tensions of most of the solids are probably greater than these, for the surface-tension generally diminishes with increase of temperature; and you see that they amount to very considerable forces. We have to do, then, with an agency which we cannot neglect. In all these cases the tension measured is at a surface bounded by air, and is such as tends to contract the surface. We have, then, at the boundary between a crystallizing solid and the fluid, be it gas or liquid, out of which it is solidifying, a certain amount of potential energy; and by the laws of energy the condition of equilibrium is, that this potential energy shall be a minimum. The accepted theory of surface-tension is that it arises from the mutual

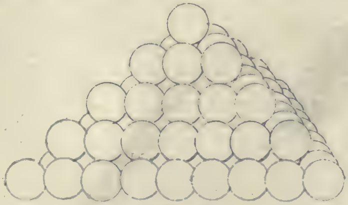

FIG. 4.

attraction of the molecules. The energy will therefore be a minimum for a surface in which the molecules are as closely set as possible.

Now, if you draw a surface through a heap of balls packed so that each is touched by twelve others, you will find that the surfaces which have the greatest number of centres of balls per unit area are all plane surfaces. That in which the concentration is greatest is the surface of a regular octahedron, next comes that of a cube, then that of a rhombic dodecahedron, and so on according to the law of indices of crystallographers.

The relative numerical values of these concentrations are as follows, taking that of the faces of the cube as unity:—

Octahedron	1·1547	Tetrakishexahedron	0·4472
Cube	1·0000	Eikositessarahedron	0·4083
Dodecahedron ...	0·7071	Triakisoctahedron ...	0·3333

We do not know that the surface-tension is exactly in the inverse proportion to the concentration, all that we can at present say is that it increases as the concentration diminishes.

If, then, the molecules occupy spherical spaces, the bounding surface will *tend* to be a regular octahedron.

But we have another point to consider. If a solid is bounded by plane surfaces, there must be edges where these planes meet. At such an edge the surface-tensions will have a resultant (see Fig. 5) tending to compress the mass, which must be met by a corresponding opposite pressure, and unless there is some internal strain there must be a corresponding resultant of the tensions on the opposite side of the crystal. Hence, if one face of a form is developed the opposite face will also be developed ; and generally, if one face of a form be developed all the faces will be developed ; and if one edge, or angle, be truncated, all the corresponding edges, or angles, will be truncated. Were it otherwise, there would not be a balance between the surface-tensions in the several faces. But there is another point to be taken into account. The surface energy may become less in two ways—either by reducing the tension per unit surface, or by reducing the total surface. When a liquid separates from another fluid, as chloroform from a solution of chloral hydrate on adding an alkali, or a cloud from moist air, the liquid assumes the form which, for a given mass, has the least surface—that is, the drops are spherical. If you cut off the projecting corners and plane away the projecting edges of a cube or an octahedron, you bring it nearer to a sphere, and if you suppose the volume to remain constant, you still diminish the surface. And if the diminution of the total surface is not compensated by the increased energy on the truncations, there will be a tendency for the crystals to grow with such truncations. The like will be true in more complicated combinations. There will be a tendency for such combinations to form, provided the surface energy of the new faces is not too great as compared with that of the first simple form.

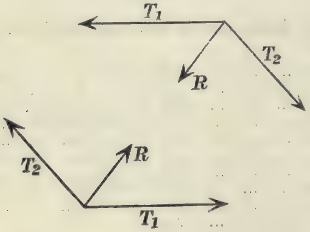

FIG. 5.

But it does not always happen that an octahedron of alum develops truncated angles. This leads to another point. To produce a surface in a continuous mass requires a supply of energy, and to generate a surface in the interior of any fluid is not easy. Air may be supersaturated with aqueous vapour, or a solution with a salt, and no cloud or crystals be formed, unless there is some discontinuity in the mass, specks of dust, or something of the kind. In like manner, if we have a surface already, as when a supersaturated solution meets the air or the sides of the vessel containing it, and if the energy of either of these surfaces is less than that of a crystal of the salt, some energy will have to be supplied in order to produce the new surface, but not so much as if there were no surface there to begin with. Hence, crystals usually form on the sides of the vessel or at the top of the liquid. When a solid separates from a solution there is generally some energy available from the change of state, which supplies the energy for the new surface. But at first when the mass deposited is very small the energy available will be correspondingly small, and since the mass varies as the cube of the diameter of the solid, whereas the surface varies as the square of the diameter, the first separated mass is liable to be squeezed into liquid again by its own surface-tension. This explains the usual phenomena of supersaturated solutions. A deposit occurs most easily on a surface of the same energy as that of the deposit, because the additional energy required is only for the increased extent of surface. It explains, too, the tendency of large crystals to grow more rapidly than small ones, because the ratio of the increase of surface to that of volume diminishes as the crystal grows.

While speaking of the difficulty of creating a new surface in the interior of a mass, the question of cleavage suggests itself. In dividing a crystal we create two new surfaces—one on each piece, and each with its own energy. The division must therefore take place most readily when that surface energy is a minimum. Hence the principal cleavage of a crystal made up of molecules having their motions comprised within spherical spaces will be octahedral. As a fact, we find that the greater part of substances which crystallize in the octahedral, or regular system, have octahedral cleavage. But not all ; there are some, like rock salt and galena, which cleave into cubes, and a very few, like blende, have their easiest cleavage dodecahedral. These I have to explain. I may, however, first observe that some substances—as, for instance, fluor-spar—which have a very distinct octahedral cleavage are rarely met with in the form of octahedra, but usually in cubes. In regard to this, we must remember that the surface energy depends upon the nature of both the substances in contact at the surface, as well as on their electrical condition, their temperature, and other circumstances. The closeness of the molecules in the surface of the solid determines the energy, so far as the solid alone is concerned ; but that is not the only, though it may be the most important factor conducing to the result. It is therefore quite possible that, under the circumstances in which the natural crystals of fluor were formed, the surface energy of the cubical faces was less than that of the octahedral, although when we experiment on them in the air it is the other way. This supposition is confirmed by the well-known fact that the form assumed by many salts in crystallizing is affected by the character of the solution. Thus alum, which from a solution in pure water always assumes the octahedral form, takes the cubic form when the solution has been neutralized with potash.

To return to the cubic and dodecahedral cleavages. If we suppose the excursions of the parts of the molecule to be greater in one direction than in the others, the figure within which the molecule is comprised will be a prolate spheroid ; if less, an oblate spheroid. Now, as already explained, the spheroids will be packed as closely as possible if the axes are all parallel and each is touched by twelve others. Now suppose the spheroids arranged as in Fig. 6, with their axes perpendicular to the plane of the figure; place the next layer in the black triangular spaces, and complete the pyramid. The three faces of the pyramid will be equal isosceles triangles; and if the spheroids be oblate, and the axis half the greatest diameter, the three angles at the apex of the pyramid will be right angles. The crystal will have cubic symmetry, but the relative condensation in the faces of the cube, octahedron, and dodecahedron, will be as 1 : 0·5774 : 0·7071. The easiest cleavage would therefore be cubic, as in rock salt and galena.

Again, if the spheroids have their axes and greatest diameters in the ratio of $1 : \sqrt{2}$, and we place four, as in Fig. 7, with their axes perpendicular to the plane of the figure, then place one upon them in the middle, and then four more upon it, in positions corresponding to those of the first four, we get a cubical arrangement, the centre of a spheroid in each angle of a cube, and one in the centre of the cube. Crystals so formed will have cubic symmetry, but the concentration of molecules will be greatest in the faces of the dodecahedron, and their easiest cleavage will be, like that of blende, dodecahedral.

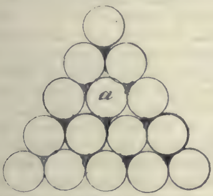

Fig. 6.

If spheroids of any other dimensions be arranged, as in Figs. 1 and 2, with their axes perpendicular to the plane of Fig. 1, we shall get a crystal with the symmetry of the pyramidal system. If the spheroids be prolate, the fundamental octahedron will be elongated in the direction of the axis, and if sufficiently elongated, the greatest condensation will be in planes perpendicular to the axis, and the easiest cleavage, as in prussiate of potash, in those planes. On the other hand, if the spheroids be sufficiently oblate, the easiest cleavage will be parallel to the axis.

If spheroids be arranged, as in Fig. 6, with their axes perpendicular to the plane of the figure, they will, in general, produce rhombohedral symmetry, with the rhombs acute or obtuse, according to the length or shortness of the axes of the spheroids. The cubical form already described is only a particular case of the rhombohedral. If the ratio between the axes of the spheroids and their greatest diameters be only a little greater, or a little less, than 1 : 2, the condensation will be greatest in the faces of the rhombohedron, and the easiest cleavage will be rhombohedral, as in calcite. If the spheroids be prolate, the easiest cleavage will be perpendicular to the axis of symmetry, as in beryl and many other crystals. Such crystals have a tendency to assume hexagonal forms—equiangular six-sided prisms and pyramids. To explain this, it may be seen in Fig. 6 that, in placing the next layer upon the spheroids represented in the figure, the three spheroids which touch that marked *a* may occupy either the three adjacent white triangles or the three black ones. Either position is equally probable. The layer occupying the white triangles is in the position of a twin to that occupying the black triangles. So far as the central parts of the layer are concerned, it will make no difference in which of these ways the molecules are packed. It is only at the edges that the surface-tension will be affected. If the form growing be a rhombohedron, a succession of alternating twins will produce a series of alternating ridges and furrows in the rhombohedral faces, which will give rise to increased surface-tension, which will tend to prevent the twinning. On the other hand, an hexagonal form and its twin, formed in the way indicated, are identical, and we have in this fact a cause tending to the production of hexagonal forms. This tendency is increased by the fact that, for a given volume, the total surface of the hexagonal forms is in general less than that of the rhombohedral. Indeed, such forms lend themselves to the formation of almost globular crystals, as is well seen in pyromorphite and mimetite.

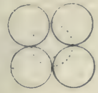

Fig. 7.

If the spheroids be arranged with their axes in other positions than those we have been discussing, or if the molecules occupy ellipsoidal spaces, they will, when packed so that each is touched by twelve others, give figures of less symmetry. The results may be worked out on the lines indicated in the foregoing discussion, and will be found to correspond throughout to the observed facts.

Bravais long ago proposed various arrangements of molecules to account for crystalline forms, and Sohncke has extended them to further degrees of complication in order to account for additional facts in crystallography. But neither of them has given any reason why the molecules should assume such arrangements. To me it seems that only one arrangement can be spontaneously assumed by the molecules, and that the varieties of crystalline form depend on the dimensions of the ellipsoids and the orientation of their axes. Curie also has indicated that the development of combined forms, as those of cube and octahedron, will depend on the surface-tensions in the faces of these forms, but he has not indicated how the surface-tension is connected with the crystalline arrangement, or why the energy of a cubic face should be greater or less than that of an octahedral face.

We are now in a position to understand the interesting facts brought forward by Prof. Judd in a discourse delivered at the Royal Institution early this year. However long a crystal has been out of the solution, or vapour, from which it was formed, its surface-tension will remain unaltered, and when it is replaced it will grow exactly as if it had not been removed. Also, if any part be broken off it, the tension of the broken surface will, if it be not a cleavage face, be greater than on a face of the crystal, and in growing, the laws of energy necessarily cause it to grow in such a way as to reduce the potential energy—that is, to replace the broken surface by the regular planes of less surface energy. The formation of "negative crystals" by fusing a portion in the interior of a crystalline mass, is due to the same principle. Surfaces of least energy will be most easily produced inside as well as outside, and in a crystalline mass of course they will be parallel to the external faces of the crystal. We see the same thing in the action of solvents. Most metals assume a crystalline texture on cooling from fusion, and when slowly acted on by dilute acids the surfaces of greater energy are most easily attacked, in accordance with the laws of energy, and the undissolved metal is left with surfaces of least energy which are the faces of crystals. This is easily seen on treating a piece of tin plate, or of galvanized iron, with very dilute aqua regia. In fact, solution is closely connected with surface energy. It is probably the low surface energy of one form of crystals of sulphur which makes them insoluble in carbon disulphide, and this low surface energy may be an electrical effect.

I pointed out that the development of all the faces of a form, and the similar modification of all corresponding edges and angles of a crystal, is in general necessary in order to produce equilibrium under the surface-tensions. But we sometimes find crystals with only half the modifications required for symmetry. In such cases the surface-tensions must produce a stress in the interior tending to deform the molecules. When the crystal was growing, there must have been equilibrium, and therefore a pressure equal and opposite to this effect of the surface-tension. There are various ways in which we may suppose that such a force would arise. The electric field might give rise to a stress in opposition to the aggregation of the molecules in the closest possible way, and then the crystal would grow such faces as would produce an equal and opposite stress. Inequalities of temperature, or the presence of molecules of other kinds amongst those of the crystal, might produce similar results. When the stress due to electricity, or to temperature, was removed by change of circumstances, that due to the surface-tensions would persist, and the crystal would be left with an internal strain. Crystals of this sort, with unsymmetric faces, generally betray the internal strain, either by developing electricity of opposite kinds at the two ends when heated or cooled, or they affect polarized light, rotating the plane of polarization. That these effects are due to the internal strain is shown by the fact that tourmalines, and other crystals, which are pyro-electric when unsymmetrical, show no such property when symmetrically grown. Also sodium chlorate in solution, quartz when fused, and so on, lose their rotatory power. Substances which in solution show rotatory power, as a rule develop unsymmetric crystals. This is well seen in the tartrates. The constitution of the molecules must be such that they will not, without some strain, form crystals; and equilibrium, when the crystal is growing, is attained by means of the opposing stress due to want of symmetry in the surface-tensions. In all such crystals the rotatory power of the solution disappears in whole or in part. We cannot test this in biaxial crystals, but, according to Des Cloiseaux, sulphate of strychnine is the only substance which shows rotation both in the solution and in the crystalline form, and in it the rotatory power is much increased by the crystallization. Effects comparable with these may be produced by mechanical means. A cube of rock salt, which has no effect on plane-polarized light in its ordinary state, changes the plane of polarization when it is compressed in a vice. And a cleavage slice of prussiate of potash, which is uniaxial, may by compression be distorted so as to give in a convergent beam of polarized light elliptical rings, and two eyes like a biaxial crystal.

THE ERUPTION OF VESUVIUS OF JUNE 7, 1891.

DURING the latter part of 1890 and the early part of the present year, the central activity of Vesuvius has very slightly varied, except about the new year, when it was considerably increased, rising to the third or fourth degree, simultaneous with the stoppage of the lateral outflow of lava that had been going on since August 7, 1890. Since then, up to the present outburst, the central activity has been generally at the first degree, and the cone of eruption has slowly grown in height.

On June 1 there was a crater within the central eruptive cone, of about 50 m. in diameter, near the centre of which was the eruptive vent, surrounded by another embryonic eruptive cone. On that day, four small eruptive mouths opened around the embryonic cone in the bottom of the central crater, the smallest being to the east.

Thus the volcano remained till June 7, at 10 a.m., when activity stopped, only a small quantity of vapour escaping from central vents. At midday a radial cleft opened at the north toe of the cone of eruption (May 1889, June 1891) traversing towards its east end, the little sickle-shaped ridge, the remnant of the 1885–86 crater, but, as yet, gave out little vapour. At 4 to 4.30 p.m., shocks of earthquake commenced, limited only to the upper slopes of Vesuvius, and simultaneous with the extension of the radial fissure down the side of the great Vesuvian cone for nearly half its way opposite the Punta del Nasone of Monte Somma, from which, at about 5.30 p.m., issued a little lava, whilst from the upper extremity of the fissure at the toe of the cone of eruption much vapour escaped, so that from Naples the smoke-plume arose from this point. From 5.30 to 7 p.m. the fissure still extended lower, accompanied from time to time by local earthquakes, noises, and the elevation of columns of black dusty smoke. At a few minutes to 7 the floor of the Atrio del Cavallo was reached, and a remarkably black column of smoke had arisen.

My friend Dr. L. Sambon saw this column arise, and came to inform me immediately, as I had left off watching the mountain at 5.30. After taking a photo of the mountain, we left Naples at 9 p.m., spent some time in inquiries at Resina and near the Observatory. Everything was now dark, as the volcano had calmed down at 8 p.m. At 2 a.m., June 8, we were at the eastern extremity of the Observatory ridge, and commenced to wend our way across the lava surface towards Monte Somma. We were at the lowest part of the depression at the west end of the Atrio del Cavallo, where it joins the Fossa della Vetrana, and along which some of the largest lava-streams have flowed (1855, 1872, &c.), when suddenly on our right above us (2.23 a.m.) a vast quantity of bright red vapour arose from the new outpour of lava. We hastened our steps as much as the road and our lantern would allow us, so as to reach the escarpment of Monte Somma, the foot of which was followed till near the Punta del Nasone, and close to the theatre of eruption. Here we clambered up some distance above the level of the Atrio to watch events whilst we ate our late supper or early breakfast. Along the slope of the great cone in the line of fissure were a few luminous points from some pieces of still uncooled lava of the little that had oozed forth from the lower half of the fissure. At about 60 or 80 yards from the foot of the great cone two or three fountains of lava were throwing up jets of molten rock for 2 or 3 m., and the lava was slowly spreading out on the almost horizontal plain of the Atrio in several tongues. The lava must have still been high in the main chimney, as the vapour that issued at the top of the fissure showed a slightly red illumination. So we remained till daylight, when we could see the fissure on the side of the cone. The mouth that formed at 5.30 the previous day was still smoking a little, whilst the fissure below it sent off several ramifications at an acute angle like the branches of an inverted tree, from several of which little streams of lava had been given out, where they had soon consolidated. We now followed the base of the great cone to the lower railway station, where we found all the people up and dressed, frightened by the strong shock and noises at 2.23 a.m., coincident with the fresh outflow of lava that we had witnessed, but which shocks we had not felt, although they were described as the strongest that had been felt.

Having ascended to the summit of Vesuvius, we found the central crater rapidly enlarging by the falling in of its edges. From the new fissure at its summit was issuing much vapour under pressure, and rich in sulphurous acid, which is, even in traces, intolerable; and the hot air coming from innumerable new fissures rendered approach very difficult. We did, in fact, once jump across part of the fissure, but returned much quicker on account of the hot irritant vapours. An approach from the opposite side was equally unsuccessful. At some old fumaroles on the 1872 crater plain, I collected some crusts of boric acid and alum, both rare products at this volcano.

One of three terminations we may expect to these phenomena, which are very characteristic of a lateral disruption, so common at Vesuvius:—

(1) Should the lava cool sufficiently to plug the radial dyke, no further phenomena will occur, and activity will be restored to the central vent.

(2) If this plugging only partially takes place, lava may dribble forth for months, but probably the escape of vapour will soon be restored to the central vent.

(3) If the rent should widen, considering how low it extends, we may expect a grand eruption which might rival that of 1872, which commenced near the same spot and much in the same way; the mechanism by which this occurs I have explained elsewhere.[1]

My best thanks are due to Mr. L. Sambon for his company and help, and to Mr. E. Treiber, Inspecting Engineer of the Vesuvian Railway, for kind information.

Naples, June 9. H. J. JOHNSTON-LAVIS.

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[1] H. J. J. L., "The Relationship of the Structure of Igneous Rocks to the Conditions of their Formation," Scientific Proceedings R. Dublin Soc., vol. v., New Ser., pp. 112-56.

NOTES.

A LARGE and influential meeting was held at Edinburgh on Monday to consider the arrangements which ought to be made for the visit of the British Association to that city next year. The Lord Provost presided. On the motion of Sir William Turner the following were elected Vice-Presidents:—The Lord Provost, the Marquis of Lothian, the Earl of Rosebery, Lord Kingsburgh, Principal Sir William Muir, and Prof. Sir Douglas Maclagan. A local executive committee was chosen, and Mr. A. Gillies Smith was appointed honorary local treasurer. In a letter from Mr. Griffiths, secretary of the Association, it was stated that Sir Archibald Geikie, who will preside over the Edinburgh meeting, was in favour of the meeting being held early in August. A considerable majority, however, voted in support of a proposal that the meeting should begin on Wednesday, September 28.

ON July 28 and the three following days, at Bournemouth, the British Medical Association will hold its fifty-ninth annual meeting under the presidency of Dr. J. Roberts Thomson. The scientific business of the meeting will be conducted in nine sections. An address in medicine will be given by Dr. Lauder Brunton; an address in surgery by Prof. Chiene; and an address in public medicine by Dr. Cox Seaton.

A PHYSICAL Observatory, furnished with specially designed apparatus for the prosecution of investigations in radiant energy and other departments of telluric and astro-physics, has been established as a department of the Smithsonian Institution. The communication of new memoirs bearing in any way on such researches is requested, and for them it is hoped that proper return can be made in due time.

THE *Standard* understands that on the vote for the salary of the President of the Board of Trade, either Sir Henry Roscoe or Sir Lyon Playfair will call attention to the action of the Government with regard to the proposed Institute of Preventive Medicine.

THE Committee of the French Academy has decided, by five votes to four, that the prize of 20,000 francs should be given to M. Elisée Reclus, author of the well-known "Nouvelle Géographie Universelle." It is expected that the Academy will ratify the decision.

ACCORDING to a Reuter's telegram from Simla, dated June 12, Drs. Rake and Buckmaster have succeeded in cultivating the leprosy bacillus in serum. They were aided in their researches by Surgeon-Major Thomson.

IN reply to Mr. Bryce, in the House of Commons on Monday, the Lord Advocate stated that it would be the duty of the Government during the ensuing year not only to weigh very carefully the claims of secondary education in Scotland as one of the interests competing for a share of the additional Scotch grant, but also to prosecute further inquiries as to the means by which any grant available for that purpose might be usefully applied. Many proposals had already been submitted to and considered by the Scotch Education Department, and these, as well as any suggestions which might be made, would receive further careful consideration. The Government would also endeavour to bring all necessary statistics down to the latest date, so as to afford the necessary information for the solution of all branches of this difficult question.

THE funeral of Sir Richard Burton took place on Monday at the church of St. Mary Magdalene, Mortlake. The tomb represents an Arab tent, with a crucifix over the entrance. The interior is a small chapel with altar and some Oriental lights.

IT has been decided that a Geographical Society shall be formed at Liverpool. A preliminary committee has been appointed, and it has issued a circular stating the objects of the new body.

ACCORDING to a telegram sent through Reuter's Agency from Naples on June 16, the flow of the lava stream from Vesuvius had stopped, and Signor Palmieri, the Director of the Observatory on the mountain, had expressed his belief that the outflow might be regarded as at an end.

SLIGHT but continuous earthquake shocks were felt at Verona on June 10; and on the 11th, at 8.30 a.m., a very violent shock occurred at Tregnano and Badia Calavena. This was plainly felt in Verona also. Another violent shock occurred at Tregnano on the 13th, and on the 15th shocks were reported from Castelnuovo, Peschiera, Somma Campagna, and Desenzano.

THE first volume of a new meteorological Review has been published, containing observations taken in the south-west of Russia for the year 1890. This system was organized by Prof. A. Klossovsky in 1886, and now numbers nearly 600 observers. The observations refer chiefly to temperature, wind, rainfall, &c., for climatological and agricultural purposes. The Review also contains several articles of importance, *e.g.* (1) on phenological phenomena; (2) on the harvests in connection with meteorological observations; (3) on the movements of clouds; (4) actinometric observations made at Kieff. These are written in the Russian language only; the positions of the stations, and various data referred to in the text, are illustrated by maps and diagrams.

AT a meeting of the Royal Statistical Society, on Tuesday, a paper was read by Mr. Noel A. Humphreys, Secretary of the Census Office, on the results of the recent census and estimates of population in the largest English towns. The first part of the paper was devoted to the consideration of the recently-issued results of the census in April last in the twenty-eight large English towns dealt with in the Registrar-General's weekly returns. It was pointed out that, although the increase of population within the present boundaries of these towns showed an increase of nearly a million in the last ten years, the increase was less, by considerably more than half a million (605,318), than would have been the case if the rate of increase had been the same as in the preceding ten years, 1871–81; and that the rate of movement of population showed striking variations in the different towns. The rate of increase in these twenty-eight towns, it was stated, has pretty constantly declined in recent years, and has fallen with scarcely a break during the last five intercensal periods from 24·3 per cent. in 1841-51 to 11·0 per cent. in 1881-91. The percentage of increase within the boundaries of registration London (practically those of the county of London) declined in the same period from 21·2 to 10·4. The rate of actual decline of population in central London continues to increase, and the rate of increase of the other parts of the metropolis, including even the aggregate outer ring of suburban districts, continues to decline. Examined in detail, the provincial towns show, with few exceptions, the operation of similar laws; actual decrease in the central portions, and marked decline in the rate of increase in the other portions, the latter being specially noticeable in those towns with comparatively restricted areas. This examination, while showing the marked general decline in the rates of increase in these towns, discloses striking variations in the rates of increase in successive census periods. Mr. Humphreys called attention to the fact that these striking changes in the rates of movement of population in the large towns interpose the greatest difficulty in estimating, even approximately, their population in intercensal periods. The estimate of population in Liverpool, based upon the rate of increase between 1871 and 1881, exceeded the recently enumerated number by more than 100,000, or by 20 per cent.; while in Salford the percentage of over-estimate, by the same method, was 26 per cent. Thus the recent birth-rates and death-rates in these two towns have been under-estimated by no less than a fifth and a fourth, respectively. The various methods that have been at different times suggested for estimating the population of towns in intercensal years, in substitution of Dr. Farr's method, still used by the Registrar-General's Department, were severally considered, and it was shown that no hypothetical method yet devised affords reasonable promise of satisfactory results. It was therefore urged that a quinquennial census could alone supply a remedy for the present difficulty, which threatens to impair the public faith in death-rates, the failure of which would most seriously hinder and imperil the health progress of the country.

At the meeting of the Linnean Society of New South Wales, on April 29, Mr. T. W. Edgeworth David exhibited, on behalf of Mr. J. E. Carne, Mineralogist to the Department of Mines, Sydney, a specimen of precious opal from the White Cliffs about fifty miles northerly from Wilcannia. Precious opal and common opal have lately been discovered in this locality in a formation corresponding to the Desert Sandstone of Queensland. The opal occurs disseminated as an infiltrated cement throughout the mass of the sandstone in places, and also replacing the calcareous material of fossils. It also occurs in cracks in the sandstone and in fossil wood, which is somewhat plentifully distributed throughout the sandstone, and occasionally replaces part of the original woody tissues of the silicified trees.

Mrs. J. King van Rensselaer contributes to the Proceedings of the U.S. National Museum an interesting paper on the playing cards used in Japan. They are more distinctly original, she says, than any others, and show no marks of the common origin which the Italian, Spanish, German, French, Hindoo, and Chinese cards display. Forty-nine in number, they are divided into twelve suits of four cards in each suit. One card is a trifle smaller than the rest of the pack, and has a plain white face not embellished with any distinctive emblem, and this one is used as a "joker." The other cards are covered with designs that represent the twelve flowers or other things appropriate to the weeks of the year. Each card is distinct and different from its fellows, even if bearing the same emblem, and they can be easily distinguished and classified, not only by the symbolic flowers they bear, but also by a character or letter that marks nearly every card, and which seems to denote the vegetable that represents the months. The only month that has no floral emblem is August, and that suit is marked by mountains and warm-looking skies.

Prof. D'Arcy W. Thompson has edited an interesting volume of "Studies from the Museum of Zoology in University College, Dundee." The volume consists of the first twelve numbers of a journal in which the zoologists connected with the Dundee University College hope to find "an incentive to their own diligence, a way of communication with the outer world, and a means of giving direction and consecutive purpose to all their work." The editor contributes five papers, and the writers associated with him are Miss Mary L. Walker, Prof. H. Leboucq, Dr. H. St. John Brooks, Mr. Alexander Meek, and Prof. W. K. Parker.

An interesting illustration of the antagonistic action of poisons is mentioned in the current number of the *Pharmaceutical Journal*. Dr. Mueller, of Yackandandah, Victoria, has written a letter in which he states, says our contemporary, that in cases of snake bite he is using a solution of nitrate of strychnine in 240 parts of water mixed with a little glycerine. Twenty minims of this solution are injected in the usual manner of a hypodermic injection, and the frequency of repetition depends upon the symptoms being more or less threatening, say from 10 to 20 minutes. When all symptoms have disappeared, the first independent action of the strychnine is shown by slight muscular spasms, and then the injections must be discontinued unless after a time the snake poison reasserts itself. The quantity of strychnine required in some cases has amounted to a grain or more within a few hours. Both poisons are thoroughly antagonistic, and no hesitation need be felt in pushing the use of the drug to quantities that would be fatal in the absence of snake poison. Out of about 100 cases treated by this method, some of them at the point of death, there has been but one failure, and that arose from the injections being discontinued after 1¼ grain of strychnine had been injected. Any part of the body will do for the injections, but Dr. Mueller is in the habit of making them in the neighbourhood of the bitten part or directly upon it.

The Rev. J. Hoskyns-Abrahall writes to us that on June 10, about 10.30 p.m., near Woodstock, he saw what he describes as "a beautiful phenomenon." "Suddenly," he says, "at the zenith, east of the Great Bear, shone forth a yellow globe, like Venus at her brightest. Dropping somewhat slowly, it fell obliquely southward. As it passed in its brilliant career, it lighted up its dusky path with a glorious lustre. When it had descended about half-way down toward the horizon, it burst into a sparkling host of glowing fragments, each dazzlingly shot over with all the hues of the rainbow."

The Register of the Johns Hopkins University for 1890–91 has been issued. It contains a great mass of well-arranged facts relating to the work of that flourishing institution.

Mr. C. French, Government Entomologist at Melbourne, is contributing to the *Victoria Naturalist* a series of notes on the insectivorous birds of Victoria. In the first paper, which appears in the May number, he describes the Australian Bustard (*Choriotis australis*). Some months ago Mr. French made an appeal to the Victorian Government for the permanent protection of this, the most useful insect-destroying bird in the colony. His appeal was supported by the Council of the Zoological Society of Melbourne; and the Government has not only acceded to the request, but has placed the matter before the Government of New South Wales, who, it is hoped, will at once see the necessity for the preservation of so valuable a bird.

Dr. A. Koenig has issued as a separate volume the account of his ornithological observations made during his explorations in Madeira and the Canary Islands. It is a notable memoir, and several new species and sub-species of birds are described. He is somewhat severe on some British ornithologists for having tried to forestall him in the description of the Chaffinch of Palma, which he was the first to discover. The editor of the *Journal für Ornithologie*, in which the paper first appeared, also adds some strictures on the ways of British naturalists. Dr. Koenig apparently has some grounds for his complaint, but a *tu quoque* argument could be upheld against him, for he persists in calling a *Regulus* by his new name of *satelles*, though he admits that it is *Regulus teneriffæ* of Seebohm, and he does not refer to the British Museum "Catalogue of Birds," in which he will find that his identifications of the Madeiran and Canarian *Fringillæ* were all published long before he gave them to the world as new facts. These small matters do not, however, affect the importance of the essay, which is worked out with remarkable care, and is, in fact, a monographic review of the ornithology of Madeira, Teneriffe, and Palma. Eight coloured plates illustrate the article.

In a paper lately read before the Scientific Section of the Manchester Literary and Philosophical Society, Mr. John Watson maintains that the re-development of lost limbs is not unusual among insects. He himself has had three cases in which limbs have been re-developed, and one case of complete cicatrization. Re-development, he says, can take place either at the larval or the pupal stages of an insect's metamorphosis.

The British Consul at Hankow, writing of the varnish exported from that city, says he is informed that it is the gum of a tree—the *Rhus vernicifera*. On this tree, before daylight, incisions are made; the gum that runs out is collected in the dark, and strained through a cotton cloth bag, leaving behind a large amount of dirt and refuse. This operation can only be performed in the dark, as light spoils the gum and causes it to cake with all the dirt in it. It cannot be strained in wet weather, as moisture causes it to solidify. When the Chinese use this varnish, they rub it on with a sort of mop, or swab, made of soft waste silk. It should only be used in wet weather, as, if the atmosphere is dry when it is rubbed on, it will always be sticky. As used by the Chinese, the varnish takes about a month to dry, and during the time it is drying it is poisonous to the eyes. The Consul thinks that this gum may have been one of the ingredients of the celebrated Cremona varnish, and he suggests that it might be worth the while of musical instrument makers to make experiments with it with a view to producing a varnish that would give a mellow instead of a glassy sound.

The Insect-house in the Zoological Society's Gardens is now in excellent order, and well deserves a visit. In addition to the Silk-moths that are usually present during the warm weather, the Papilioninæ, or Swallow-tail butterflies, afford at the present time the chief display. The perfect insects of several species of the genus *Papilio* have appeared—*P. cresphontes*, *ajax*, and *asterias* from North America, *P. alexanor* from the Mediterranean shores, and the handsome *P. maackii* from Japan. The last-named has been seen for the first time in the house this year, and offers a striking contrast to the other species of the genus that have previously been exhibited in the Gardens, it being of black and golden-green colours instead of the yellows and blacks that we are accustomed to in our European Swallow-tails. *P. cresphontes* has appeared in large numbers in the house, but no varieties have been obtained. This also is the first season for two other beautiful Papilioninæ, viz. *Doritis apollina* from Asia Minor, and the Japanese *Sericina telamon.* The latter shows considerable difference in the markings of the sexes. The North American *Limenitis disippus* can be at present seen in all its stages, and is well worthy of attention, the caterpillar moving along the leaf-stalks with a peculiar interrupted gait. Of the Sphinx moths, the South European *Deilephila alecto* has already appeared, and *D. nicæ* is expected. These insects are, however, not seen to advantage in confinement, as their superb powers of flight cannot be displayed in a small compartment. Two examples of the Orthoptera are alive in the house—*Diaphemora femorata*, one of the Stick- or Twig-insects from North America, and *Empusa egena* from Southern Europe. The former has been reared from eggs laid in the Insect-house, but these progeny are not so healthy as those obtained from freshly-imported eggs. The *Empusa* is of a most bizarre form, and belongs to the family Mantidæ, the species of which feed only on living creatures. The public is indebted to Mr. S. H. Carver for the opportunity of seeing living scorpions; he has sent examples of two species of this group from Egypt, both of which unfortunately are unidentified, there being obvious difficulties in the way of carrying about live scorpions and comparing them with dried specimens. There is a third scorpion, from South Europe, living with its Egyptian congeners; it has a small delicate tail, and is altogether a less frightful creature, though assuming a menacing attitude with equal readiness. A spider, *Lycosa portosantana*, from Madeira, is healthy, and is a fine creature, though insignificant by the side of its neighbour, a huge *Mygale* from South America. The latter, as well as the scorpions, is fed with mice, which are given to it dead, though in its native haunts a *Mygale* has been known to prey on living individuals of these small mammals.

In the current number of the *Board of Trade Journal* some interesting facts as to cotton cultivation in Russian Turkestan are given, on the authority of a Russian correspondent of the *Monde Économique*. After the submission of the Khanates of Central Asia, the trade of the country was carried on chiefly with the towns of Russia in Europe, and was confined at first to the export in small quantities of cotton grown from native seeds, of rice, raw silk, and other similar products. It is only during the last ten years that the industry of the country has extended to any considerable degree, owing to the ingress of speculators, and has changed its primitive character. There have been established all kinds of works and factories, and in 1884 the cultivation of cotton of American origin was essayed. This trial succeeded so well that all classes of society, including even public officials, devoted themselves to this culture, which has become one of the chief branches of industry in the country. The new cotton produced in Central Asia is equal to that of America, and finds an excellent outlet among the cotton spinneries and mills of Russia. But the consumption in European Russia does not suffice for the ambitious aims of native producers, and they look forward to the possibility of opening up trade in the foreign markets of Europe.

The new number of the *Internationales Archiv für Ethnographie* fully maintains the reputation of this excellent periodical. Among the contents is a paper in which Dr. J. D. E. Schmeltz continues his elaborate account of the collections from Corea in the ethnographical museum at Leyden. Dr. Heinrich Schurtz has an interesting article on the geographical distribution of negro costume. As usual, the plates illustrating the various contributions are most carefully executed.

A further communication upon the new peroxide of sulphur, SO_4, by Prof. Traube, of Breslau, will be found in the current number of the *Berichte.* This interesting substance is obtained when solutions of sulphuric acid containing at least 40 per cent. of acid are subjected to electrolysis, as a crystalline deposit upon the anode. The crystals were first observed some time ago by Berthelot, but were considered by him as identical with the oxide S_2O_7, which he had previously obtained by the action of the silent electrical discharge upon a mixture of sulphur dioxide and oxygen. Prof. Traube, however, finds that the substance obtained at the anode in the electrolysis of 40 per cent. solutions of sulphuric acid is represented by the formula SO_4, and is quite a different substance from Berthelot's S_2O_7. It is, as predicted by

Mendeleeff, not the anhydride of an acid, but a neutral oxide of a similar chemical character to hydrogen peroxide. It may be best separated from the excess of 40 per cent. acid by removing the latter, after dilution with three times its volume of water, by means of freshly prepared barium phosphate. It cannot, however, be preserved in pure water, as it parts with oxygen so readily, becoming reduced thereby to ordinary sulphuric acid. That it is not an anhydride is proved by the fact that it yields no salts of the type K_2SO_5 with alkalies. Neutral solutions containing it, in which it appears to be permanent, may be readily prepared by neutralizing the solution in 40 per cent. acid with caustic soda, potash, or magnesia. The properties of SO_4 in either acid or neutral solution are somewhat remarkable. When boiled in contact with platinum wire or platinum black it is energetically decomposed with evolution of quantities of oxygen. If the neutral solution is employed, it becomes strongly acid. Indigo solution is oxidized and decolorized slowly, but instantly if a little ferrous sulphate is added. SO_4, however, in spite of this ready decomposition into oxygen and sulphuric anhydride, is but a weak oxidizing agent, being incapable even of oxidizing oxalic acid or carbon monoxide. But under certain circumstances it acts as a powerful reducing agent. For instance, if an emulsion of peroxide of lead in 40 per cent. sulphuric acid is brought in contact with a quantity of similar acid which has been subjected to electrolysis so as to charge it with SO_4, a rapid evolution of oxygen gas occurs, and the peroxide of lead is converted into ordinary sulphate of lead. In a similar manner precipitated peroxide of manganese is rapidly reduced to manganous sulphate with evolution of oxygen, and silver peroxide likewise dissolves up to a clear solution of silver sulphate with violent effervescence due to the escape of oxygen. Prof. Traube regards sulphur peroxide as built up on the type $SO_2(O_2)$, resembling hydrogen peroxide, H_2O_2. He considers that Berthelot's oxide, S_2O_7, is a molecular compound of SO_3 and SO_4, for it does not dissolve in water without decomposition, breaking up into sulphuric anhydride and oxygen, which is evolved. On the other hand, it appears, like SO_4, to be perfectly stable in a moderately concentrated solution of sulphuric acid.

The additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mr. James B. Leckie; a White-fronted Amazon (*Chrysotis leucocephala*) from Cuba, presented by Mrs. Lacabra; a Radiated Tortoise (*Testudo radiata*) from Madagascar, an Angulated Tortoise (*Chersina angulata*), three Smooth-bellied Snakes (*Homolosoma lutrix*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Green Lizard (*Lacerta viridis*) from France, presented by Mrs. Hill; three Horned Lizards (*Phrynosoma cornutum*) from Texas, presented by Mr. James E. Talmage; five Squirrel-like Phalangers (*Belideus sciureus* ♂ ♂ ♂ ♀ ♀) from Australia, a Grand Eclectus (*Eclectus roratus*) from Moluccas, deposited; two Elliot's Pheasants (*Phasianus ellioti* ♀ ♀) from China, two Rufous Tinamous (*Rhynchotis rufescens*) from Brazil, purchased; two Marbled Newts (*Molge marmorata*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

Newly-discovered Markings on Saturn.—*Edinburgh Circular* No. 16, issued by Dr. Copeland on June 10, contains the following information:—

Mr. A. Stanley Williams, of Burgess Hill, Sussex, has discovered three delicate but distinct markings in the equatorial region of Saturn. The first and third of these are round bright spots, somewhat brighter than the white equatorial zone in which they occur. The second is a smaller dark marking on the equatorial edge of the shaded belt which forms the southern boundary of the white zone. Mr. Williams has obtained abundant proof of the reality of these markings, but points out that it requires patience and practice to see them readily. It is very desirable to obtain repeated observations of their times of transit across the planet's central meridian. To facilitate these observations, Mr. Williams has prepared the following table, using 10h. 14·6m. as the provisional time in which the planet rotates on its axis:—

Approximate Greenwich Mean Time at which the Spots may be expected on Saturn's Central Meridian.

1891.	Spot 1 (white).		Spot 2 (dark).		Spot 3 (white).	
	h.	m.	h.	m.	h.	m.
June 20	7	50	8	47	10	9
21	4	20	5	17	6	39
22	11	5	12	2	13	24
23	7	32	8	29	9	51
24	4	2	4	59	6	21
25	10	47	11	44	13	6
26	7	14	8	11	9	33
27	3	44	4	41	6	3
28	10	29	11	26	12	48
29	6	56	7	53	9	15
30	3	26	4	23	5	45
July 1	10	11	11	8	12	30
2	6	38	7	35	8	57
3	3	8	4	5	5	27
4	9	53	10	50	12	12
5	6	20	7	17	8	39
6	2	50	3	47	5	9
7	9	35	10	32	11	54
8	6	2	6	59	8	21
9	2	32	3	29	4	51
10	9	17	10	14	11	36

The Rotation Period of Venus.—The *Bulletin de l'Académie Royale de Belgique*, No. 4, contains a paper, by M. Niesten, of Brussels Observatory, *à propos* the rotation of the planet Venus. The observations and drawings made by M. Stuyvaert and the author from 1881 to 1890 do not appear to confirm the persistence of the dark markings during a long period, as found by Schiaparelli and others. It is also shown that De Vico's period of 23h. 21m. 21·93s. is more in accordance with the observations than Schiaparelli's period of 224·7 days. Twelve drawings of the planet, and a map showing all the markings, accompany the paper.

A New Asteroid (310).—M. Charlois discovered the 310th minor planet on May 16. Its magnitude was 13.

THE ROYAL GEOGRAPHICAL SOCIETY.

The anniversary meeting of the Royal Geographical Society was held in the University of London on Monday afternoon, the President, Sir Mountstuart Grant-Duff, in the chair. The first business was the award of the medals and other honours for the year. The Founder's Medal was delivered to Sir Dillon Bell, Agent-General for New Zealand, for transmission to Sir James Hector, K.C.M.G., F.R.S., Director of the New Zealand Geological Survey. The Swedish Minister received the Patron's Medal on behalf of Dr. Fridtjof Nansen, who was unable to attend. Other honours were awarded to Mr. William Ogilvie, for his explorations of the Mackenzie and Yukon regions; Lieutenant B. L. Sclater, for instruments to be used in the exploration of Nyassaland; Mr. A. E. Pratt, for his journeys in Szechuen; Mr. W. J. Steains, for his investigations on the Rio Doce, South America. Mr. H. J. Mackinder then introduced the students of the Training Colleges who had been successful in obtaining the prizes offered by the Society annually on the results of the Christmas examinations in geography. Mr. Mackinder spoke briefly on the progress of geographical education, and on the results of the four years' awards to the Training Colleges.

The Secretary then read the annual report of the Society, from which it appears that on May 1 last the total number of Fellows was 3579, being a net increase of 84 on the previous year. The total income up to the end of December 1890 was £9531, and expenditure £8218. The estimated value of the Society's investments is £25,648, and of its total assets £46,248. During the past year, 900 books and pamphlets have been added to the library, and 936 sheets of maps to the map collection, besides 25 atlases, 700 photographs, 151 lantern-slides, and 51 views.

The President then proceeded to deliver the annual address on the progress of geography during the past year, dealing mainly with the explorations which have been carried on in various parts of the world.

"The year," he said, "of which I am about, with your permission, to give some account, has not been, so far as geographical discoveries are concerned, a very brilliant or sensational one. Brilliant and sensational years are, alas! likely to grow fewer and fewer as the globe we inhabit becomes ever better known to us. If, however, the year has not been made memorable by much *extensive* exploration it has put to its credit no small amount of *intensive* exploration. A good many gaps in our knowledge have been filled up, and a great deal of solid useful work accomplished. All this healthy activity has been represented in our Proceedings, and much of it has found its way to our Fellows through the papers which have been read in this theatre. Many of these have been extremely interesting. I may mention particularly the account of Messrs. Jackson and Gedge's journey to Uganda, Colonel Tanner's observations on the Himalayan Range, and Mr. Pratt's journey to Szechuen. These last were illustrated, as it will be remembered, by drawings and by photographs of exceptional merit, which were examined carefully by large numbers after our meetings closed. As you will have learnt from the report of the auditors, the total assets of the Society have considerably increased, and we are in a position to give most efficient assistance to any thoroughly well considered schemes which are laid before us. I am very sure, however, that the Fellows will consider that, although we are rich, it is none the less our duty to scrutinize carefully all proposals which are made to us, and to see that the money which they give so generously is applied only to really promising objects. Such we considered to be Mr. Ramsay's explorations in Asia Minor, and Mr. Theodore Bent's examination of the remarkable ruins at Zimbabye in South Africa. Instruments to the value of over £600 have been lent during the past year to intending travellers, and thirty-six gentlemen have received instruction from Mr. Coles, partly at the expense of the Society, for the purpose of making them more efficient as explorers. Our duties dividing themselves into two great classes—the acquisition of knowledge and the diffusion of knowledge—I think the Society will hail with pleasure a considerable increase of our expenditure under the head of 'Scientific Purposes,' which amounted for last year to nearly £600. That sum included £178 for the purpose just alluded to, £120 for the promotion of geographical education in connection with the Training Colleges, the University Local Examinations, and the Oxford University Extension Movement, and a contribution of £150 towards the salaries of each of the Geographical Lecturers at the Universities of Oxford and Cambridge. I am happy to be able to report that our efforts to promote geographical education in the first of these great national institutions are being crowned with success, thanks to the enlightened views now prevailing there, to the powerful assistance of the Warden of Merton and other friends in high place, and to the zeal and high intelligence of Mr. Mackinder, who is rapidly winning not only golden opinions for himself, but an excellent place for his science on the banks of the Isis. Negotiations are now in progress which will, I hope, result in the establishment of a Travelling Scholarship at the joint expense of our Society and of the University of Oxford. Our Fellows will, no doubt, have observed that efforts are being made to have the Ordnance Survey pushed on more rapidly than hitherto, as well as to make more generally accessible to the public the results of so much well-directed labour. They will approve, I feel sure, of the Society's assisting these efforts in all legitimate and reasonable ways."

The President then proceeded to review the exploring work of the year, most of which has already been dealt with in NATURE.

PARKA DECIPIENS.[1]

THIS very interesting fossil is derived from various localities in Scotland, all of which are believed to be Lower Devonian. It was first described in 1831 by Dr. Fleming, and since then has been noticed on several occasions, and variously regarded as the spawn of Mollusca or Crustaceans, and as of vegetable origin.

The material upon which the present observations are based was collected by Mr. James Reid[2] and Mr. Walter Graham, both of whom have offered many valuable suggestions as to the probable nature and affinities of the fossil. As found, the *Parka decipiens* usually consists of oval masses bearing rounded impressions or disk-like bodies of carbonaceous matter. Associated with these are also stems and linear leaves of two dimensions, and a third form having a general resemblance to Pachytheca, which is found in the same beds, and differing from it in having a more discoid form, and being devoid of structural markings.

The authors show that the fossil is probably a rhizocarp allied to *Pilularia*, and that there are at least three forms recognizable, of which one is referred to the species, and the other two to varieties. The views thus stated are based upon differences of size and upon the fact that certain of the disk bodies show spores of two kinds, and in some cases prothalli in various stages of development, all derived from the same sporocarp.

The paper is illustrated by a plate of figures.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—In the list of the Mathematical Tripos (Part II.) Mr. Bennett, of St. John's, the Senior Wrangler, Mr. Crawford, of King's, the fifth Wrangler, and Miss Philippa G. Fawcett, "above the Senior Wrangler," are placed in the first division of the First Class.

SCIENTIFIC SERIALS.

THE *American Meteorological Journal* for May contains the following articles:—Cold waves, by Prof. T. Russell. In the report of the Chief Signal Officer for 1889, he expressed the view that the origin of cold waves was due to mixture of upper and lower air causing cooling of the layer next to the ground. On further examination of the subject, in connection with the observations at mountain stations, he admits the incorrectness of those views, and states that, while it is essential to connect the low temperature and high pressure in some way, the cooling of the ground by radiation, and of the air by contact and conduction, will not completely explain the cause of cold waves.—How could the Weather Service best promote agriculture?, by M. W. Harrington. The American Weather Service has hitherto devoted itself more particularly to the interests of commerce, while the State Services have had the interests of farmers more distinctly in view. What the farmer wants to know is, where and when a local shower will fall. While the complete solution of this problem may be impossible, the approximate solution lies in the multiplication of local forecasting stations, and in the intelligent use of the indications of the Central Office, combined with the indications which he can himself observe. The author recommends more attention to climatology as distinct from weather changes, and to the relations between plants, soil, and meteorology.—Is the influenza spread by the wind?, by H. H. Hildebrandsson. This is a translation, by the author, from an article in the Journal of the Medical Society at Upsala, and is, practically, a reply to an article in NATURE of December 19, 1889, where it is stated that the malady is probably spread by the wind. The author shows, by a map and table, the places and dates at which influenza occurred in Sweden, from inquiries of medical men. The result of the research goes to show that the influenza is propagated by infection, that it is conducted from place to place through human circulation, and that the time of incubation is two to three days. The state of the weather seemed to have no influence on the spread of the malady; in fact, it raged with the same severity in countries possessing very different climates, and during very different weather conditions.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 4.—"On a Determination of the Mean Density of the Earth and the Gravitation Constant by means of the Common Balance." By J. H. Poynting, D.Sc., F.R.S., Professor of Physics, Mason College, Birmingham.

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[1] "Notes on Specimens from the Collections of Messrs. Graham and Reid," by Sir Wm. Dawson, LL.D., F.R.S., and D. P. Penhallow, B.Sc., F.R.S.C. Abstract of a paper read before the Royal Society of Canada, May 1891.

[2] Mr. Reid acknowledges his indebtedness to Mr. Langlands, the lessee of Myreton quarries, whose kind permission to examine these quarries was so freely granted.

In a paper printed in the Proceedings of the Royal Society, No. 190, 1878, an account was given of some experiments undertaken in order to test the possibility of using the common balance in place of the torsion balance in the Cavendish experiment. The success obtained seemed to justify the continuation of the work, and this paper contains an account of an experiment carried out with a large bullion balance, in place of the chemical balance used in the preliminary trials. The work has been carried out at the Mason College, Birmingham.

The Principle of the Experiment.—The immediate object of the experiment may be regarded as the determination of the attraction of one known mass on another. If two spheres, of masses M and M′, have their centres a distance d apart, the attraction is, according to the law of gravitation, GMM'/d^2, where G is the gravitation constant. Astronomy justifies the law in certain cases as regards M'/d^2, but does not give the value of G or M, except in the product GM. To find G we must measure GMM'/d^2 in some case in which both M and M′ are known. Having found G, we may determine the mean density of the earth, for, assuming that it is a sphere of radius R, the weight of any mass M′ at its surface is

$$G \times \tfrac{4}{3}\pi R^3 \Delta M'/R^2$$
$$= \tfrac{4}{3} G\pi R \Delta M'.$$

But if g is the acceleration of gravity the weight of M′ may be expressed as $M'g$. Equating these values, we get

$$\Delta = \tfrac{3}{4}\frac{g}{G\pi R}.$$

Method of Using the Common Balance.—With the length of beam used (about 123 cm.) a differential method was applicable, in which the attraction on the beam was eliminated. Two spherical masses of lead and antimony, about 21 kilos. each, were hung from the two arms of the balance, so that their centres in the first position were about 30 cm. above the centre of a large attracting mass, a sphere of lead and antimony about 153 kilos., placed on a turntable, so that it could be brought in turn immediately under either of the suspended attracted masses. A balancing mass of half the weight, and at double the distance from the centre of the turntable, was found necessary, so that the centre of gravity should be in the axis of rotation. Before this was used, the ground level was seriously altered by the rotation of the turntable. The attraction of the balancing mass was calculated and allowed for.

The alteration in the weights of the attracted masses, due to the motion of the attracting masses from one side to the other, was the quantity to be measured. When this was determined in the lower position of the attracted masses they were raised to about double the distance, and the attraction again determined. The difference eliminated the pull on the beam, suspending wires, &c. To lessen the effect of want of homogeneity or sphericity in the masses, or of want of symmetry in the turntable, the masses were all inverted and changed over each to the other side, and the weighings repeated.

The position of the beam was determined by the reflection of a scale in a mirror used with "double suspension." The mirror was suspended by two silk threads, one attached to the end of the ordinary pointer about 60 cm. below the central knife edge, the other parallel to it, being attached to a fixed support. The mirror turned through an angle about 150 times as great as that through which the beam turned, and one scale division corresponded to an angle of tilt in the beam of about 2/15ths of a second.

The value of a scale division was determined by the use of two equal riders which could be placed on or taken off wire frames representing the scale pans of a small subsidiary beam, 2·5 cm. long, fixed parallel to and at the centre of the large beam. When one rider was placed on one supporting frame the other was at the same instant lifted off the other frame.

The balance was left free throughout a series of weighings, and no moving parts of the apparatus were connected with the case.

The values obtained are as follows:—

$$\text{The gravitation constant } G = \frac{6{\cdot}6984}{10^8}.$$

$$\text{Mean density of the earth } \Delta = 5{\cdot}4934.$$

In the paper a description is given of a new form of cathetometer used to measure the diameters of the masses.

"Quadrant Electrometers." By W. E. Ayrton, F.R.S., J. Perry, F.R.S., and W. E. Sumpner, D.Sc.

In 1886 it was noticed, on continuously charging up the needle of Sir William Thomson's bifilar suspension quadrant electrometer No. 5, made by Messrs. White, of Glasgow, and in use at the laboratories at the Central Institution, that the deflection of the needle, when the same P.D. (potential difference) was maintained between the quadrants, instead of steadily increasing, first increased, and then diminished; so that, both for a large charge on the needle as well as for a small, the sensibility of the instrument was small. A similar effect had been described by Dr. J. Hopkinson, in the Proceedings of the Physical Society, vol. vii. Part 1, for the previous year, and the explanation he gives of this curious result is, that if the aluminium needle be below the centre of the quadrants, the downward attraction of the needle, which varies with the square of the needle's charge, increases the pull on the bifilar suspension, and so for high charges more than compensates for the increased deflecting couple due to electrical action. On raising, however, the needle of our electrometer much above the centre of the quadrants, the anomalous variation of sensibility of the instrument with increase of charge in the needle did not disappear; and even when the needle was raised so that it was very close to the top of the quadrants, and when, if Dr. Hopkinson's explanation were correct, the sensibility (or deflection corresponding with a given P.D. between the quadrants) ought to have been very great for a large charge on the needle, it was, on the contrary, found to be small.

The needle was carefully weighed, with the platinum wire attached and the weight dipping into the acid, and a calculation was made as to the magnitude of the effect that should arise from the change of the pull of the fibres due to any upward or downward attraction of the needle by the quadrants. This calculation showed that for a P.D. of 3000 volts between the needle and the quadrants, the amount of such attraction was quite unable to account for the observed diminution of sensibility with large charges in the needle. Dr. Hopkinson says in his paper, "Increased tension of the fibres from electrical attraction does not therefore account for the whole of the facts, although it does play the principal part." The experiments that we made at the end of 1886 and beginning of 1887, confirmed by the calculation above referred to, proved that, at any rate in our specimen of the quadrant electrometer, the principal part of the anomalous action was not caused by an increased tension of the fibres, and that therefore some other cause must be looked for to explain the observed results.

We therefore decided to make a complete investigation of the laws connecting the variation of the sensibility of the instrument with the potential of the needle, the distance between the fibres, the distance between the quadrants, and the position of the needle.

The results of the investigation, briefly summed up, are as follows:—

(1) The quadrant electrometer, as made by Messrs. White, although it may be carefully adjusted for symmetry, does not usually even approximately obey the recognized law for a quadrant electrometer when the potential of the needle is altered.

(2) The peculiarities in the behaviour of the White electrometer are due mainly to the electrical action between the guard tube and the needle, and to the slight tilting of the needle that occurs at high potentials.

(3) By special adjustments of the quadrants of the White electrometer, the sensibility can be made to be either nearly independent of the potential of the needle, or to be directly proportional to the potential, or to increase more rapidly than the potential of the needle.

(4) By altering the construction of the instrument, as described, the conventional law for the quadrant electrometer is obtained without any special adjustment of the quadrants beyond that for symmetry, and the instrument is rendered many times as sensitive as the specimen we possess of the White pattern.

Linnean Society, June 4.—Prof. Stewart, President, in the chair.—After nominating as Vice-Presidents Mr. A. W. Bennett, Dr. Braithwaite, Mr. F. Crisp, and Dr. St. G. Mivart, the President took occasion to refer to the loss which the Society had sustained by the recent death of a Vice-President, Prof. P. Martin Duncan, F.R.S. His genial presence at the meetings, no less than his valued contributions to the publications of the Society, would, he felt sure, be missed by everyone.—Sir Walter Sendall, who was present as a visitor, exhibited a curious cocoon of a moth belonging to the genus *Tinea*, and made some remarks on its construction and peculiar coloration.—The President exhibited a case of Lepidoptera and Coleoptera, which he had selected to illustrate some of the more notable secondary sexual characters in insects, and made some interesting explanatory observations.—Dr. John Lowe exhibited some eggs of *Mantis religiosa* which he had found adhering to the underside of stones on mountain-sides in the Riviera.—On behalf of Mr. F. J. Hanbury, Mr. W. H. Beeby exhibited and made remarks on a sterile form of *Ranunculus acris*, on which some criticism was offered by Prof. H. Marshall Ward.—A paper by Mr. M. C. Potter was read, on diseases of the leaf of the cocoa-nut tree. The specimens examined had been received from Ceylon through Dr. Trimen, and in Mr. Potter's opinion the diseases noticed were referable to three causes—namely, to the rays of the sun, to the ravages of insects, and to Fungi. These were separately considered, and descriptions were given of the different appearance which the leaves, thus variously affected, presented. A discussion followed, in which Prof. H. Marshall Ward criticized in some detail the observations which had reference chiefly to Fungi.—Two papers followed by Dr. P. H. Carpenter, on some Arctic *Comatulæ* and on some *Crinoidea* from Madeira, upon which Mr. W. Percy Sladen offered critical remarks.—The President then gave an abstract of a paper which he had prepared on a hermaphrodite mackerel, and exhibited the specimen on which his observations were founded, referring also to the recent cases of hermaphroditism in the trout and cod which had been brought to the notice of the Society. A commentary by Prof. G. B. Howes brought the proceedings to a close.

EDINBURGH.

Royal Society, May 18.—The Hon. Lord Maclaren, Vice-President, in the chair.—Dr. Buchan read a paper on the barometer at Ben Nevis Observatory, in relation to the direction and strength of the wind. In arranging the results, Dr. Buchan has referred the direction of the wind to sixteen points of the compass, although the observations are actually made with reference to the thirty-two points. The readings of the barometers at the high level and the low level stations, when reduced to sea-level, exhibit marked differences dependent upon the direction of the wind. The investigation extends over the period of nine months commencing in August last. During that time, all the very high winds have been from the east-south-east and the south-east, these being the directions in which the wind blows freely along the top of the mountain to the Observatory. In eleven cases the wind from these directions attained a speed of 120 miles an hour or more; and the (reduced) barometer at the high level station read about one-sixth of an inch lower than the instrument at the low level station. In no other direction was a higher velocity than 70 miles an hour noted; and in the directions from west to north-north-west, east, and east-north-east, the velocity was never greater than 30 miles an hour. With northerly winds the instruments at the top of the mountain record a much lower speed than that which, from observations of the drift of the clouds, is seen to be reached at a small height above the top of the mountain. The cause of this comparative calm immediately at the top is the impact of the air upon the face of the cliff which lies to the north of the Observatory. The stream lines are thus suddenly deflected upwards. In such cases the depression of the barometer is about three times as great as that which occurs with an equally strong wind from other directions, and indicates the formation of a region of low pressure around the Observatory. A peculiar result which is observed with other directions of the wind is that the (reduced) high level barometric reading exceeds the (reduced) low level reading when the wind blows at about the rate of 5 miles an hour. The reverse is always true when the speed of the wind exceeds that rate, on the one hand, or is extremely small, on the other. This seems to indicate an increase of pressure in air-currents which ascend the mountain, and so may explain the fact that the top of the mountain is frequently clear, while dense cloud is being constantly formed at a short distance above it.—Dr. J. Berry Haycraft gave an account of some experiments which show (1) that the displacements of the heart, which since Harvey's time are supposed to take place with every contraction, do not really occur in the unopened chest, and (2) that the cardiogram has been misinterpreted by physiologists. It is usually supposed that, during each contraction, the heart twists towards the right while its apex moves forward, and, pressing against the wall of the chest, causes the "apex beat." Again, it has been supposed by some that, during expansion, all diameters of the heart are not increased, but that, on the contrary, one diameter is diminished in length. Dr. Haycraft's experiments show that all diameters are increased during expansion, and that all are diminished during contraction. They show also that the motions, above described, do not occur in the unopened chest. The heart, in order that it may be observed in the opened chest, is necessarily separated from its attachments and falls towards the back of the chest (the animal operated upon being supposed to be placed upon its back). During expansion, the heart becomes flaccid, and so is flattened against the back of the chest. The first effect of the stiffening which occurs during the muscular contraction is therefore an elevation of the heart, against gravity, towards the front of the chest. Similarly, if the animal be turned upon one side, the heart, during contraction, moves towards the upper side of the chest; and the "beat" can even be made to take place towards the back. In the unopened chest, the heart on the whole remains in position during contraction, and therefore its boundaries move from the chest walls. But the cardiogram, as usually interpreted, shows that the chest wall is thrown outwards by the impact of the heart during contraction. Dr. Haycraft asserts that this is due to deformation of the heart by pressure of the chest wall when the button of the cardiograph is pressed against the exterior of the chest. The first effect of the muscular contraction and stiffening of the heart is therefore increased pressure against the chest-wall, which gives rise to the up-stroke of the cardiogram. When the cardiograph is made as light as possible, the up-stroke is greatly diminished; but it never entirely vanishes, because the flaccid heart is always slightly distorted by the chest-wall even when the cardiograph is not pressed against it. Dr. Haycraft further shows that the sinuosities, which always appear to a greater or less extent on the cardiogram, are not due to peculiarities in the action of the heart, but are instrumental in their origin, being caused by oscillations which result from the inertia of the cardiograph.—Dr. Hugh Robert Mill read a paper on the physical geography of the Clyde sea area, and the salinity and chemical composition of its waters. He described records, and discussed observations, made by himself and other members of the staff of the Scottish Marine Station. The observations dealt with extend over a period of three years, and their reduction has occupied, in addition, the greater part of two years. In the first part of the paper the author gives a detailed description, illustrated by a bathymetric chart, of the configuration of the Clyde sea area, with a special account of the various loch basins. The area and volume of each of these depressions are calculated, and the area of land which drains into each of them is measured on accurate maps. The rainfall is discussed in detail, and the river discharge is calculated indirectly, tables being drawn up to show the volume of rain water which flowed into each of the lochs during each month of the year. The month of maximum rainfall over most of the area is January, that of minimum rainfall is May. The whole sea area is conveniently divided into two parts—the seaward, of great extent, bordered with comparatively low ground, and lying in a region whose average rainfall is 44 inches; and the landward, made up of deep narrow loch basins, bordered by lofty mountains, and occupying a region whose average rainfall approaches 60 inches. In the latter part of the paper the positions of thirty-four stations (twenty-seven in the landward, and seven in the seaward division), at which observations were regularly made, are described. The method of collecting water samples, and the method of determining the density by means of a *Challenger*-type hydrometer, are given in detail. A record of 850 determinations of density made during twelve trips, which extended over two years, are given in an appendix. Twenty tables are given, which show the relations of salinity to configuration, tides, and rainfall, and which exhibit the relative amounts of pure sea-water and of fresh-water which were present in each of the divisions of the sea area at certain selected times. It was found that the amount of salt present in the water of the Clyde sea area varies with the season, the water being, as a rule, freshest in February, one month after the maximum rainfall, and saltest in July or August, two months after the minimum rainfall. The surface water exhibited the greatest changes, the seasonal variations being more regular at greater depths. Even at the head of lochs 50 or 60 miles distant from the open sea the percentage of pure sea-water present was rarely less than 88; the fresh river-water which poured in in enormous volume after heavy rain rapidly mixing with the sea-water, which was constantly renewed by the tide. So rapid and complete is this process of interchange, that the amount of river-water actually present diluting the water of the Clyde sea area is much less than the amount which passes through it every year, and is not equal to half of the average rainfall. In an average year 1·25 cubic miles of water, 97·5 per cent. of which is pure sea-water, and 2·5 per cent. fresh-water, enters the area at every tide; and a slightly greater amount is withdrawn, the whole being freshened a little so as to contain 2·7 per cent. of its volume of fresh-water. The great saltness of the deep water of the sea lochs, on which their importance as fishing-grounds depends, appears to be due to two causes. One of these is the thorough mixture of the tidal water from bottom to surface as it pours across the shallow bars at the mouths of the lochs. The saltest surface water was always found at flood-tide, off Otter Spit in Loch Fyne, where the salt water welled up from beneath in consequence of the rapid shoaling of the channel. Another cause of thorough mixture is the influence of the wind, which seems to set up a complete vertical circulation. Thus if wind is blowing strongly down Loch Fyne, the freshened surface water is driven out of the loch, and very salt water rises at the head of the loch to take its place. In a down-loch wind the surface water is almost always saltest at the head of the loch, and diminishes in salinity towards the open sea. The paper concludes with a summary of the chemical composition of the water.

Paris.

Academy of Sciences, June 8.—M. Duchartre in the chair.—On the currents which give rise to cyclones, by M. H. Faye. The views held by Dr. Hann and Prof. Ferrel concerning cyclones and anticyclones are compared. The author believes that cyclones, but not anticyclones, are dynamical phenomena, with which local circumstances of temperature have nothing to do, and he shows that they depend on the general movements of the atmosphere due to Polar cold and equatorial heat. On this point, therefore, M. Faye agrees with Dr. Hann.—Note on the presence of the *Kophobelemnon* in the waters of Banyuls, by M. H. de Lacaze-Duthiers.—The mastodon of Cherichira, by M. Albert Gaudry.—A new chemical balance for rapid weighings, by M. Victor Serrin.—Partial eclipse of the sun on June 6, observed at Nice, by M. Perrotin. With a power of 280, the time of first contact was observed to be 5h. 54m. 26s.; and of second contact, 6h. 53m. 26s. Nice mean time.—Observations of the new asteroid discovered at Nice Observatory on May 16, by M. Charlois. The observations are for May 16 and 25.—Observations of Brooks's comet (1890 II.), made with the great equatorial of Bordeaux Observatory, by MM. G. Rayet and L. Picart. Twenty-three observations for position were made between February 3 and April 29. The comet has been followed from March 27, 1890, to April 29, 1891.—On the theory of shooting-stars, by M. Callandreau. The author develops the equation of condition to be fulfilled by radiant-points belonging to the same family of meteors. According to Mr. Denning's observations, the Perseid radiant-point moves towards the east during the period of activity, a fact indicated as probable by Leverrier in 1871. This is in conformity with the equation of condition, which shows that if the latitude of a radiant-point varies slightly the longitude increases.—On two systems of differential equations, of which the hyperelliptic functions of the first order form the integrals, by M. F. Caspary.—Determination of the mechanical equivalent of heat, by M. Constantin Miculesco. The method adopted was similar in principle to that used by Joule. Thirty-one experiments made with this apparatus gave very accordant results, and the mean of them all give 426·7 as the mechanical equivalent of a calorie in kilogram-metres.—Dielectric properties of mica at high temperatures, by M. E. Bouty. The principal result of the research is that the dielectric constant is almost invariable for rapid alternations.—Application of the principle of the transmission of pressures to widely separated telephone transmitters, by M. P. Germain.—Action of ammonia on some compounds formed with halogen salts of mercury, by M. Raoul Varet. The author has studied the action of ammonia on compounds formed with mercury iodide and metallic cyanides, with the idea of determining the *rôle* of certain compounds of ammonia in double decompositions.—On a new method of preparing silicon chloro-iodides, by M. A. Besson.—On three cases of free development observed in Bryozoas ectoproctæ, by M. Henri Prouho.—On the locusts of Algeria, by M. Charles Brongniart.—On the morphological nature of the phenomena of fecundation, by M. Léon Guignard. It results from the observations that the phenomenon of fecundation consists not only in the copulation of two nuclei of different sexual origin, but also in the fusion of two protoplasms, also of different origin, and represented essentially by the directing spheres of the male and the female cell.—On the inclosures of nephelinic syenites found in the middle of phonolites from Höhgau and in some other beds; conclusions to be drawn from them, by M. A. Lacroix.—Observations of the parallelism of Upper Cretaceous strata of the Western Pyrenees (Lower Pyrenees and Landes), by M. Jean Seunes.—The sympathetic nerve of accommodation for the observation of distant objects, by MM. J. P. Morat and Maurice Doyon.—Researches on the existence of parasitic organisms in diseases of the crystalline lens of the eye of man, and on the possible *rôle* of these organisms in the pathology of certain ocular affections, by MM. Gallippe and L. Moreau.—On the employment of carbon bisulphide dissolved in water for the destruction of Phylloxera, by M. A. Rommier.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Glimpses of Nature: Dr. A. Wilson (Chatto).—Revelation of the Trinity: S. B. G. McKinney (Stock).—Oysters and all about Them, 2 vols.: J. R. Philpots (Richardson).—Die Veränderlichkeit der Temperatur in Österreich: J. Hann (Wien).—Monograph of the British Cicadæ, vol. ii. Part 6: G. B. Buckton (Macmillan).—A Guide-book to Books: edited by E. B. Sargant and B. Whishaw (Frowde).—Our Country's Flowers: W. J. Gordon (Day).—Primo Resoconto dei Risultati della Inchiesta Ornitologica in Italia; Parte Terza ed Ultima Notizie d'Indole Generale: E. H. Giglioli (Firenze).—Chambers's Encyclopædia, vol. vii. (Chambers).—Hand-book of the London Geological Field Glass: H. G. Seeley (Philip).—Teaching in Three Continents; W. C. Grasby (Cassell).—Bulletins de la Société d'Anthropologie de Paris, 4^{e} fasc. (Paris, Masson).—Journal of the Chemical Society, June (Gurney and Jackson).—Quarterly Journal of Microscopical Science, vol. xxxii. Part 3 (Churchill).

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THURSDAY, JUNE 25, 1891.

EDUCATIONAL ASPECTS OF FREE EDUCATION.

An innocent outsider would naturally suppose that the discussion on a proposal for free education would turn chiefly on educational and social considerations. So long as the question was of merely academic interest, this was, to a large extent, the case. It is true that strong Churchmen viewed with distaste a change which might increase the growing difficulty, found by voluntary school managers, of making both ends meet, or might possibly even sweep them off the board altogether, and that the enthusiasm of many partisans on the other side for the remission of fees was heightened by the hope that such a measure would give a new impetus to the formation of School Boards. But, on the whole, the disputants made at least an attempt in public to discuss the matter in its bearings on the child, the teacher, and the parent. The overburdened parent, the pauperizing effect of partial remission, the child kept from school because of his parents' poverty, the teachers converted into tax-collectors—these were the stage properties of the one party; while the stock-in-trade of the other side included the sacred necessity of guarding "parental responsibility," and the assertion that no one values what he does not pay for, and that to tax the hard-earned savings of the respectable middle-class to free the education of the children of the worthless and unthrifty was a Socialistic proposal of the crudest kind.

We now find that most of this talk was pure cant. It ceased to be heard from the moment when free education became a practical party question. To outward appearance the contest over the Bill has become a kind of Jerusalem race—everyone wishing to leave to someone else the unpleasant task of formulating the criticisms with which he secretly sympathizes, but to which fear of his constituents prevents him from giving utterance.

If we could induce the parties to break through this conspiracy of agreement, we should find that, with a few exceptions, the point on which the advocates feel most keenly is the possibility of using the Act as a lever either to destroy or to perpetuate for ever the voluntary school system. In spite of the apparent calm, the battle between the supporters of School Boards and voluntary schools is raging fiercely below the surface; and most of the amendments put down for the Committee stage are certain to represent attempts, more or less open or disguised, to wrest the provisions of the Act to suit the purposes of one or the other party.

It must be confessed that this is to a great extent natural. The Act of 1870 was a compromise: the present Bill virtually reopens the question, and it is felt that, whatever be the logic or want of logic in the argument that Imperial grants should involve local control, the time when large additional grants are being made to voluntary schools is the time, *if ever*, to drive home the question of popular management. We do not, then, quarrel with those who feel that the opportunity must not be lost of raising this question; indeed, we should respect them more if they raised it more openly. But we do protest against the almost total omission of all educational considerations in the arguments used on both sides.

It is time that the third party to the dispute—the real friends of education—made themselves heard. Their one object is to see that the educational benefits of the measure should be maximized, and the incidental evils minimized. They ask what is to be demanded in the shape of increased efficiency in return for a new grant of £2,000,000 to school managers. Is a great part of it to be allowed to be absorbed by the reduction of private subscriptions and rates, or is it to be used to improve the children's education, and make it a better preparation for their future industry?

In the rural districts, the grant in lieu of fees will almost universally be in excess of the income now received from fees. There will therefore be a surplus in the hands of the managers, or manager—for very often these schools are in the hands of one man. Where will this surplus go? In our opinion some method ought, if possible, to be found of "ear-marking" it for education rather than for subscribers' pockets. If this were done, nearly the whole of the rural schools of England might be raised in character. It would be possible, for example, to introduce, with the aid of the new surplus, some simple teaching in agricultural subjects, such as is recognized in the Code, but is at present a dead letter; for the increased grant would be quite enough to pay a competent travelling teacher to give such instruction in a group of schools. If there were universal county or district school authorities, it might be well to hand over the surplus grant into their hands, to be used solely for the improvement of the various schools on whose account it was paid. As, unfortunately, our organization is piecemeal, we are forced to deal direct with each school, and we can therefore only appeal to public-spirited managers to take care that the children for whose education they are responsible reap the full advantage of every penny which they receive over and above the present fees charged. It is to be feared, however, that in many cases the managers are at the mercy of their subscribers, and many of them would probably now welcome the proposal made by the Bishop of London, but foolishly rejected by his clerical friends on the late Royal Commission—that a certain minimum of private subscriptions should be required by law in the case of every voluntary school. If such a provision were in force, school managers in the country would be saved many anxious forebodings at the present time.

The second point in the Bill on which educational reformers should fix their attention is the limitation of the benefits to children between five and fourteen. The lower limit need not trouble us, and may be left to be worried by the "poor man's" numerous friends. But the upper limit should be resolutely opposed. It is quite true that at the present time it is of comparatively little importance—only affecting some few thousands of children. But if one of the great objects of educational policy is to lengthen the period of school life, the handful of children at elementary schools above fourteen should certainly not be fined for staying there; if anything, they should receive scholarships to enable them to do so. In our opinion, moreover, ex-seventh standard children (who are not for the most part touched by the present Bill) should be also admitted free, or at least sufficient scholarships should be provided to enable any poor child who has passed the standards to continue his education either in the school or elsewhere. We do not say that such scholarships should be universally provided out of the present grant, but they would be a most proper object to which to apply part of the surplus which will be handed to many schools over and above the fee equivalent. These considerations suggest another possible way of dealing with the surplus grants. The great object of those who are interested in the development of higher elementary, technical, and secondary education should be to strengthen instead of weakening the connection between primary and higher schools. It is to be feared that any provision for freeing elementary schools up to a certain point or a certain age, will tend to sever rather than to unite the two grades of schools, unless the flow between them is at the same time stimulated by the establishment of free scholarships or in other ways. A free (or partly free) elementary school is not the ultimate ideal. We want a free road kept open to the University. Is it too late to throw out the suggestion that school managers receiving a fee-grant in excess of the amount previously received in fees should be required to use the surplus for an object akin to that contemplated by the main provisions of the Bill—viz. the extension of free education for selected scholars beyond the narrow limits of the primary schools, in other words the provision of continuation scholarships? Up to a short time ago it would have been replied that in many cases there were no higher institutions accessible, but the application of the Local Taxation grant to technical and secondary education is fast changing all that, and a proposal which a few years since would have been unfeasible is now well within the range of practical politics.

DIFFERENTIAL AND INTEGRAL CALCULUS.

Differential and Integral Calculus, with Applications. By Alfred George Greenhill, M.A., F.R.S. Second Edition. (London: Macmillan and Co., 1891.)

PROF. GREENHILL is known to the academic world as an accomplished mathematician who has powerfully helped to advance certain branches of applied mathematics; he is also known to the readers of NATURE as a friend (militant) of the practical man. We say at once, in all sincerity, that we sympathize with Prof. Greenhill in both his capacities. The volume on the infinitesimal calculus now before us, although professedly a second edition, is in reality a new work, addressed to the special needs of the practical man by his mathematical friend Prof. Greenhill.

Of many of the author's didactic innovations we highly approve. The treatment of the differential and integral calculus together from the very beginning is a piece of sound method, the introduction of which has been delayed merely by the bad but not infrequent practice of separating the two as examination subjects. The introduction of the hyperbolic functions to systematize the integrations which can be performed by means of the elementary transcendents, has been, as we can testify from experience, a great help in elementary teaching. The admirable "chapter in the integral calculus" which was published separately in an extended form some years ago, and is now condensed and simplified in a separate chapter at the end of the work under review, is the most important addition to the teaching material of the integral calculus that has been made for a long time; that chapter alone is worth the price of Prof. Greenhill's book. The plan of drawing the illustrations of the subject from departments of pure and applied mathematics with which the learner may afterwards have to do is also excellent. Finally, there blows through our author's pages that inimitable freshness which emanates from the man who is familiar with much that is newest and best in his day, who does not merely make extracts from books, but who speaks of things in which he has taken a part. This freshness can only be compared to that agreeable odour which inland people tell us comes from mariners and others who cross the sea from strange lands. Like these same mariners, our author produces from his pockets strange and puzzling curiosities, such as reciprocants, tide predicters, Schwarzian derivatives, Mehler's functions, to delight and to dazzle the learner. It is true he tells but little of these things; still, it is pleasant to look at them; and they make us happy under our present toil by leading us to think that we too may one day visit the country where these pretty things are at home amidst their proper surroundings.

Where there is so much to praise we are truly sorry to insinuate the bitter drop of blame; but, much as we love and follow Plato, something must be conceded to truth. In the first place, we think that in this second edition the introduction of heterogeneous illustration has been overdone. The fundamental rules of the infinitesimal calculus are really very few in number, and the practical man's friend would do well to impress that upon him at the outset, instead of scattering these principles through a large volume, and overlaying them with thick masses of disconnected application, to such an extent that poor Mr. Practical-Man is in danger of losing his tools among the shavings, or, to use a metaphor which Prof. Greenhill's pupils might prefer, of not seeing his guns for smoke. Prof. Greenhill must recollect that the man that sits down to read his book is not all possible practical men rolled into one, but *one* poor practical man—say, an engineer—who wants some knowledge of the infinitesimal calculus, and who will find many of the illustrations more indigestible than the principles of the calculus itself. Would it not be better for the practical man, as well as for any other man, to have the few leading principles of the calculus set before him with an adequate but moderate amount of illustration of a uniform geometrical kind, and not to be dazed by a flood of oracular statements about soap-bubble films, tide-predicters, &c., in the course of his initiation? Such digressions are most useful now and then in a lecture; they serve to give picturesqueness to the discourse, and help to fix the attention of the hearer: but we think that too many of them destroy the usefulness of a text-book, the object of which is quite different from the purpose of a lecture.

The matter we have just been criticizing may, perhaps, be held to be one of taste; and we cheerfully admit that much should be allowed to a writer of strong individuality. After all, we love to have the author in his book. There is another matter, of more importance, on which we would appeal to Prof. Greenhill. When a man, so able and unconventional as he, writes a book of 455 pages on the infinitesimal calculus, is it too much to expect that he will everywhere give a thorough discussion of its few fundamental principles, that he will rigorously prove what he professes to demonstrate, and honestly point out what he assumes without demonstration? We certainly expect him to root out of the subject every trace of the sham demonstration—that wily artifice of the coaching and examining days of our dear old *alma mater*—which used sometimes to be dignified by the name of the "short proof." This used, to be employed when we had on hand the establishment of some proposition which was not universally true (although usually so enunciated), or which had exceptions too tedious to enumerate in an examination. The method was to make a kind of *précis* containing as few words of intelligible English as possible, but a considerable sprinkling of ingeniously constructed but unexplained symbols and formulæ; so that an examiner of average conscience, suspecting that the truth was not there, might nevertheless, without mental distress, make believe that it *was* there, and award the coveted marks.

We complain that Prof. Greenhill should countenance the slipshod exposition of elementary principles which is the bad feature of so many of our English mathematical text-books. Having started his furrow, he should have ploughed to the end. He may retort that he has adhered to the traditional usage out of consideration for the weakness of the practical man, who abhors sound logic quite as much as his academic brother. Cruel consideration for the practical man! for what *he* wants above all is a firm grasp of the fundamental principles of the calculus; he has rarely any use for the analytical house of cards, composed of complicated and curious formulæ, which the academic tyro builds with such zest upon a slippery foundation.

It would take up too much of the columns of NATURE to give all the examples that might be adduced of the laxity we complain of. A few must suffice. We are told in § 1 that the "calculus to be developed is the method of reasoning applicable to variable quantities in a state of continuous change;" yet no definition or discussion of "continuity" is given: the word, so far as we can find, does not occur again in the first chapter, although it is the keynote of the subject. "Newton's microscope," for example, is quoted in § 9, as a proof of the theorem $Lt(\text{chord}/\text{arc}) = 1$; but the essential condition, "in medio curvaturæ continuæ," which makes it a proof (if proof be the word that describes its purpose) is omitted. Although the differential calculus is merely a piece of machinery for calculating, and calculating with limiting values, a limiting value is not defined; nor is there any discussion of the algebra of limiting values—a matter which has puzzled beginners in all ages, and which has stopped many on the threshold of the calculus. It is true that we are referred to Hall and Knight's "Algebra," but what we find there is little to the purpose, and certainly could never have been meant by its authors as a foundation for the differential calculus.

In § 16 we are given a quantity of elementary instruction, in the middle of which the trigonometrical functions are inadequately defined; but nothing adequate is said regarding the sense in which the many-valued functions $\sin^{-1}x$, $\cos^{-1}x$, &c., are continuous: and in § 25 the beginner is led by implication to believe that $d(\sin^{-1}x)/dx$ is always $+ 1/\sqrt{(1 - x^2)}$, and $d(\cos^{-1}x)/dx$ always $- 1/\sqrt{(1 - x^2)}$; although this is not so, and the point is one that is of the greatest importance in the integral calculus, and is a standing rock of offence for learners. In § 28 we have, reproduced "for the sake of completeness," the time-honoured "short proof" of the existence of the exponential limit, which proof is half the real proof *plus a suggestio falsi*. If the proper proof (a very simple matter) was thought too much for the reader, then it would have been better simply to tell him the fact, and not to corrupt his intellectual honesty by demanding his assent to a piece of reasoning which is not conclusive. § 31 is no better; what, for instance, *does* Prof. Greenhill mean, after proving that $\exp n = e^n$, where n is a positive integer, by saying, "and thence generally by induction, $\exp x = e^x$ for all values of x." It would scarcely be possible to write down a statement to which more exceptions could be taken unless "induction" is a misprint for "assumption."

The chapter on the expansion of functions is not satisfactory. We are first introduced to "a general theorem called *Taylor's theorem*, by means of which any function whatever can be expanded [in ascending powers of x]." Prof. Greenhill knows as well as we that there is no such theorem. No theorem ever to be discovered will expand in ascending powers of x, $1/x$, $\sqrt{x}$, $\log x$, or any function which has $x = 0$ for a critical point. Why does our author hide his light from the reader? Does it make the apprehension of Taylor's theorem any easier to enunciate it falsely? We are told in § 114 that "some functions, for instance $\sec^{-1}x$, ... cannot be expanded in an infinite series in ascending powers of x, because x must be greater than unity, and the expansion by Taylor's or Maclaurin's theorem would be *divergent*, and the theorem is then said to fail."

"This difficulty will be avoided if we can make the series terminate after a finite number of terms."

We would not advise the practical man to try to overcome the difficulty of expanding $\sec^{-1}x$ by the method thus indicated (use of Maclaurin's theorem with the remainder), because the result might be that the bond of amity struck in the preface between him and the author would be broken. All the king's horses and all the king's men will not get over this difficulty. Incidentally we are told in § 112 that a rigorous proof is given in treatises on trigonometry of the resolution into factors of $\sin\theta$ and $\cos\theta$. If standard English treatises, such as Todhunter, Locke, and Johnson, are meant, this is not true: the demonstrations they give are unsound. Mr. Hobson's article on trigonometry in the "Encyclopædia Britannica" is the only separate English treatise on trigonometry of which we are aware where a sound proof can be found.

When so many novelties of less importance are noticed, surely our author might have found a place for a reference to the theorem that puts the expansibility of a function in ascending powers of x in its true position, viz. Cauchy's theorem that every function is so expansible within a certain region surrounding $x = 0$, provided $x = 0$ be not a critical value. Considering the great importance of the fact, and its close connection with the applications of mathematics to physical problems, some mention might have been made of the importance of the critical points of a function in determining its value. A full discussion of such things is doubtless impossible in an elementary treatise; but the reader should at least be warned that what is given regarding the expansion of functions in power-series is a mere fragment of what is known on the subject. The tendency of Prof. Greenhill's chapter on the expansion of functions certainly will be to suggest to the mind of a beginner wrong general notions on the subject.

In § 126 we have two proofs given that

$$\partial^2 f(x, y)/\partial x \partial y = \partial^2 f(x, y)/\partial y \partial x,$$

both of them insufficient; for the one rests on the assumption that $f(x + h, y + k)$ can always be expanded in an integral h-k-power-series, the other on the assumption that

$$\underset{h=0}{\mathrm{L}} \ \underset{k=0}{\mathrm{L}} \chi(h, k) = \underset{k=0}{\mathrm{L}} \ \underset{h=0}{\mathrm{L}} \chi(h, k),$$

both of which propositions are liable to exception.

In the discussion of single and double integrals, no hint is allowed to reach the reader of the necessity of convergency as a condition of their having any meaning at all, of the precautions that must be observed in differentiating them, or in altering the order of integration, and so on. Still, the reader is given a proof of Green's theorem. What use this is likely to be to one ignorant of the fundamental character of the convergency and discontinuities of multiple integrals, upon which many of the most important applications of the theorem in question depend, it is not easy to see. Too much of the work before us bears, in fact, the character of a hurriedly written *précis* or syllabus of lectures; witness, for example, the oracular character of §§ 146, 151, 152, &c. Our author makes enormous demands on the intelligence of a beginner if he expects him to follow and understand exposition so elliptical.

One more example of the thing we complain of. In § 183 we are introduced to Fourier's series. No proof is given (none was to be expected in an elementary treatise) of the conditions under which the expansion is possible, but it ought to have been stated that there are such conditions. Moreover, the method given for the determination of the coefficients is a mere *memoria technica* for recollecting them. It has no demonstrative force, because, as the author must be very well aware, it is not unconditionally allowable to replace the integral of an infinite series (even if it be convergent) by the sum of the integrals of its separate terms. In order that this may be admissible, the series must be *uniformly* convergent.

Seeing that the world is very evil, and not to be mended in a day, we must put up with such things in the ordinary writer of English text-books, who caters for the victims of our manifold examinations; but in a pillar of mathematical society like Prof. Greenhill they are "most tolerable and not to be endured." A work with his name on its back, and the impress of his vigorous personality on its pages, will not remain long in a second edition. If he would be at once the friend of the practical man, and a well-deserver of the mathematical republic, let him, when the third edition is called for, reduce his elementary work to the compass of the first edition or less, and replace all half demonstrations by honest statements of fact; and let him, meantime, write a larger work, to which he can refer the elementary reader who takes for his motto, "*Thorough.*"

G. C.

THE GEOLOGY OF THE COUNTRY ROUND LIVERPOOL.

Geology of the Country around Liverpool. By G. H. Morton, F.G.S. Second Edition. (London: Philip and Son, 1891.)

IN this work Mr. Morton has entirely re-written the "Geology of the Liverpool District," first published in 1863, by the light of the various discoveries made since that time, and especially of the Geological Survey maps and memoirs. He has succeeded in making a compact and well-printed hand-book, which will be of great service to the students of the local geology. The area described extends to about 20 miles from Liverpool on every side, excepting the sea on the west. The strata which he describes range from the Upper Silurians of the Vale of Clwyd through the Carboniferous, Permian, and Triassic rocks, down to the recent alluvia. To a geologist the chapter relating to the Carboniferous rocks of North Flintshire and the Vale of Clwyd will be of great interest, as it shows the thinning off of the strata as they approach the ancient Carboniferous land of North Wales. The Carboniferous Limestone, over 3000 feet thick in North Lancashire, is reduced to 1700 feet in North Flint and the Vale of Clwyd; while the Yoredales and Millstone Grits, over 9000 feet thick between Clitheroe and Burnley, are represented by the Cefn-y-Fedw Sandstone, 370 feet. The Lower and Middle Coal-measures, too, of South-West Lancashire, 3180 feet thick, have dwindled down to no more than 1000 feet as they approached the Welsh Silurian Hills. It is therefore obvious that the Snowdonian area was dry land while the Carboniferous sea occupied the areas of Lancashire, Derbyshire, and Cheshire, and that it also overlooked the forest-covered morasses, now represented by the coal-seams of the same region in the Upper Carboniferous age. In the table of the rocks (p. 6) Mr. Morton gives 300 feet as the thickness of the Millstone Grit in South-West Lancashire. It is probably much more than this, and not much less than 2000 feet. Mr. Morton also, we may remark, understates the thickness of the Keuper Marls, which he puts down at 400 feet (p. 75). In the Lancashire and Cheshire plain it is 700 + feet, and is estimated by Prof. Hull at 3000 feet.

Mr. Morton, in dealing with the deep boring at Bootle, made in 1878, under the advice of the writer of this review, is mistaken in supposing that it was aimed at the water in the Permian Sandstone. It was intended to strike the water in the Lower Bunter Sandstones, and to draw upon the enormous area of water-bearing strata in the Lancashire and Cheshire plain, which have their outlet seawards between Prescot and the estuary of the Dee. It is very likely that the Permians are not represented under Liverpool. We expected to strike the Coal-measures at 1000 feet. The boring was successful, both from the geological and the engineering point of view. It proved that the Lower Bunter Sandstones below the top of the Upper Pebble-beds are more than 1300 feet thick, and that they are highly charged with water. This thickness is altogether without precedent, and Liverpool is to be congratulated upon being built upon so great a thickness of water-bearing Triassic rocks. Mr. Morton, should the work reach another edition, would do well to deal at greater length with the water-supply available from the Triassic strata. Mr. Boult has tabulated the well-sections, and all students of the geology of Liverpool would do well to examine his valuable tables.

We would call special attention to Mr. Morton's section—unfortunately, the work is not divided into chapters—on the origin of the estuary of the Mersey. While the river has been draining its present watershed from a period far more remote than the Pleistocene age, he holds that the estuarine portion is comparatively modern, dating probably not further back than post-Roman times. It would not, he argues, following Sir James Picton, have been neglected by the Romans, if it had then "presented the copious body of water which it does at the present day." There is no evidence that they did neglect it. The Manchester Ship Canal works have revealed the existence of Roman remains, probably the Veratinum of the anonymous geographer of Ravenna, on the banks of the Mersey close to Warrington, and Mancunium (Manchester) is on one of its tributaries. They used it, as they used all the rivers of Britain, for their own ends. Deva (Chester), the great port, and military centre of the north-west, was not far off, and amply sufficient for the western trade at a time when there were no ports in Ireland. The commercial importance of the Mersey is solely due to the trade with the New World. There was no reason why the Romans should have paid special attention to the estuary of the Mersey; and it was outside the system of their roads. Nor can the date, 1279, of the great inroad of the sea over the Stanlow Marshes, by which the Abbey of Stanlow, built upon a rock only 28·5 feet above O.D., lost much of its land, be taken as evidence of the modern formation of the estuary. The river swings to and fro at the present time, depositing silt here, and carrying away its banks there. In our opinion, therefore, the post-Roman origin of the Mersey is not proved. It is still less likely that it is the result of a local submergence, which has not affected Warrington and the adjacent area of Chester. As the evidence stands, the date of the estuary of the Mersey belongs to the same remote prehistoric period as the estuary of the Thames and of the Humber—certainly after the time of the boulder clays, and probably long before there were any written records in Britain. All three are later than the time of the submarine forest which, on the west of Britain, afforded shelter, not merely to our Neolithic ancestors, but to their domestic animals, such as the small shorthorn (*Bos longifrons*), the goat, and the dog.

W. Boyd Dawkins.

OUR BOOK SHELF.

Les Microbes, les Ferments, et ses Moisissures. Par le Dr. E. L. Trouessart. Deuxième Edition. Bibliothèque Scientifique Internationale. (Paris, 1891.)

This is not only an enlargement but a distinct improvement on the first edition. Chapters i. and ii., as in the first edition, give an excellent though short account of the morphology and physiology of fungi and of yeast. Although chapter iii. (on bacteria) is enlarged, we do not think it is sufficiently up to date; thus, for instance, on pp. 74 and 75, the author questions the existence of true flagella in bacteria, and states that their motility is essentially different from that of flagellate infusoria. Again, in the section in which putrid decomposition is described no mention is made of the entire tribe of Proteus, the essential microbe of putrefaction.

Chapters iv. and v. (pathogenic bacteria) are considerably enlarged, both as to text and illustrations. The rest of the book, chapters vi.–ix., does not differ in any essential respect from its predecessor.

On the whole, the book is very commendable as a concise text-book, well written and copiously illustrated, and as such deserves a high place in the literature of the subject.

Botanical Wall Diagrams. Size 31½ inches by 24 inches, printed in colours. (London: Society for Promoting Christian Knowledge, 1891.)

A first instalment of six of these diagrams is now published. The plants illustrated so far are: common elder, deadly nightshade, scarlet runner, hop, Virginia tobacco, and wild camomile. We do not know on what principle the selection has been made. It is rather a pity that, out of so small a number, two (deadly nightshade and tobacco) belong to the same natural order, and show no very essential structural differences. In time we hope that all the important orders will be represented. The drawings (executed by Engleder, of Munich) are quite artistic, and the colouring excellent. The diagrams are thus very pleasing as pictures, and at the same time the botanical details are correct.

If the series is continued as well as it has been begun, it ought to be a very useful help in the elementary systematic teaching of botany.

D. H. S.

Chambers's Encyclopædia. New Edition. Vol. VII. (London and Edinburgh: W. and R. Chambers, Limited, 1891.)

No one who has had occasion to refer to the new edition of Chambers's "Encyclopædia" can have failed to appreciate the care and ability with which it is being prepared. The editor has been fortunate enough to secure the co-operation of many eminent writers, and the information given in the various articles, speaking generally, is well up to date and presented in the way most likely to be convenient for students. We are here concerned only with the papers on scientific subjects, and these, in the present as in the preceding volumes, are in every way worthy of the place which has been assigned to them in the scheme of the work as a whole. Prof. P. G. Tait contributes a short but masterly paper on matter, and Dr. Buchan gives a clear and interesting account of meteorology. The essential facts about the Mediterranean are compressed into very small space by Dr. John Murray, who also writes on the Pacific. Prof. James Geikie deals with mountains and palæontology, and Dr. Alfred Daniell has a good popular article on optics, devoted mainly to the history of optical science. In an article on man, Mr. J. Arthur Thomson states very well some of the problems relating to human characteristics, the origin or descent of man, and the antiquity of the race; and the same writer sketches the career of Pasteur, and treats of mammals and parasites. Mimicry forms the subject of an excellent paper by Mr. E. B. Poulton. Of course, no subject is treated exhaustively, but the information given, so far as it goes, is sound, and ample enough for the purposes for which an encyclopædia is usually consulted.

Glimpses of Nature. By Andrew Wilson. (London: Chatto and Windus, 1891.)

Mr. Wilson does not profess to present in this book anything strictly new, or to give a full account of the various subjects with which he deals. Nevertheless, the volume may be of considerable value, for on all the groups of facts in which he is interested he is able to discourse brightly and pleasantly, and many of his short papers are well calculated to excite in the minds of intelligent readers a desire for more ample knowledge. The papers are reprinted from the *Illustrated London News*.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

The Fusing and Boiling Points of Compounds.

I herewith send you the translation of a note just presented for me by M. Berthelot to the Paris Academy, as you may see in the *Comptes rendus.* I have added two illustrations and a few words in italics. Gustavus Hinrichs.

St. Louis, May 8.

Statement of the General Law determining the Fusing and Boiling Points of any Compound under any Pressure, as Simple Function of the Chemical Constitution of the same. By Dr. Gustavus Hinrichs.

The atomic form of normal lineary compounds, such as the paraffins, alcohols, acids, is very nearly prismatic. All other serial compounds may be referred to these, either as isomerics or as substitution products. The boiling and fusing points of these latter are obtained from those of the former according to laws and processes published by me about twenty years ago, partly in my "Principles of Molecular Mechanics," 1874, and in Notes of the *Comptes rendus* for 1873 and 1875; *partly in papers of the Proceedings of the American Association for the Advancement of Science for* 1868. It remains, therefore, only to show how these fundamental points are determined for prismatic compounds.

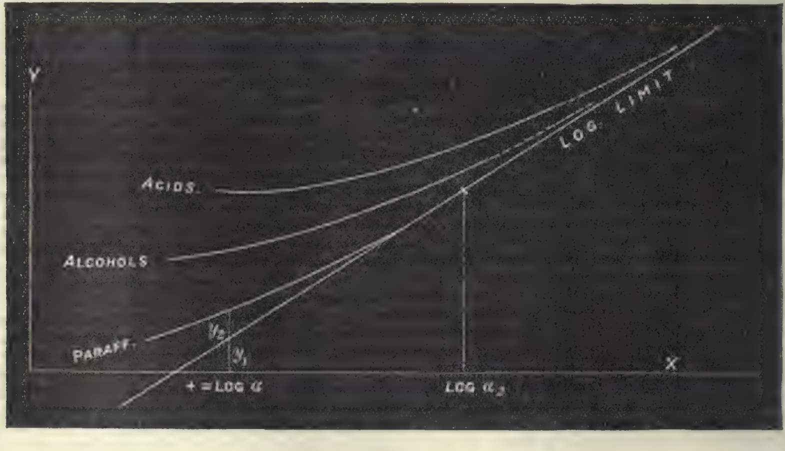

Fig. 1.

The boiling-point t of a prismatic compound consists of two distinct functions, namely—

$$t = y_1 + y_2 \quad . \quad . \quad . \quad . \quad (1)$$

where

$$y_1 = k_1(\log a - \log \alpha_1) \quad . \quad . \quad . \quad . \quad (2)$$

and

$$y_2 = k_2(\log \alpha_2 - \log a)^2 \quad . \quad . \quad . \quad . \quad (3)$$

The symbols α_1 and α_2 represent certain definite values of the atomic weight a of the compound, while k_1 and k_2 are constants.

For every value of the atomic weight a greater than α_2 the formula (1) is limited to $t = y_1$, which, according to (2), represents the straight line which I call the logarithmic limit, the ordinate being the boiling-point t, the abscissa x, the logarithm of the atomic weight $x = \log a$. For values of a less than the above limit α_2, the parabolic ordinate y_2, determined by (3), must be added to y_1, according to (1), in order to obtain the boiling-point.

Accordingly, the boiling-point curve of any homologous series of prismatic atom-form consists of a parabolic arc (3), tangent to the logarithmic limit (2), at the point determined by $a = \alpha_2$. The constant k_1 determines the inclination of the logarithmic limit, and k_2 may be called the parameter of the parabolic branch.

All compounds derivable by terminal substitution from normal paraffins have a common logarithmic limit, determined by $k_1 = 583°.75$ and $\alpha_1 = 72.78$, the pressure being 760 mm. Every individual homologous series of this great family of compounds is completely determined by the special values of the two constants α_2 and k_2. For example, the thirty-five normal paraffins C_nH_{2n+2} are determined by $\alpha_2 = 201$, and the parameter $k_2 = 200°$. For the monamines, the corresponding values are $\alpha_2 = 278$, and $k_2 = 225°$. I have determined these constants for all the important series. Furthermore, these values are themselves functions of the atom or radical which characterizes the head of the corresponding homologous series—*that is, H for the paraffins, H_2N for monomines, &c.*

If now the co-ordinate $z = \log p$, where p is the pressure of the saturated vapours, be laid off on the third rectangular axis the above given values belong to the plane XY determined by $p = 760$ mm. For the pressure $p = 15$ mm. the logarithmic limit is determined by $k_1 = 517°.0$, and $\alpha_1 = 113.81$. It will be noticed that its inclination towards the X axis is less, and that it intersects the same at a greater distance from the origin. The logarithmic limit surface, generated by the logarithmic limits for all pressures, is a hyperbolic paraboloid, fully determined by the above two lines for 15 and 760 mm. pressure.

For any liquid, the absolute temperature T of the boiling under a pressure of p atmospheres is determined by the same general law slightly specialized as follows:—

$$T = Y_1 + Y_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where

$$Y_1 = K_1[1{\cdot}4 + \log p] \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

and

$$Y_2 = K_2[\log \pi - \log p]^2 \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The logarithmic limits of all liquids intersect in the same absolute zero point determined by $T = 0 = -273°$ C. and $\log p = -1{\cdot}4$. For each individual liquid this limit extends upwards to the critical point of the liquid, $p = \pi$ and $T = \theta$. For many liquids the critical point can be theoretically calculated, as well as the value of the parameter. It is understood that the parabolic curve is tangent to the logarithmic limit at the critical point.

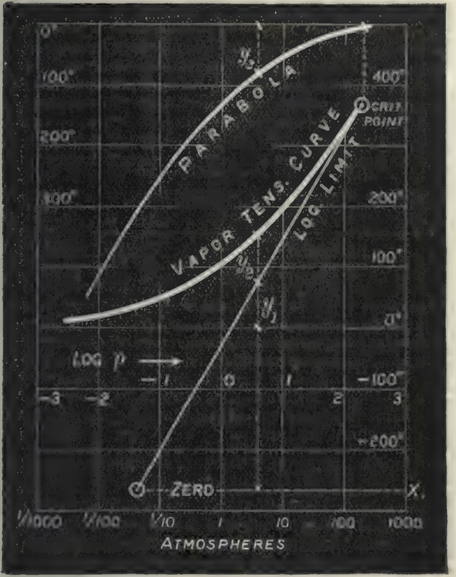

FIG. 2.

It hardly needs to be said that the tension of dissociation, and even the solubility of solids, are subject to the same general law.

The fusing points are obtained by simply changing the sign in (1) to

$$t = y_1 - y_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

so that the parabolic curve will be placed below the logarithmic limit.

One of the most remarkable results of this research is the mechanical determination of the true position of the carbon atoms in organic serials, and the complete explanation of the difference in fusing point between compounds containing an even and odd number of carbon atoms.

It should also be understood that the change in fusing point produced by change in pressure is expressed by the same general law.

Putting $\log a = x$, $\log p = z$, and $\log \alpha = \xi$, $\log \pi = \zeta$, the formulæ (1) to (7) will become

$$t = y_1 \pm y_2, \quad y_1 = k_1(x - \xi), \quad y_2 = k_2(\xi_2 - x)^2 \; . \; (8)$$
$$T = Y_1 + Y_2, \quad Y_1 = K_1(z - \zeta), \quad Y_2 = K_2(\zeta_2 - z)^2 \; . \; (9)$$

These formulæ strikingly show the simplicity of the laws stated, and also determine the surfaces formed by the coordinates x, t, and y in general.

In subsequent notes special topics covered by this general law will be taken up, and the complete concordance of the law with the results of observations will be shown.

Porpoises in African Rivers.

IN reference to Mr. Sclater's letter in NATURE of June 11 (p. 124), the following may be interesting to your readers:—

The skull of a Delphinoid Cetacean from Cameroon has lately come into my hands, through the kindness of Prof. Pechnel-Loesche. The sender, Mr. Edward Tëusz, gave the following information concerning it. The animal to which it belonged was caught in Kriegschiff Bay, after very heavy rains, and was being devoured by sharks. The contents of the stomach consisted of grass, weeds, and mangrove fruits. None of the natives had ever seen the animal before. In preparing the skull, Mr. Edward Tëusz noticed that the nostrils projected above the surface of the forehead.

I am preparing for publication a detailed description of the skull, and must here confine myself to remarking that, though the animal belongs to the genus Sotalia, it differs in several essential points from all the species of that genus hitherto described. I have no doubt that it is a new species. There are twenty-seven teeth on each side in each jaw. Their form, in that they are not pointed, but worn down, indicates, as also do the contents of the stomach, that the animal is herbivorous. It therefore seems certain that it is a fresh-water animal. It is well known that other Sotalia live in rivers.

Jena, June 20. WILLY KÜKENTHAL.

PHYSICAL SCIENCE FOR ARTISTS.[1]

I.

I THINK it right that I should begin by explaining how it is that I am here to-day, to lecture to you on a subject which touches art as well as science. It happens in this wise. Some years ago, while studying a certain branch of optics, it became important for me to try to learn something of the exact sequence of colours at sunrise and sunset; and being, like you, busy all day in a large city, I thought it would not be a bad idea, and that it would save a little time, if I studied pictures representing these phenomena *en attendant* the happy holiday time that I should spend in the country. So I went to the Academy and other picture galleries, and endeavoured to get up the information from pictures which I could not at that time get from Nature herself. I then had, as I have still, such an extreme respect for art and artists that I was perfectly prepared to take the pictures as representing truthfully what I wanted to see. The result, however, brought me face to face with a difficulty that I was not long in finding out. I was driven to the conclusion that artists could be divided into two distinct classes—those who studied Nature and Nature's laws, and gave us most exquisite renderings of this or that, and those who apparently considered themselves far superior to any such confining conditions as would be imposed by any law; and that, unfortunately, made me a little doubtful as to the results.

My friend, and your friend, Dr. Russell, happens to know this little bit of my experience, and hence it doubtless is that he requested me to come down to-day to say a few words to you, his plea being that this College is one of the very few institutions of its kind in the world where there is a studio and a physical laboratory side by side.

That, then, is the reason I am here, and what I want to impress upon you to-day is that the highest art can only be produced by those who associate the study of physical science with the study of art, and that therefore the possible producers of the highest art can only be looked for in such an institution as this if training of any kind has anything to do with it.

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[1] A Lecture delivered at Bedford College, by J. Norman Lockyer, F.R.S. on June 10, 1891.

I think that the *general* conditions of art training as they exist at present absolutely bar any sufficient knowledge of the laws and conditions of natural phenomena on the part of art students.

The *best* art of the time has always been on a level with the best science of the time, and if it had not happened that the first schools and the first Universities clustered round medical schools and schools of anatomy, I do not think that so much attention would be given to-day to anatomical science to the exclusion of all other branches.

You see, then, it comes to this. It is conceded by the art world that in a certain direction the phenomena of Nature require to be studied, otherwise that tremendously exuberant literature on Anatomy for Artists would not have been written, and more than half of the time of students of art would be spent in studying something else rather than those things which they do study.

It is on that ground that I would venture to say that in other institutions, as in this one, the study of physical science should be added to the other branches already recognized by the art world.

I am not an artist. I am not an art critic. I am almost unacquainted with the language usually employed by those who write on art subjects. I shall not deal with opinions, the algebraical sum of which in relation to the qualities of any one picture I have often noticed is zero; but what I shall try to do is to stick as closely as I can to the region of fact, and endeavour to show you, by two or three individual instances, how a student who wishes to become a great artist—as some of you no doubt do—will find his or her ambition more likely to be realized if the study of physical science be combined with that of "Art as she is taught" to-day.

In looking at the Academy Catalogue this year one finds the motto, "La mission de l'art n'est pas de copier la nature, mais de l'exprimer," and this is a true motto. But let us analyze it a little. To "express" suggests a language; a language suggests a grammar, if it is to be perfect, satisfying. But what can this grammar be, in the case we are considering, but the laws underlying the phenomena the "expression" of which, in his own language, constitutes the life-work of the artist. Should he be content to show himself a bumpkin? Are solecisms to be pardoned in his expressions because, so far, scientific training and thought are so limited? Is he justified in relying upon the ignorance of mankind, and, if so, is the highest art always to remain divorced from the highest knowledge?

Now it so happens that the branch of physical science which is above all things the thing to be studied by artists, is the branch of it which is already familiar to you—namely, optics. There could be no art without light; no artists without light; and the whole work of an artist, from the beginning to the end of his life, is to deal with light. Now we live in a world of white light. We might live in a blue world, or a green world, and then the condition of things would be different; but we can, in our laboratory, make our world red or green for the moment; but sometimes, indeed, when we do not seek to make this experiment, we find the world changed for us by the means which we employ for producing artificial lights, such as candles, gas, or the electric light; since in these, colours are not blended in the same way as in a sunbeam.

We thus come to the question of the radiation of light, and the way in which this light, whatever its quality, is reflected by natural objects; it is by this reflection that we see them. Everything that an artist paints which is white, is painted white by him for the simple reason that it reflects sunlight complete. It is perfectly clear that any reflecting surface can only reflect the light which it receives, although all surfaces do not reflect all of it—we have red walls and green trees; the direction of the light is not changed, except in the way of reflection, and you are already acquainted with the imperative law of optics—that when light falls upon a body and is reflected, the angle of reflection is equal to the angle of incidence.

To us this drastic law is of the very highest interest. We can apply it to art in a great many ways, but I will only take two very simple ones. Oftentimes it is our fortune to be in the country by the side of a river, or at the seaside. In both cases we see things reflected in water, and at first sight it would seem that here the artist ought to find perfectly free scope; but the worst of it is that, though he has free scope, sometimes his picture becomes very unpleasant to people who are acquainted with the law I have stated. I find here some diagrams, prepared by the kindness of some of our friends, which will show you the intimate connection between art and science in this direction. In the pictures which you will see in the Royal Academy and the New Gallery, I fancy you will see some which, if you care to study them from this point of view, will be found not to agree with the law.

In the diagrams we have a surface of water and observers at the top and bottom of a cliff. We have on the other side of this surface of water a tree. Now, what anyone would do who disdains to "copy" Nature, and who paints without thinking, is this: he would paint what he saw on the bank, and then turn it upside down and paint it again. But you see that will not do, because the conditions are as you see them here. The higher spectator, No. 1, the angles of incidence and reflection being equal, although he can see the upper part of the tree and part of the trunk, will not be able to see it all completely reflected in the water. You see that the lower part of the tree cannot be seen in the reflection, because any light reflected by it first to the water and then to the eye is really cut off from the eye of the spectator by the bank; if you greatly vary your distance from the other side of the water, you will find the reflection as represented in the other diagram. Now, to anyone who has studied optics, if such a matter as this is represented wrongly in a picture, it becomes an intolerable nuisance, and when you go away you feel sorry that the artist did not do justice to what he wished to represent. A good example of truth to Nature in this respect is to be seen at the German Exhibition—No. 205—in one of the landscapes, which I saw last night; it is a beautiful instance of careful study, and is absolutely true in this respect. The artist has shown how a mountain side, with high lights upon it, reflected on the surface of a lake, appears very different in the reflection, in consequence of an intervening elevation near the edge of the water. When you have thought out the difference of the appearances on the lake and on the hillside, you will appreciate the truth and skill of the artist enormously.

Another serious fault arising from the neglect of this same law is to be found in very many pictures in which we get the reflection of the sun or moon in water.

Obviously, if the water is disturbed, the reflection upon the water must depend upon the direction of the disturbance. I need not say more than that to you. You will quite understand what I mean; but if you look at the pictures in the Royal Academy this year—Nos. 677, 1071, and 1155—you can see how very admirably this reflection can be rendered; and if you look at 165 and think the conditions out, you will wonder how the artist should trouble to paint something that is absolutely opposed to the physical law.

You know that, in those instances where you get a natural reflection, if the light source be beyond the object which reflects the light, the nearer it is in a line with it the more light will be reflected. You see that that rule relates to almost every landscape or seascape that is painted, for the reason that our air is filled with particles which reflect light. If it were not so, our atmosphere would be absolutely black.

It therefore follows that the light of the sky must increase in intensity as the sun or moon is approached—

that is to say, in a sun-setting or moon-setting, if you paint an unbroken sky, there must be an increase of intensity towards the light source. I am almost ashamed to make such a statement, because it is so obvious to you as students of science, but to the artist who is not a very strict observer, why should it strike him? The fact remains that it has not struck a great many artists. If you study the pictures Nos. 650, 989, 1144, in the Royal Academy, and No. 39 in the New Gallery, you will find there indications of a neglect of this law. Now the sky is far more luminous than it ought to be by the light indicated by the landscape. Again, the setting sun is not so bright as the clouds which it is supposed to illuminate, and in some cases there is absolutely no grazing reflection indicated, and, if anything, the sky is rather less luminous where the sun is than further away!

A good rule, and one which a student of physical science would be certain to act upon with considerable care, would be never to show anything as reflected which was not there.

An interesting example of this kind was exhibited in the Academy some years ago. It so happened that a French man of science wrote a book on physical phenomena, beautifully illustrated. Among the illustrations was a coloured copy of a photograph of a soap bubble. Now the laboratory in the Collège de France, in which the photograph was taken, was, like yours, very well lighted by many windows, and the soap bubble was blown in the middle of it. A translation of this book appeared in English, and the illustrations were reproduced.

An artist had a most excellent idea. He thought he would paint a picture of a garden, which he did admirably. The foreground looked bare, so he thought he would put children playing in it. It next struck him, apparently, that the children did not seem to be quite sufficiently occupied, so he painted one blowing soap bubbles. But, alas! less fortunate than you, the artist had no laboratory in which he could blow and study soap bubbles for himself; so what did he do? He copied the bubble which was riddled with windows, although there were no windows in the garden. He thought that the nature of bubbles was windowy.

Then, again, in the matter of reflection, it would not be right that I should fail to remind you that, besides things terrestrial, we have the moon, which rules the night, and rules the night because it reflects the sunlight to us. Now, in a little talk like this I must not take up much time with astronomy, but it is fortunate that books on astronomy can be got for 6*d.* or 1*s.* which will tell us, say, in half an hour, the chief points about the moon which we need consider in the present connection. The moon is lighted by the sun. The sun can only light one half at a time. If we are on the side of the moon which is lighted by the sun, we must see the complete lighted half which we call a full moon. If we see a full moon, we must have our back to the sun. When the position of the moon with reference to the earth is such that we can see half the lighted portion of the moon, we generally find that the part of the moon which is turned to the sun is lighted up.

But none of these things are so in art. Last year a picture in the Academy was absolutely disfigured by the dark part of the moon being turned to the sun. Surely it was not worth the artist's while to paint a moon if he did not know how to do it. But the moon has been treated, if possible, worse than that. Some years ago a friend who knew I was interested in astronomy had another friend who had painted a picture, and he wished me to look at it to see if the moon was right. I went and saw the picture, and had to say that the moon was wrong. It was perfectly clear that the picture was intended to represent the sun setting on the right, beyond the part of the landscape included in the picture, so that the moon rising on the left, and shown in the picture, must be full. My friend said to me he knew this, and that as a matter of fact the artist had painted a full moon to start with, but he had altered it because it "destroyed the balance of his picture." That you see was where art came in. And then he added that the painter was not satisfied with the moon as it stood! I told my friend to say that I regretted that the full moon destroyed the balance of the picture, and that even a delicate crescent did not make things quite right, and I suggested that the effect of two or even three moons, of different sizes if needs be, should be tried. The artist said that this was nonsense; I replied that I did not consider it greater nonsense than the moon as he had represented it, and so the matter ended.

I am sure that the students of this College will know that such things as these are to be avoided, even if there were difficulties caused by the non-existence of a book on astronomy. No artist need paint a moon in a picture if he be too ignorant to paint it properly.

Everything that you paint in a picture, which you paint because it reflects light, should be painted its proper size in relation to the other objects. It seems, however, that the moment a body which reflects light does not happen to be on the surface of the earth, you may, in art, make it as large as you please. I do not think that the moon's distance from the earth gives us any right to treat it in this way.

An eminent American astronomer some years ago looked at the pictures in the New York galleries from this point of view. The moon subtends a certain angle. Everything else in a picture can be expressed in this way the moment you put a moon into it. This astronomer took the trouble to get out a statistical table of the heights of the different mountains and hills as drawn by American artists in pictures of places taken from other places (the distances being therefore known) with a moon thrown in. The maximum height was 105 miles, and the lowest 13!

Next, permit me to say a few words on another point, in order to show that the student of art will delight more and more in his work as he or she knows more and more of physical science. I now take refraction. You know that refraction can be divided into deviation and dispersion. The phenomena of deviation teach us that when a beam of light, whatever its colour, passes out of one medium into another its course is changed. An experiment, which is easily performed and which is more a home experiment than a laboratory one, is to put a coin into a basin and look over the edge in such a direction that the coin is just invisible: then fill it with water, the coin appears. Another experiment is to insert a straight body, such as a pencil, into this bowl of water: it appears to be broken; refraction, then, appears to make water shallower than it really is. If you look at 1094, you will find that this deviation has been made to act the wrong way.

It is rather a bad thing to attempt to paint a nymph partly in and partly out of *clear* water, because her body, if the picture be truly painted, would follow suit with the pencil.

Passing from deviation to dispersion we come to rainbows. You have learned, and perhaps seen demonstrated by experiment, that we deal with a beam of white light coming from the sun and refracted at the front surface of a rain-drop. It is next reflected and again refracted down to the eye, so that the eye sees a bow, with all the spectrum colours due to the dispersion. If the light be strong enough, we get what is called a supplementary bow, and, in consequence of internal reflections, the two reds are brought together.

The point is that in this dispersion, brought about by the rain-drops, the effect is produced in a plane passing through the sun, your eye, and the rain-drop; your eye being in the centre, so that if you see a rainbow at all, you must have your back to the sun. The bow is always circular, and high or low according to the height of the sun. Those are, of course, conclusions which a very restricted study of physical science will make perfectly clear: why you get the two reds together when two bows are visible; why the blue is inside, and the red outside the single bow, also follows from a demonstration which your teacher will give you, or which you can get from a book. The main point is that a rainbow is produced by a physical cause; so that, if you once grasp the idea of the cause of a rainbow, its whole anatomy will remain for ever with you.

It is quite impossible for you to see a rainbow in prospective, or projected on the sky as an ellipse. That will be quite clear, I think. Still, both these are recognized art-objects. I am sorry to say that in this year's Academy there is one case in which you will find that the fundamental condition of having your back to the sun has been neglected or forgotten by the artist. In No. 395 a most exquisite stump of rainbow is seen, most beautifully painted, and you naturally think, of course, that you have your back to the sun, but the artist has not been contented with painting the rainbow, he has painted cattle as well, and their shadows sweep across the picture. Another rainbow, 595, is excellently painted. The artist not only knows a great deal about rainbows, but wishes you to know that he knows, an umbrella being emphatically *en évidence*.

(*To be continued.*)

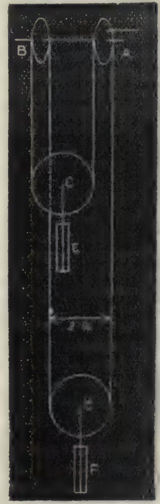
THE FARADAY CENTENARY.

On Wednesday, June 17, at the Royal Institution, Lord Rayleigh delivered a lecture in connection with the hundredth anniversary of Faraday's birth. The Prince of Wales presided.

Lord Rayleigh said that the man whose name and work they were celebrating was identified in a remarkable degree with the history of that Institution. If they could not take credit for his birth, in other respects they could hardly claim too much. During a connection of fifty-four years, Faraday found there his opportunity, and for a large part of the time his home. The simple story of his life must be known to most who heard him. Fired by contact with the genius of Davy, he volunteered his services in the laboratory of the Institution. Davy, struck with the enthusiasm of the youth, gave him the desired opportunity, and, as had been said, secured in Faraday not the least of his discoveries. The early promise was indeed amply fulfilled, and for a long period of years by his discoveries in chemistry and electricity Faraday maintained the renown of the Royal Institution and the honour of England in the eye of the civilized world. He should not attempt in the time at his disposal to trace in any detail the steps of that wonderful career. The task had already been performed by able hands. In their own Proceedings they had a vivid sketch from the pen of one whose absence that day was a matter of lively regret. Dr. Tyndall was a personal friend, had seen Faraday at work, had enjoyed opportunities of watching the action of his mind in face of a new idea. All that he could aim at was to recall, in a fragmentary manner, some of Faraday's great achievements, and if possible to estimate the position they held in contemporary science.

Whether they had regard to fundamental scientific import, or to practical results, the first place must undoubtedly be assigned to the great discovery of the induction of electrical currents. He proposed first to show the experiment in something like its original form, and then to pass on to some variations, with illustrations from the behaviour of a model, whose mechanical properties were analogous. He was afraid that these elementary experiments would tax the patience of many who heard him, but it was one of the difficulties of his task that Faraday's discoveries were so fundamental as to have become familiar to all serious students of physics.

The first experiment required them to establish in one coil of copper wire an electric current by completing the communication with a suitable battery; that was called the primary circuit, and Faraday's discovery was this: That at the moment of the starting or stopping of the primary current, then, in a neighbouring circuit, in the ordinary sense of the words, completely detached, there was a tendency to induce a current. He had said that those two circuits were perfectly distinct, and they were distinct in the sense that there was no conducting communication between them, but, of course, the importance of the experiment resided in this—that it proved that in some sense the circuits were not distinct; that an electric current circulating in one does produce an effect in the other, which is propagated across a perfectly blank space occupied by air, and which might equally well have been occupied by vacuum. It might appear that that was a very simple and easy experiment, and of course it was so in a modern laboratory, but it was otherwise at the time when Faraday first made it. With all his skill, Faraday did not light upon truth without delay and difficulty. One of Faraday's biographers thus wrote:—"In December 1824, he had attempted to obtain an electric current by means of a magnet, and on three occasions he had made elaborate and unsuccessful attempts to produce a current in one wire by means of a current in another wire, or by a magnet. He still persevered, and on August 29, 1831—that is to say, nearly seven years after his first attempts—he obtained the first evidence that an electric current induced another in a different circuit. On September 23rd, he writes to a friend, R. Phillips: I am busy just now again with electro-magnetism, and think I have got hold of a good thing, but cannot say; it may be a weed instead of a fish that, after all my labour, I at last haul up." We now know that it was a very big fish indeed. Lord Rayleigh proceeded to say that he now proposed to illustrate the mechanics of the question of the induced current by means of a model (see figure), the first idea of which was due to Maxwell. The one actually employed was a combination known as Huygens's gear, invented by him in connection with the winding of clocks. Two similar pulleys, A, B, turn upon a piece of round steel fixed horizontally. Over these is



hung an endless chord, and the two bights carry similar pendant pulleys, C, D, from which again hang weights, E, F. The weight of the cord being negligible, the system is devoid of potential energy; that is, it will balance, whatever may be the vertical distance between C and D. Since either pulley, A, B, may turn independently of the other, the system is capable of two independent motions. If A, B turn in the same direction and with the same velocity one of the pendant pulleys, C, D, rises, and the other falls. If, on the other hand, the motions of A, B are equal and opposite, the axes of the pendant pulleys and the attached weights remain at rest. In the electrical analogue the rotatory velocity of A corresponds to a current in a primary circuit, that of B to a current in a secondary. If, when all is at rest, the rotation of A be suddenly started, by force applied at the handle or otherwise, the inertia of the masses E, F opposes their sudden movement, and the consequence is that the pulley B turns *backwards*, *i. e.* in the opposite direction to the rotation imposed upon A. This is the current induced in a secondary circuit when an electromotive force begins to act in the primary. In like manner, if A, having been for some time in uniform movement, suddenly stops, B enters into motion in the direction of the former movement of A. This is the secondary current on the break of the current in the primary circuit. It might perhaps be supposed by some that the model was a kind of trick. Nothing could be further from the truth. The analogy of the two things was absolutely essential. So far was this the case that precisely the same argument and precisely the same mathematical equations proved that the model and the electric currents behaved in the way in which they had seen them behave in the experiment. That might be considered to be a considerable triumph of the modern dynamical method of including under the same head phenomena the details of which might be so different as in this case. If they had a current which alternately stopped and started, and so on, for any length of time, they, as it were, produced in a permanent manner some of the phenomena of electrical induction. The particular apparatus by which he proposed to illustrate those effects of the alternating current was devised by a skilful American electrician, Prof. Elihu Thomson, and he had no doubt it would be new to many. The alternating current was led into the electro-magnet by a suitable lead; if another electric circuit, to be called the secondary circuit, was held in the neighbourhood of the first, currents would be induced and might be made manifest by suitable means. Such a secondary circuit he held in his hand, and it was connected with a small electric glow lamp. If a current of sufficient intensity were induced in that secondary circuit it would pass through the lamp, which would be rendered incandescent. [Illustrating.] It was perfectly clear there was no conjuring there; the incandescent lamp brightened up. One of the first questions which presented itself was, what would be the effect of putting something between? Experimenting with a glass plate, he showed there was no effect, but when they tried a copper plate the lamp went completely out, showing that the copper plate was an absolute screen to the effect, whatever it might be. Experiments of that kind, of course in a much less developed and striking form, were made by Faraday himself, and must be reckoned amongst some of his greatest discoveries.

Before going further, he might remark on what strong evidence they got in that way of the fact that the propagation of the electric energy which, having its source in the dynamo downstairs, eventually illuminated that little lamp, was not merely along the wires, but was capable of bridging over and passing across a space free from all conducting material, and which might be air, glass, or, equally well, vacuum. Another kindred effect of a striking nature, devised by Prof. Elihu Thomson, consisted in the repulsive action which occurred between the primary current circulating around a magnet and the current induced in a single hoop of aluminium wire. Illustrating this by experiment, he showed that the repulsion was so strong as to throw the hoop up a considerable height. Those effects were commonly described as dependent upon the mutual induction between two distinct circuits, one being that primarily excited by a battery or other source of electricity, while the other occurred in a detached circuit. Many surprising effects, however, depended on the reactions which took place at different parts of the same circuit. One of these he illustrated by the decomposition of water under the influence of a single Daniell cell with the aid of self-induction.

About the time that the experiments of which he had been speaking were made, Faraday evidently felt uneasiness as to the soundness of the views about electricity held by his contemporaries, and to some extent shared by himself, and he made elaborate experiments to remove all doubt from his mind. He re-proved the complete identity of the electricity of lightning and of the electricity of the voltaic cell. He was evidently in terror of being misled by words which might convey a meaning beyond that which facts justified. Much use was made of the term "poles" of the galvanic battery. Faraday was afraid of the meaning which might be attached to the word "pole," and he introduced a word since generally substituted, "electrode," which meant nothing more than the way or path by which the electricity was led in. "Electric fluid" was a term which Faraday considered dangerous, as meaning more than they really knew about the nature of electricity, and as was remarked by Maxwell, Faraday succeeded in banishing the term "electric fluid" to the region of newspaper paragraphs

Diamagnetism was a subject upon which Faraday worked, but it would take him too long to go into that subject, though he must say a word or two. Faraday found that whereas a ball of iron or nickel or cobalt, when placed near a magnet or combination of magnets, would be attracted to the place where the magnetic force was the greatest, the contrary occurred if for the iron was substituted a corresponding mass of bismuth or of many other substances. The experiments in diamagnetism were of a microscopic character, but he would like to illustrate one position of Faraday's, developed years afterwards by Sir Wm. Thomson, and demonstrated by him in many beautiful experiments, only one of which he now proposed to bring before them. Supposing they had two magnetic poles, a north pole and a south pole, with an iton ball between them, free to move along a horizontal line perpendicular to that joining the poles, then, according to the rule he had stated, the iron ball would seek an intermediate position, the place at which the magnetic force was the greatest. Consequently, if the iron ball be given such a position, they would find it tended with considerable force to a central position of equilibrium; but if, instead of using opposite poles, they used, *e.g.*, two north poles, they would find that the iron ball did not tend to the central position, because that was not the position in which the magnetic force was the greatest. At that position there was no magnetic force, for the one pole completely neutralized the action of the other. The greatest force would be a little way out, and that, according to Faraday's observations, systematized and expressed in the form of mathematical law by Sir Wm. Thomson, was where the ball would go. [This was illustrated by experiment.]

The next discovery of Faraday to which he proposed to call attention was one of immense significance from a scientific point of view, the consequences of which were not even yet fully understood or developed. He referred to the magnetization of a ray of light, or what was called in more usual parlance the rotation of the plane of polarization under the action of magnetic force. It would be hopeless to attempt to explain all the preliminaries of the experiment to those who had not given some attention to those subjects before, and he could only attempt it in general terms. It would be known to most of them that the vibrations which constituted light were executed in a direction perpendicular to that of the ray of light. By experiment he showed that the polarization which was suitable to pass the first obstacle was not suitable to pass the second, but if by means of any mechanism they were able, after the light had passed the first obstacle, to turn round the vibration, they would then give it an opportunity of passing the second obstacle. That was what was involved in Faraday's discovery. [Experiment.] As he had said, the full significance of the experiment was not yet realized. A large step towards realizing it, however, was contained in the observation of Sir William Thomson, that the rotation of the plane of polarization proved that something in the nature of rotation must be going on within the medium when subjected to the magnetizing force, but the precise nature of the rotation was a matter for further speculation, and perhaps might not be known for some time to come.

When first considering what to bring before them he thought, perhaps, he might include some of Faraday's acoustical experiments, which were of great interest, though they did not attract so much attention as his fundamental electrical discoveries. He would only allude to one point which, as far as he knew, had never been noticed, but which Faraday recorded in his acoustical papers. "If during a strong steady wind, a smooth flat sandy shore, with enough water on it, either from the receding tide or from the shingle above, to cover it thoroughly, but not to form waves, be observed in a place where the wind is not broken by pits or stones, stationary undulations will be seen over the whole of the wet surface. . . . . These are not waves of the ordinary kind, they are (and this is the remarkable point) accurately parallel to the course of the wind." When he first read that statement, many years ago, he was a little doubtful as to whether to accept the apparent meaning of Faraday's words. He knew of no suggestion of an explanation of the possibility of waves of that kind being generated under the action of the wind, and it was, therefore, with some curiosity that two or three years ago, at a French watering-place, he went out at low tide, on a suitable day when there was a good breeze blowing, to see if he could observe anything of the waves described by Faraday. For some time he failed absolutely to observe the phenomenon, but after a while he was perfectly well able to recognize it. He mentioned that as an example of Faraday's extraordinary powers of observation, and even now he doubted whether anybody but himself and Faraday had ever seen that phenomenon.

Many matters of minor theoretic interest were dealt with by Faraday, and reprinted by him in his collected works. He was reminded of one the other day by a lamentable accident which occurred owing to the breaking of a paraffin lamp. Faraday called attention to the fact, though he did not suppose he was the first to notice it, that, by a preliminary preparation of the lungs by a number of deep inspirations and expirations, it was possible so to aërate the blood as to allow of holding the breath for a much longer period than without such a preparation would be possible. He remembered some years ago trying the experiment, and running up from the drawing-room to the nursery of a large house without drawing any breath. That was obviously of great practical importance, as Faraday pointed out, in the case of danger from suffocation by fire, and he thought that possibly the accident to which he alluded might have been spared had the knowledge of the fact to which Faraday drew attention been more generally diffused.

The question had often been discussed as to what would have been the effect upon Faraday's career of discovery had he been subjected in early life to mathematical training. The first thing that occurred to him about that, after reading Faraday's works, was that one would not wish him to be anything different from what he was. If the question must be discussed, he supposed they would have to admit that he would have been saved much wasted labour, and would have been better *en rapport* with his scientific contemporaries if he had had elementary mathematical instruction. But mathematical training and mathematical capacity were two different things, and it did not at all follow that Faraday had not a mathematical mind. Indeed, some of the highest authorities (and there could be no higher authority on the subject than Maxwell) had held that his mind was essentially mathematical in its qualities, although they must admit it was not developed in a mathematical direction. With these words of Maxwell he would conclude: "The way in which Faraday made use of his idea of lines of force in co-ordinating the phenomena of electric induction shows him to have been a mathematician of high order, and one from whom the mathematicians of the future may derive valuable and fertile methods."

THE ROYAL NAVAL EXHIBITION.

THE Naval Exhibition, now being held at Chelsea, is distinctly a popular show. The management—recognizing that the first duty of an Exhibition is not to show a pecuniary deficit—has wisely decided to follow the lead given by Sir Philip Cunliffe Owen, and has devoted the chief of its energies to fireworks, waxworks, peep-shows, pictures, shooting-galleries, mimic sham fights, and musical entertainments of a kind known to sailors as "sing-songs." The end justifies the means. Not only does the Committee of distinguished Admirals labour to supply Londoners with a cheap and innocent means of enjoyment, but the final result will be the establishment of a substantial fund to endow a most deserving charity. Fortuitously there are features which possess a more serious interest; and though there may be nothing especially new in the Exhibition, the man of science who has not been brought much in contact with naval matters may find there a good deal that is worth consideration.

The Exhibition appears to be divided into about half-a-dozen sections, each under the direction of a committee. Of these the "Entertainments" and "Refreshments" Committees are of course the chief; but the Models Committee appears to be the one which has made the most serious effort to present a distinctly naval subject in logical sequence. In the Seppings Gallery there is a collection of models of warships illustrating the progress of naval architecture, from the *Great Harry* down to the very latest design of armour-clad battleship. The model of the *Great Harry* is of very doubtful authenticity, and is of modern construction, having been made by the aid of such pictures of the great sixteenth-century ship as exist. No historical collection of British warships would, however, be even approximately complete without a representation of this vessel. Charnock, our great authority on the subject, has styled her "the parent of the British Navy"; and if it be true, as supposed, that she was the first warship to sail on a wind, the claim is most amply justified. In fact, naval architecture as a science was not founded until it was discovered that ships could be, otherwise than by the aid of oars, taken to the quarter from which the wind was blowing. It must have seemed a great feat in those days—little less than necromancy. Fortunately for the timid intellects of our ancestors, the revelation broke upon them gently, for the rounded hulls, high topsides, and curiously rigged craft could not have sailed more than a point or two to windward. Still, it was the *Great Harry*, or one of her contemporaries, by means of which this new feature in seamanship was inaugurated; a feature by which the great middle period in the world's history of naval warfare was created, and which enabled the sailors of those times to make a distinct advance upon the lessons taught them by their instructors in the art of shipcraft, the Phœnicians, Romans, and Scandinavians. It would have been well if we had improved on our predecessors in other nautical matters as well; and we then should not have had, even in the present century, our shipwrights attaching lead sheathing to ships' bottoms with iron nails. The Romans used copper fastenings when they lead-covered the under-water part of their vessels.

There are but three models of seventeenth-century ships in the Exhibition, but one of these is a vessel that forcibly illustrates, by contrast, the mutability of the present age. The *Royal William* was designed by the first great naval architect, Phineas Pett—whose name might almost more appropriately have been given to the Models Gallery than that of Seppings—and was built at Chatham in 1670. She was originally a three-decker, carrying one hundred guns, but in 1757 she was cut down to a ship of 84 guns, and was finally broken up in 1813—a fact duly recorded by the present Director of Naval Construction, Mr. W. H. White, in his delightful lecture on "Modern War Ships," delivered a few years ago at the Mansion House. The *Royal William* must not, however, be taken as an example of the endurance of ancient materials so much as of the slow changes in design which characterized the proceedings of our ancestors. The original material part of the *Royal William* only lasted twenty-two years, for she was rebuilt, we are told, in 1692, and again in 1719; so that in this respect she compares unfavourably with so modern a vessel as our first ironclad, the *Warrior*, which has only recently been taken out of the Navy after a service career of not far from 30 years. Even now the *Warrior* has not been removed from the Navy list because she has become worn out, but simply because she has become obsolete. If we could reach finality in design—if the inventive brain would stagnate—there is no reason why the modern iron-built warship should not outlast its wooden predecessor by almost as great an extent as it exceeds it in power of destruction. It is true the natural life of the old ships was a long one. The *Victory* was forty years old when she was engaged in the battle of Trafalgar, and had seen much active service, having been launched at Chatham in 1765; but then she had been laid by as worn out in 1801, and it was only after extensive repairs that she was made fit for sea. A year or two ago, it will be remembered, she was found to be so rotten that she would have sunk at her moorings had she not been taken into dock and in part rebuilt. On the other hand, there is no reason why an iron ship should not last, provided she were properly painted and kept up, perhaps until the era when warships will have become relics of a barbarous past. The expression "properly painted" must be here taken in its literal sense; and with regard to steel ships due steps must be followed to remove mill-scale, a precaution which has not always been taken of late, as quite recent mishaps have testified.

Passing from hulls to motive power, we find the same governing principles as to durability of material and impermanence of design more strongly emphasized in the practice of to-day compared with that of the naval era which closed with the introduction of steam and iron hulls. With comparatively small variations in detail the rig of war ships has remained unchanged from the days of Pett down to those within the memory of men still living. The *Henri Grace à Dieu* shows a distinctly mediæval rig—although her fighting-tops are ridiculously like those of our very latest armour-clads—but it would take almost a sailor's eye to point out the differences in sail plan between Vandevelde's beautiful painting of the *Sovereign of the Seas*, "built in 1637," and the ships which appear on the canvases of Stanfield, Turner, and Cooke. So much for permanence of design with masts and sails; with the succeeding mode of propulsion, engines and boilers, we find as striking a result in the opposite direction. Steam machinery was first introduced into the Royal Navy in small gun-boats, and later in the paddle-wheel frigates, but it was not until the screw was proved to be the more effective instrument that even the most sanguine engineers could hope that engines and boilers would successfully rival masts and sails as a means of propulsion. We pass over, therefore, the unimportant era of paddle-wheels, but even taking screw engines alone we find that during the last forty years far greater changes have taken place in the design of steam machinery than characterized the arrangement of masts and sails during the two hundred years elapsing between the time the *Sovereign of the Seas* was built and the practical introduction of steam into the Navy; indeed we might, without any great fear of contradiction, go further and say that to the eye of the engineer there is no greater affinity between the screw engines of forty years ago and those of the present day, than existed between the rigging of the ships of the Norse sea-kings and those of almost our own day, putting on one side only the element of size. The collection of engine models in the Exhibition is far from complete, and is not to be compared with that of ship models. There is a good reason for this, as engineers work to drawings, and models are seldom made excepting as records; whilst their cost is so great as to render them available only for very rich firms. The collection of models shown by Messrs. Maudslay, Sons, and Field constitute the greater part of the historical collection in the Exhibition. Here may be seen representations of the first types of steam-engine introduced into the Navy; and we think a comparison of the early engines in this collection with, say, the magnificent model of the *Sardegna's* engines, shown by Messrs. Hawthorn, Leslie, and Co., will bear out the remarks we have made. What path the progress of marine engineering will follow in future it is difficult to forecast. The inventions of to-day always seem to have reached finality, but it is difficult to imagine that any fundamental change can be effected so long as we retain the use of steam as a vehicle for the conversion of heat into work. It may be that a little engine shown in the Exhibition—Priestman's oil engine—may contain the germ of a principle upon which marine engines may be designed in future, and that before we have got far into the twentieth century the marine boiler, with all its costliness and complication, may have become as much a relic of the past as the pole masts and uncouth sails of the *Great Harry*. Before that time arrives, however, the four-stroke cycle will have to be superseded.

It is, however, the steam boiler, rather than the engine, which has governed the design of ship machinery. Forty- to forty-five years ago, steam pressures were not generally higher than 5 to 8 pounds per square inch. With the introduction of tubes in place of flues, which took place between 1840 and 1850, the working pressure rose to 15 pounds per square inch. The square box boiler was in use, and with that type the working pressure was limited to about 30 pounds per square inch, or not much beyond, unless the staying of the flat surfaces was carried to an undesirable extent. With such a limit of pressure, the simple expansion engine was, properly, the usual type, but when the cylindrical marine boiler was introduced, the average steam pressure quickly rose to 60 pounds to the square inch, and the compound engine naturally followed. The surface condenser formed a necessary part of this step in advance, for, with the higher temperature due to the increased steam pressure, it was impossible to pass large quantities of salt water through the boilers without rapidly scaling them up. For some time the difficulty in generating higher pressure steam caused stagnation in marine engineering practice; until the substitution of steel for iron in boiler making, the advent of new types of furnaces, and improvements in the machinery used in boiler construction have enabled pressures as high as from 150 pounds to even 200 pounds to the square inch to be carried. The result has been that, for the two-cylinder compound engine, there have been substituted two types of engine, known respectively as the triple expansion engine and the quadruple expansion engine. The names are misleading, as even the ordinary compound engine expands its steam more than three or four times.

The growth of the science of marine engine design, which we have so briefly sketched out, may appear, to those who are not engineers, but little more than a record of increasing steam pressures. Undoubtedly a higher steam pressure has been the fundamental reason for these advances, but the carrying out of these successive changes in pressure has necessitated an entire reconstruction of marine engine practice; so that an engine working at 15 pounds pressure can hardly be said to belong to the same category as one working at 150 to 200 pounds pressure. Tooth-wheel gearing, which was first used with screw propellers, has long ago disappeared, side levers and trunks are no longer introduced, and the surface condenser has become a necessity. In the old days, with jet condensers, the boilers were fed entirely with salt water, now in the best marine practice the condensed steam is all returned to the boiler, excepting that which is unavoidably lost, and this quantity is made up by special distillers and condensers, the manufacture of which has introduced a new branch of marine engineering, as may be judged by several exhibits by different firms in the Exhibition. The practice of circulation of refrigerating water through the surface condenser by means of separate centrifugal pumping engines has also introduced a distinctive type of auxiliary marine engine, upon which several important firms have been chiefly employed. Indeed, the increase in auxiliary machinery has been as marked a feature in the recent progress of marine engineering as have been the changes in the main engines themselves. A battleship of the first class will carry between seventy and eighty separate engines, in addition to those used for driving the propellers. These include electric light engines, hydraulic machinery in connection with the working of heavy guns, steering engines, &c. As an instance of what is gained by the use of auxiliary machinery, an instance given by Mr. White may be quoted. On one occasion it took 78 men 1½ minutes to put the helm of the *Minotaur* hard over. Steam gear was subsequently fitted, by the aid of which two men were able to do the same thing in 16 seconds.

We do not propose to give a list of the various objects exhibited, to which we have referred in penning these remarks. The official catalogue performs that function far more completely than we could hope to do. The collection at Chelsea is well selected and fairly complete, and there will be found there material for object-lessons in all we have advanced in this brief sketch. We may, however, with advantage, add a few figures as to money cost, which cannot fail to be of interest, and for which we are indebted to the Director of Naval Construction. The cost of a 100-gun line-of-battle ship at the beginning of the century was about £65,000 to £70,000, armament and stores being excluded. The corresponding outlay on the 110-gun sailing three-deckers of 1840 was about £110,000; and that of the 121-gun screw three-deckers of 1859 about £230,000, machinery included. The *Warrior*, completed in 1861, cost over £375,000; and the *Minotaur* class about £480,000. With the increase in size of the *Dreadnought*, and the introduction of hydraulic mechanism, came an increase of cost to £620,000; while the *Inflexible* cost no less than £810,000." The *Nile* and *Trafalgar*, complete with armament, would represent little less than a million sterling each. The cost of the armour-plating, propelling machinery, and hydraulic gun mountings alone, would have paid for five first-rates of Nelson's time. The sum paid for the armour alone on one of our latest battleships, such as the *Royal Sovereign*, would pay for the Natural History Museum at South Kensington; whilst even a first-class torpedo-boat costs as much to build and equip as a 40-gun frigate of Nelson's time.

A GEOLOGICAL EXCURSION IN AMERICA.

I BEG to call to your attention the following short account of a geological excursion planned for the benefit of foreign geologists who may attend the coming meeting of the International Geological Congress in this city in August next. It will afford an exceptionally favourable opportunity for European geologists to become personally familiar with the most important geological phenomena of the United States.

I venture, therefore, in their interest, to request that you publish some notice of it in your widely circulated periodical, with a request that those who desire to take part in it will kindly advise me as early as possible, in order that arrangements may be thoroughly perfected beforehand. A single train will carry 75 to 100 persons comfortably. If more join, the party will be arranged in two trains. Arrangements will have to be made beforehand at the various stopping places along the road for the reception of the party, and you can therefore readily understand the importance of knowing as early as possible how many are to be accommodated.

S. F. EMMONS, Secretary.

Washington, D.C., May 30.

For the close of the fifth session of the International Congress of Geologists, which is to be held at Washington, D.C., from August 26 to September 2, a grand geological excursion has been organized, which presents unusual attractions and facilities for the European geologists who attend the Congress, and who wish to see some of the geological wonders which have become familiar to them through the memoirs of American geologists. The excursionists will start from Washington, on September 3, on a special train of Pullman vestibuled cars, which will constitute a moving hotel, being provided with sleeping and toilet accommodations for both ladies and gentlemen, restaurant cars, smoking, reading, and bath rooms, and barber's shop, and so arranged that travellers can pass freely at all times from car to car through covered passages. It will accompany the party wherever the rails are laid in the regions visited, the hours being arranged so that all the most interesting portions of the route will be passed over in the daytime, and stops may be made wherever any object of special interest to the travellers presents itself. American geologists who have made special studies of the different regions visited will accompany the train, and explain their geological structure upon the ground. The main route laid out is over 6000 miles (nearly 10,000 kilometres) in length, and extends over 38° of longitude and 12° of latitude. It is planned to occupy 25 days, and the cost per person will be 265 dollars (1325 francs), which will cover all necessary expenses, of whatever kind, during the trip.

The following are the principal objects of geological interest which will be seen by those who make the excursion:—

Going westward, the Appalachian Mountains are first crossed, and an opportunity will be had to see the closely appressed Palæozoic rocks which constitute their typical structure. The prairie region of Indiana and Illinois, at the southern end of Lake Michigan, its ancient outlet into the Mississippi River, will be seen on the second day, and the Kettle moraines of the ancient Glacial sheet will be visited under the guidance of Prof. Chamberlin. On the third day the twin cities of Minneapolis and St. Paul, centres of the great wheat-growing region of the north-west, will be visited, and glacialists will have an opportunity to see one of the time gauges of the Glacial period, at the Falls of St. Anthony, on the Mississippi River.

During the fourth day the Great Plains of Dakota will be crossed, and toward its close the characteristic Badland topography of the Upper Missouri region will be seen. On the morning of the fifth day the travellers will leave the train at the entrance to the Yellowstone Park, and during the following week will be transported by stages through the Park region, stopping at rustic hotels established near points of special interest. The various geyser basins, the hot lakes and mud volcanoes, the obsidian cliffs, the falls and cañon of the Yellowstone River, the Yellowstone Lake, and other objects of interest, will be successively visited under the guidance of Messrs. Arnold Hague and Jos. P. Iddings.

On the twelfth day the railroad journey will be resumed, and, after crossing the crest of the Rocky Mountains in Montana, a stop of several hours will be made at the famous mining town of Butte, whose mines produced, during 1890, over 26 million dollars worth of copper, silver, and gold.

The morning of the thirteenth day will find the travellers on the edge of the great lava plains of the Snake River. Those especially interested in volcanic phenomena will have an opportunity here of making a side trip across these plains to Shoshone Falls, where the Snake river makes a single leap of over 200 feet, and cuts a narrow gorge 600 feet deep in the andesitic and basaltic lavas. The main party meanwhile will proceed southward into Utah, viewing the desert mountain ranges, the shore-lines of ancient Lake Bonneville, and skirting the shores of its present relic, the Great Salt Lake, will reach Salt Lake City, the Mormon capital, in the afternoon. A halt of three days will be made in Salt Lake City, which will give the travellers an opportunity of seeing the Mormons, the desert scenery around Salt Lake (with bath in the lake), and the magnificent Wahsatch Mountains. The Pleistocene phenomena will be explained by Mr. G. K. Gilbert, and the mountain structure and mining geology by Mr. S. F. Emmons.

On the sixteenth day the railroad journey will be continued across the Wahsatch Mountains into the plateau region of the Colorado River, crossing that stream in the afternoon, and obtaining views of great monoclinal scarps, and groups of laccolitic mountains in the distance.

On the seventeenth day the Rocky Mountain region of Colorado will be entered, through its finest cañon gorges, affording wonderful geological sections. Halts of a few hours each will be made at Glenwood Springs and at the famous mining town of Leadville, which has produced over 150 million dollars worth of silver and lead.

On the eighteenth day the train will descend the great mountain valley of the Arkansas River, between mountain peaks over 14,000 feet high, and through cañon gorges 3000 feet deep, debouching upon the plains through the Royal Gorge at Cañon City, where a remarkable geological section in the "Hogback" ridges will be visited. A short stop will be made at Pueblo, a great centre of smelting works; and Manitou Springs, in a sheltered nook under Pike's Peak, will be reached in the evening.

The nineteenth day will be spent at Manitou Springs, the vicinity of which abounds in objects of geological and mineralogical interest, and those who wish may make the ascent of Pike's Peak (14,200 feet) by rail.

The twentieth day will be spent at Denver, the capital of Colorado, a beautiful city of 130,000 inhabitants, having a view of the whole eastern front of the Rocky Mountains. For those who desire it, a further excursion of ten days or more will be organized under the guidance of J. W. Powell and C. E. Dutton, to the Great Cañons of the Colorado River in Arizona, which they have so fully described in their writings. More detailed visits to the mining districts of Colorado will be directed by S. F. Emmons for those who wish to remain over for that purpose. Those who remain over will receive tickets securing them passage to New York by regular trains when they are ready to start.

The special train will leave Denver on the evening of September 21, crossing the Great Plains of Kansas and Nebraska and the Mississippi Valley, and reaching Chicago on the evening of the 23rd. A day will be given to Chicago, and thence the train will skirt the Great Lakes, Michigan, Huron, and Erie, crossing a portion of Canada, and reaching Niagara Falls on the morning of September 25. Leaving there in the evening, the travellers will descend the beautiful valley of the Hudson River early the following morning, and reach New York before noon of September 26.

NOTES.

THE Delegates of the University Press have informed Prof. Sylvester that they will be prepared to bear the expense of publishing in quarto a complete edition of his mathematical works. We understand that a memorial recommending this course was addressed to the Delegates of the Press, numerously signed by leading mathematicians of the two English Universities, and by eminent members of the French Academy of Sciences.

GEOLOGISTS on this side of the Atlantic will learn with deep regret that Captain Dutton, whose admirable memoirs in the Reports and Monographs of the U.S. Geological Survey are so widely known and valued, has been ordered to take up military duty in Texas—a wide pastoral region where his genius as a geological explorer will find no scope for exercise. As a member of the Corps of Engineers, he has of course always been liable to be taken away to mere routine service of this kind, for which any ordinary officer of his grade would be sufficient. But the authorities have hitherto appreciated his remarkable powers, and have allowed them free exercise, much to their own credit and greatly for the benefit of science. Whether a new martinet has resolved to apply the rigid rules of the service we do not know. But surely there ought to be public spirit enough in the United States to put such pressure on the Engineer Department as will make it reconsider its arrangements. It has only one Captain Dutton, and should be proud of him and make the most of him.

THE Council of the Royal Meteorological Society has decided to arrange for a general dinner, open to all Fellows and their friends, to be held in commemoration of the entrance of the Society on its new premises. The dinner will take place at the Holborn Restaurant on Tuesday, July 7, at 6.30 p.m.

THE Committee appointed by the Hebdomadal Council, Oxford, to consider in what way the University could assist in the establishment of agricultural education, with a special view to the needs of the County Councils, have now submitted their report. By agricultural education the Committee understand instruction in the sciences, or the branches of science, specially applicable to agriculture, employing the latter term with the larger meaning which must have been present to the mind of Dr. Sibthorp when he designated the professorship founded by him the professorship of "Rural Economy." Used in this sense agriculture becomes not merely the science of the cultivation of the soil, but includes the knowledge of its constitution and properties, of its vegetable products, and of the structure, habits, and uses of the domestic animals that are reared upon it; so that the student has evidently much to gain by a knowledge of such subjects as botany, chemistry, animal physiology, and geology. Taking into account the requirements of the County Councils, the Committee think that the efforts of the University should in the first place be directed to the provision of an adequate supply of persons qualified to be lecturers or teachers; and those members who are most familiar with the wants of the counties lay stress upon the importance of University teachers possessing credentials of practical acquaintance with the details of farming and farm-life, which has hitherto been only accidentally—if at all—acquired by such teachers. Other classes of persons whose circumstances the Committee think deserving of consideration are young men who go to Oxford intending to take an ordinary degree, and then, either as landowners or the agents of landowners, to devote themselves to the pursuit and improvement of agriculture; and young men who might go to Oxford with a view to attending such courses of instruction as would be useful to them in agriculture, but without the intention of taking a degree. Dealing with the means already at the command of the University for providing agricultural education, the Committee point out that the professors to whose services resort would most naturally be had are the following: the Sibthorpian Professor of Rural Economy, the Sherardian Professor of Botany, the Waynflete Professor of Chemistry, the Waynflete Professor of Physiology, and the Professor of Experimental Philosophy (Physics). In addition to these University Professors, there are the Lee's Readers in Chemistry and Physics at Christ Church, and the Millard Lecturer in Physics at Trinity College, whose courses would probably be open to agricultural students. The Committee sketch the proper course of study for each class of students, and express the opinion that for the organization and supervision of the studies pertaining to agricultural education some further provision is needed than at present exists. In the Sibthorpian Professorship of Rural Economy, which is now vacant, they recognize a foundation capable of being rendered the centre of agricultural education within the University; and they strongly recommend that the duties and emoluments of the chair should be revised.

The annual dinner of the Royal Horticultural Society was held on Tuesday evening at the Hôtel Métropole. The chair was taken by Sir Trevor Lawrence, the President. The toast of the evening, "The Royal Horticultural Society," was proposed by Sir James Paget, who spoke of the work in which the Society was engaged as one that ministered to the happiness and welfare of the whole nation. The President responded. The Society is now in a most prosperous condition, and is to be congratulated on the progress it has made under Sir Trevor Lawrence's leadership.

We print elsewhere a report of the lecture delivered by Lord Rayleigh at the Royal Institution last week in connection with the Faraday Centenary. In commemoration of this anniversary the Royal Institution elected as honorary members a number of foreign men of science, several of whom came to London to be presented with the diploma of membership by the Prince of Wales. As the distinction between the Royal Institution and the Royal Society is not always so well understood in foreign countries as it is in England, the Royal Institution can hardly, perhaps, be congratulated on this "new departure." The following is the list of those on whom the honour was conferred:—Edmond Becquerel, Marcellin Berthelot, Alfred Cornu, E. Mascart, Louis Pasteur, Paris; R. W. Bunsen, Heidelberg; H. L. F. von Helmholtz, A. W. von Hofmann, Rudolph Virchow, Berlin; J. P. Cooke, Cambridge, U.S.; J. Dwight Dana, J. Willard Gibbs, Newhaven, U.S.; Simon Newcomb, Washington, U.S.; Stanislas Cannizzaro, Pietro Tacchini, Rome; Julius Thomsen, Copenhagen; T. R. Thalen, Upsala; Demetri Mendeleef, St. Petersburg; J. C. G. de Marignac, Geneva; J. D. van der Waals, Amsterdam; J. Servais Stas, Brussels.

A Commission has been appointed for the reorganization of the Paris Museum of Natural History, and held its first meeting last week under the presidency of the Minister of Public Instruction. The members are MM. Berthelot, Bardoux, Burdeau, Charles Dupuy, Darboux, Frémy, Chauveau, Milne-Edwards, and Liard.

A *conversazione* will be given by the President of the Institution of Electrical Engineers and Mrs. Crookes in the galleries of the Royal Institute of Painters in Water Colours on Monday evening, July 6.

On Monday evening, in the House of Commons, Sir H. Roscoe asked the President of the Board of Trade whether he had decided to grant the application of the Committee of the National Institute of Preventive Medicine to become incorporated under the Companies Act, with the omission of the word "limited" in view of the amended proposals which had been placed before him. Sir M. Hicks-Beach replied as follows:—"The amendment of the proposed memorandum of association referred to by the hon. member (by which it is made clear that the grant of the licence now asked for would not in any way imply approval by the Board of Trade of experiments upon living animals, or of any application to the Home Secretary for a licence for that purpose) is, no doubt, an important change in the proposals of the Institute, and will probably meet the objection stated to the deputation which lately waited upon me. There are, however, one or two other points requiring consideration, but I hope shortly to be able to arrive at a decision on the subject."

Sir Prescott Gardiner Hewett, F.R.S., died on Friday night last at his residence, Chestnut Lodge, Horsham, Sussex. He was born in 1812, and in 1836 was admitted a member of the Royal College of Surgeons, of which he was made President in 1876, in succession to Sir James Paget.

With the approval of the President, the Prince of Wales, the Council of the Society of Arts have awarded the Albert Medal to Sir Frederick Abel, K.C.B., "in recognition of the manner in which he has promoted several important classes of the arts and manufactures, by the application of chemical science, and especially by his researches in the manufacture of iron and of steel; and also in acknowledgment of the great services he has rendered to the State in the provision of improved war material, and as chemist to the War Department."

The Report of the Savilian Professor of Astronomy has been presented to the Board of Visitors of the University Observatory, and we learn from it that the photographic telescope, prepared for taking part in the International Chart of the Heavens, is at length complete. The guiding telescope also is provided with a micrometer sufficient to permit the observation of stars at a considerable distance from the centre of the plate, and the camera end of the telescope is fitted with the apparatus devised by the Astronomer-Royal, and executed by Sir Howard Grubb. The Oxford University Observatory is also provided with two *réseaux*, supplied through Dr. Vogel, of the Potsdam Observatory, and has very recently added to its equipment a measuring machine of great delicacy for the discussion of the plates taken in connection with the international scheme. Altogether the equipment of the Oxford University Observatory appears to be in a very forward state of preparedness, and Prof. Pritchard congratulates himself and the University that this equipment has entailed no unusual appeal to funds, on which there are so many claims, but has been supplied by the bounty of the late Dr. De La Rue, supplemented by strict economy in the management of the Observatory in former years. The astronomical work of the past year has been mainly confined to the discussion of the parallax of stars of the second magnitude, and this work is now on the brink of accomplishment. Seven complete determinations, including that of β Aurigæ, have been made in the year, and but six other stars, the measures of which are complete, await discussion. Prof. Pritchard concludes his Report as usual, by acknowledging the aid he has received from his two assistants. and we are glad to see speaks hopefully of his restoration to complete health.

The President of the French Republic inspected the meteorological instruments at the summit of the Eiffel Tower on June 13, and afterwards visited the Central Meteorological Office, where he witnessed M. Weyer's experiments on the formation of tornadoes, and also inspected the instruments which there register the indications of the meteorological phenomena at the top of the Eiffel Tower.

The French Minister of Public Instruction has appointed Dr. Henry de Varigny, assistant in the Museum of Natural History, to report on the University Extension movement, and has commissioned him to study the question in Edinburgh, London, and Oxford.

The proposed law on Universities is exciting a good deal of discussion in France. Many local jealousies have been aroused in connection with the question. Every town that boasts the possession of a tenth-rate medical school, or of an inadequate scientific faculty, wishes to have a University; and its political representatives have, of course, to do what they can to press its claims. On the other hand, the Government, which would willingly establish five or at most six large Universities, desires if possible, to do away with small and useless institutions.

A scientific expedition which has been organized in Maine is about to spend some time in Labrador. The principal object of the party will be to collect ethnological specimens. They will take with them a phonograph, with which they hope to obtain some materials for the study of the language and songs of the Eskimo.

In drawing up schemes for the appropriation of the funds placed at their disposal under the Local Taxation Act, 1890, for the promotion of technical instruction, the County Councils certainly ought not to overlook the claims of girls' education. With a view of aiding County Councils in this department of their work, the Committee of the National Association for the Promotion of Technical and Secondary Education has submitted to them a careful outline of subjects which are adapted for girls, and included within the scope of the Technical Instruction Acts. It is suggested that in each county a committee of ladies should be appointed to devise and carry out a scheme for the technical education of girls.

The *Sussex Daily News* of June 18 records the birth of a sea lion at the Brighton Aquarium.

On June 18, sixty distinct shocks of earthquake occurred at Serajgunge and Domar, in the Bengal Presidency. Many buildings were slightly damaged. At Serajgunge continuous earthquake shocks had been felt from noon on the preceding day.

According to a telegram from Rome, dated June 22, a strong shock of earthquake was felt that morning at Avigliano and at Aquila.

In his report on the Royal Botanic Gardens, Ceylon, for 1890, Dr. Trimen refers to the kinds of cacao in cultivation there. There is no reason to suppose, he says, that they have under cultivation more than one species of *Theobroma*, but every probability that all the varieties trace their origin to a common wild parent. It would be interesting to know which of the two fairly well-marked races recognized in Ceylon is the nearer to this original type, and the facts could probably be ascertained in Central America. The names "Criollo" and "Forastero" applied to them simply mean "wild and foreign," and seem to have had their origin in Trinidad, but it is doubtful if the former was ever really a native plant there. It was, however, the sort at one time exclusively grown in that island, where, having died out, its place was supplied by the "foreign" sort, no doubt obtained from the mainland. As seen in Ceylon, the "Criollo" (called also there "Caracas" and "Old Ceylon Red Cacao") presents very little variety, but the "Forastero" shows a remarkable range in form, size, and colour of pod and seed. No doubt crossing goes on freely in plantations even between the two main races, and it is well known in Ceylon that seed from a single tree gives a very varied progeny; but a curious remark was recently made to Dr. Trimen by a large grower, who has great opportunities for observation, that the "Forastero" varieties, which he chiefly cultivates, appear to be gradually changing their characters and becoming more like the "Old Ceylon Red," the seeds losing their dark colour on section, and becoming pale or nearly white.

In *Himmel und Erde* for June, Prof. G. Hellmann, of Berlin, begins a series of articles entitled "Meteorologische Volksbücher," being an inquiry into popular and typical meteorological works from the earliest times, and into the nature of their contents. The works to be discussed are more particularly those of Germany, although foreign literature will also find subsidiary consideration. Two works are referred to in the present article:—(1) "The Book of Nature," by Konrad von Megenberg, which is the oldest natural history in the German language, and was written about the year 1350—nearly a century before the invention of printing. It was first printed in 1475, and went through many subsequent editions. Much attention and original thought was given to meteorological subjects, and the author divided the wind-rose into 12 points; but the work is to some extent based upon a still unpublished Latin manuscript by Thomas Cantimpratensis, "Liber de natura rerum," which was written before the middle of the 13th century. (2) "Elucidarius." The author of this work is not known with certainty, but is supposed to be Jakob Köbel. This remarkable work was first published in German, in the year 1470, and was much sought for in most European countries in the 15th and 16th centuries. It deals with a variety of subjects, including meteorology and geography, and many editions were published in various countries. Dr. Hellmann gives copious extracts from the works; and historical research being a subject in which he carries great authority, his treatment of it will be found both interesting and instructive.

Messrs. Vieweg and Son, of Brunswick, intend publishing a German translation of Mr. Denning's new book, "Telescopic Work for Starlight Evenings."

A work entitled "Synopsis der Höheren Mathematik," by J. G. Hagen, Director of the Georgetown College Observatory, Washington, D.C., is to be published by Felix L. Dames, Berlin. The work is the result of labour carried on continuously during twenty years, and is intended to present a general view of the higher mathematics. It will consist of four volumes, the first of which will be issued early in August.

A valuable paper on gum-trees, by Mr. D. McAlpine and Mr. J. R. Remfry, has been reprinted from the Transactions of the Royal Society of Victoria for 1890. There are several illustrative plates, the drawings being principally reproductions of photographs taken by Mr. Remfry. These drawings show that the transverse section of the leaf-stalk of a Eucalypt may reveal a pattern useful in the determination of species.

Messrs. George Philip and Son have issued the first number of the *Blue Peter*, a monthly sailing list and review. It is intended that the new journal shall provide ample information for persons who are about to set out by any one of the principal ocean routes. There will also be articles which may serve to remind ships' officers that "there is substantial profit to be derived from a scientific training."

THE third volume of the *Photographic Recorder* is completed by the June number. The volume is admirably illustrated, and contains a valuable record of all that has been done in connection with photography during the past year.

MESSRS. W. F. BROWN AND CO., Montreal, are printing for the Government of Canada "Contributions to Canadian Palæontology," by J. F. Whiteaves, Palæontologist and Zoologist to the Canadian Survey. Part iii. of vol. i. has just been issued. It deals with the fossils of the Devonian rocks of the Mackenzie River basin.

A NOTE by M. Moissan upon the action of fluorine upon phosphorus trifluoride is communicated to the current number of the *Bulletin de la Société Chimique.* A short time ago M. Moissan described a mode of preparing the gaseous trifluoride of phosphorus. The method consisted in gradually adding phosphorus tribromide to warm zinc fluoride, washing the gas first through water, in which it is sparingly soluble, and afterwards drying by means of pumice moistened with sulphuric acid and collecting over mercury. In order to study the action of free fluorine gas upon phosphorus trifluoride as thus prepared, a special piece of apparatus was devised, constructed entirely of platinum and fluor-spar. It consisted of a platinum tube fifteen centimetres long, closed at each end by transparent plates of fluor-spar, through which the phenomena attending the reaction could be observed. The platinum tube was fitted with three side tubes, two of which were placed opposite each other about the centre of the tube, and served for the admission of the fluorine and phosphorus trifluoride respectively; the third or exit tube was of somewhat wider diameter than the entrance tubes, and was bent so as to serve as a delivery tube over a mercury trough. The whole apparatus was first filled with phosphorus trifluoride, and then the fluorine entrance tube was connected with M. Moissan's now well-known apparatus for the preparation of fluorine. As soon as the fluorine came in contact with the phosphorus trifluoride a yellow flame was produced and intense action occurred, with the production of phosphorus pentafluoride. The flame appears to be a comparatively low temperature one. On collecting the gaseous product over mercury, it was found to consist very largely of phosphorus pentafluoride, readily capable of absorption by water, and a small proportion of unaltered trifluoride which could be absorbed by potash. This reaction of fluorine with trifluoride of phosphorus is thus analogous to the conversion of phosphorus trichloride into pentachloride by the action of gaseous chlorine. An interesting reaction has also been observed by M. Moissan to occur between spongy platinum and these gaseous fluorides of phosphorus. When pentafluoride of phosphorus was passed over spongy platinum gently heated in a platinum tube, a partial decomposition was found to occur, and the issuing gas was admixed with trifluoride, and also with free fluorine. The existence of the latter in the free state was abundantly shown by its action upon crystallized silicon. When, however, the temperature of the tube was raised to dull redness, a volatile compound, containing platinum, phosphorus, and fluorine, was obtained, which was carried forward by the gaseous current and deposited in crystals in the cooler portion of the tube. When this crystalline substance is heated, it melts to a viscous liquid, which decomposes at a bright red heat. Analyses show that it is a fluophosphide of platinum, probably of the composition $2PF_3.PtF_4$, analogous to one of the similar chlorine compounds discovered by Schutzenberger, $2PCl_3.PtCl_4$. M. Moissan expresses the hope that by employing some such dissociating compound as this a purely chemical isolation of fluorine may some day be achieved.

THE additions to the Zoological Society's Gardens during the past week include three Stoats (*Mustela erminea*), European, presented by Mr. J. S. B. Borough; an Ocelot (*Felis pardalis* ♂) from South America, a Red-tailed Buzzard (*Buteo borealis*), a Laughing Gull (*Larus atricilla*) from North America, presented by Sir Henry Blake, K.C.M.G.; a Tawny Eagle (*Aquila nævioides*) from Africa, presented by Mr. K. G. Hay; a Blue-fronted Amazon (*Chrysotis æstiva*) from South America, presented by Mrs. A. G. Mussey; a Grey-breasted Parrakeet (*Bolborhynchus monachus*) from Monte Video, presented by Mr. J. R. George; four Common Quails (*Coturnix communis*), British, presented by Mr. J. C. Gie; two Chinese Geese (*Anser cygnoides*) from China, presented by Captain Creaghe; an Egyptian Gazelle (*Gazella dorcas*) from Egypt, two Abyssinian Guinea Fowls (*Numida ptilorhyncha*) from Abyssinia, two Blossom-headed Parrakeets (*Palæornis cyanocephalus*) from India, a Meyer's Parrot (*Pœocephalus meyeri*) from East Africa, three Tibetan Crossoptilons (*Crossoptilon tibetanum*) from Tibet, a Temminck's Tragopan (*Ceriornis temmincki* ♂) from China, deposited; a Vinaceous Amazon (*Chrysotis vinacea*), from Brazil, purchased; two Heloderms (*Heloderma suspectum*) from Arizona, U.S.A., received in exchange; a Burrhel Wild Sheep (*Ovis burrhel*), two Mule Deer (*Cariacus macrotis* ♂ ♀), a Bennett's Wallaby (*Halmaturus bennetti* ♂), two Impeyan Pheasants (*Lophophorus impeyanus*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

TRANSIT OF MERCURY.—The Government Astronomer at Sydney (Mr. C. Todd, C.M.G.) writes as follows regarding the transit of Mercury:—Good observations of the transit of Mercury were secured at the Observatory, on Sunday the 10th. At the ingress the conditions were extremely favourable, the sun's limb and the planet when projected on the sun's disk being exceedingly well and sharply defined, but at the egress the sun's limb was boiling and the planet was somewhat woolly, rendering it difficult to fix the exact time of internal contact. I observed with the 8-inch equatorial refractor, assisted by Mr. Cooke; and Mr. Sells observed with an 8-inch reflector.

The observations were as follow:—

Observer—C. Todd. Power 125.

INGRESS.—*External Contact.*

	Times.		
	h.	m.	s.
A. About one-third on	9	10	11
Internal Contact.			
B. Contact tangential	9	13	6·5
C. Black drop still clinging to limb	9	13	22·0
D. Rupture of black drop; planet clear of limb	9	13	49·5
EGRESS—Power 80. *Internal Contact.*			
E. Formation of black drop touching limb ...	2	0	14·1
F. Tangential contact	2	0	43·8
External Contact.			
Indentation still visible	2	4	14·8
,, ,, barely noticeable	2	4	25·8
Sun's limb complete	2	4	31·8

Observer—Mr. Sells.

INGRESS.—*Internal Contact.*

	h.	m.	s.
a. Planet nearly on disk, but not quite ...	9	12	51·3
b. True contact, momentarily seen	9	13	13·2
c. Planet pear-shaped; point of pear touching sun's limb	9	13	50·7
EGRESS.—*Internal Contact.*			
a. Pear-shaped contact	2	0	34·6
b. True contact	2	1	28·6
External Contact.			
c. Last seen; or sun's limb judged to be complete...	2	4	48·6

OBSERVATIONS OF TELLURIC LINES.—The May number of the *Memorie della Società degli Spettroscopisti Italiani* contains a paper by G. B. Rizzo on the telluric lines in the solar spectrum. Signor Rizzo has compared the intensities of the lines A, B, and α at Bosco Nero and on the Rocciamelone Mountain. In order to express the variation in the mass of air (ϵ) traversed, calculations have been made of the values at the different altitudes of $P \sec \zeta$, where P is the atmospheric pressure, and ζ is the sun's zenith distance. The following is a comparison of the values of ϵ and the mean intensities of the lines at the two stations. The scale of intensity is such that the C line = 10, and the line at 651·55 is unity.

Place of observation.	Altitude.	ϵ.	Intensities of the lines A.	B.	a.
Bosco Nero ...	1623 metres ...	1046·2 ...	50	28	3·2
Rocciamelone ...	3538 ,, ...	846·2 ...	40	20	2·2

A comprehensive bibliography of the subject accompanies the paper.

SIMILARITY OF THE ORBITS OF CERTAIN ASTEROIDS.—In the *Publications of the Astronomical Society of the Pacific*, No. 15, 1891, Prof. Daniel Kirkwood gives a list of twenty-four asteroids arranged in ten groups, according to the similarity of their orbits. The following are the groups:—

I.	84 Clio. 115 Thyra. 249 Ilse.	VI.	3 Juno. 97 Clotho.
II.	19 Fortuna. 79 Eurynome.	VII.	203 Pompeia. 200 Dynamene.
III.	134 Sophrosyne. 193 Ambrosia.	VIII.	278 Pauline. 116 Sirona. 1 Ceres.
IV.	37 Fides. 66 Maia.	IX.	245 Vera. 86 Semele. 106 Dione.
V.	218 Bianca. 204 Callisto. 246 Asporina.	X.	121 Hermione. 87 Sylvia.

Jupiter is held responsible for the perturbations necessary for the development of these groups of asteroid orbits from the primitive solar nebula.

ASTRONOMICAL AND PHYSICAL SOCIETY OF TORONTO.—The first number of the Transactions of this Society (1890-91), with which is also included the first Annual Report, has recently been issued. It contains abstracts of several interesting papers read at the meetings, among which is one on the disappearance of Saturn's rings, by Dr. Morrison, two by Mr. Shearman on coronal photography, and two by Mr. A. F. Miller on the spectroscope. A drawing of a sun-spot observed on November 30, and a hydrogen prominence measured on August 3, forms the frontispiece of the number.

A NEW ASTEROID (311).—On June 11 M. Charlois discovered the 311th asteroid. Its magnitude was 13.

THE ROYAL SOCIETY CONVERSAZIONE.

THE Ladies' *Soirée* of the Royal Society was held on the 17th instant, and was very numerously attended. The following were among the chief objects exhibited:—

Finger-prints as a means of identification, exhibited by Mr. Francis Galton, F.R.S. (1) Specimens showing the nature and character of the patterns that are formed by the papillary ridges on the bulbs of the fingers, as well as on the rest of the inner surfaces of the hands and feet. (2) Evidence of the persistence of the patterns in their essential details, however minute, from infancy to age. (3) Method of indexing a collection of finger-prints so that a determination may be quickly arrived at, whether the duplicate of a given specimen is contained in it or not. (4) Process of making finger-prints, exhibited in operation.

Registration of colours in numbers, and apparatus to show the greater sensitiveness of the eye to different colours, exhibited by Captain Abney, C.B., F.R.S., and General Festing, F.R.S. The registration consists in referring any mixed colour to a single wave-length, and a percentage of white light. With the apparatus to show the greater sensitiveness to the eye of different colours, a comparison is made by placing two colours side by side, which are at ordinary intensity of equal luminosity, and by then diminishing the intensity of each equally.

An optical illusion, exhibited by Prof. Silvanus P. Thompson, F.R.S. On two rotating disks, A and B, are spiral patterns in black and white, which seem to move radially inwards and outwards respectively. Let the observer gaze fixedly for about one minute at the centre of A, and then suddenly transfer his gaze to any object—say the face of a friend—he will see that object apparently enlarging from the middle outwards. After similarly gazing for a minute at B, and then looking at any object, he will see it apparently diminishing.

Discharge without electrodes through gases, exhibited by Prof. J. J. Thomson, F.R.S. The discharge tube in these experiments is made to form the secondary of what is essentially an induction coil, and the discharge passes round a closed current in the gas. Experiments *a*, *b*, *c*, *d* show various forms of the discharge in tubes and bulbs. *e* shows the residual glow produced when the discharge passes through oxygen. *f* shows the action of a magnetic field on the discharge; along the lines of force the discharge is facilitated, while at right angles to them it is retarded. When the magnetic field is "off," the discharge takes place in the bulb, and not in the tube; when the field is "on," in the tube, and not in the bulb. *g* illustrates the stoppage of the discharge when a gas electrically weaker than that in the discharge tube is placed in the neighbourhood of the latter.

A nickel pendulum, illustrating the effect of heat upon the magnetic susceptibility of nickel, exhibited by Mr. Shelford Bidwell, F.R.S. Nickel, which at ordinary temperatures is a magnetic metal, becomes non-magnetizable at about 300° C. A copper disk, to which a projecting tongue of nickel is attached, hangs like the bob of a pendulum from a double thread, and is deflected to one side by a magnet which attracts the nickel tongue. The heat of a spirit-lamp placed beneath the tongue quickly destroys the magnetic quality of the nickel, so that the magnet can no longer hold it; the bob accordingly falls back and performs an oscillation. On its return to the neighbourhood of the magnet, however, the tongue has cooled sufficiently to be once more attracted, but after a momentary contact it is again released, and the process is repeated. Thus the bob can be kept swinging like the pendulum of a clock.

The meldometer, exhibited by Mr. J. Joly. This instrument is for determining the melting-points of minute quantities of substances, by comparison with bodies of known melting-point. The method consists in measuring the thermal expansion of a ribbon of pure platinum when a minute quantity of a substance, dusted on its surface (and observed through a microscope), is melting. The platinum is heated by a current, and the thermal value in degrees Centigrade of its expansion found by preliminary observations, using bodies of known melting-point. The expansion of the ribbon is read by an electric-contact method. The instrument shown reads a change of 2° C. Range up to 1600° C. about. Quartz may be melted on the meldometer, and most or all of the silicated minerals.

Facsimile drawings of paintings from tombs at Beni Hasan, Upper Egypt, exhibited by Mr. Percy E. Newberry (of the Egypt Exploration Fund). A series of facsimile drawings in colour, executed by Mr. M. W. Blackden, of some of the most interesting paintings on the walls of the tombs of Ameni and Khnumhotep (XII. Dynasty, *circa* 2500 B.C.), at Beni Hasan, in Upper Egypt. These drawings are the property of the Egypt Exploration Fund.

Instrument for examining the strains in bent glass beams, exhibited by Prof. C. A. Carus-Wilson. There is a steel straining frame in which the beam to be examined is placed; this frame can be moved in any direction in its own plane between two Nicol prisms. The Nicol prisms can be rotated through any required angle. When the beam has been supported in any given manner, load is applied by a screw, and the action of the strained glass on the polarized light enables the precise state of strain all over the beam to be ascertained. The instrument has been used to determine the action of "surface loading," and to show to what extent this action affects the state of strain in beams supposed to obey the Bernoulli-Eulerian theory of flexure.

Cup-micrometer, an instrument for measuring the rate of growth of a plant, exhibited by Mr. Francis Darwin, F.R.S. A thread is attached to the upper end of the plant, passes over a pulley, and is fastened to a weight. The descent of the weight (which is a measure of the growth of the plant) is estimated by adjusting a micrometer screw carrying a small cup of oil, until a needle point on the weight touches the surface of the fluid. The method, a modification of that used by physicists to measure the rise or fall of a fluid surface, was designed by Mr. H. Darwin, of the Cambridge Scientific Instrument Company.

Electrical volatilization of metals, exhibited by Mr. W. Crookes, F.R.S.

Living animals from the aquarium of the Marine Biological Association at Plymouth, exhibited by the Marine Biological Association.

Art metal work, from the factories of Messrs. Tiffany and Co., in New York, exhibited by Messrs. Tiffany and Co. Representative articles in wrought metals; amalgamation of metals; enamelling on silver and gold.

Photographs of living corals taken in Torres Straits, exhibited by Mr. W. Savile Kent.

Prof. J. Norman Lockyer, F.R.S., exhibited:—(1) Photographs of a group of sun-spots. A series of enlargements of a group of sun-spots shown on the 12-inch sun-pictures taken under the direction of Lieut-Colonel Strahan, at Dehra Dun, India, on December 16, 18, 19, 20, 21, 22, 23, 1887. The spots have been enlarged three times, and it will be seen that great changes took place during the period of visibility.—(2) Photographs of the temples at Karnak and Edfou. These are enlargements from photographs taken in January 1891, with reference to the orientation of the temples. The photographs show that, notwithstanding the elaborate details of the architecture, the principal axes of the temples were kept perfectly clear from one end to the other.

Prof. W. Roberts-Austen, C.B., F.R.S., exhibited a new, brilliantly coloured alloy of gold and aluminium, and facsimiles of medals asserted to be of gold and of silver, transmuted from base metal by the aid of alchemy. One of the medals bears on its reverse the statement that it was struck in 1675, by J. J. Becher, in silver transmuted from lead.

Mr. Ludwig Mond, F.R.S., exhibited:—(1) Nickel-carbon-oxide. (2) Pure nickel extracted from nickel ores by means of carbonic oxide. (3) Articles of pure nickel deposited from nickel-carbon-oxide, and goods plated with nickel by exposure to nickel-carbon-oxide [$Ni(CO)_4$]. This unique chemical compound was obtained in 1890 by Mond, Langer, and Quincke, by passing a current of carbonic oxide over finely-divided metallic nickel at the ordinary temperature, and refrigerating the resulting gas. It is a colourless liquid, of high refractory power, boiling at 43° C., and solidifying at 25° C., and is split up again into nickel and carbonic oxide on heating its vapour to 180° C. It is highly poisonous; while according to Prof. McKendrick's researches it has, when injected subcutaneously in very small doses, a remarkable power of reducing the temperature of animals. The properties of this substance make it possible to volatilize nickel at a low temperature, and to extract it industrially in a perfectly pure state from all other substances with which it is found. Articles of pure nickel, and goods plated with pure nickel, are produced by exposing heated moulds or goods to nickel-carbon-oxide vapour, or to a solution of this compound in suitable solvents.

Specimens of Japanese metal work, including *Ojimé*, or sliders, *Yanoné*, or arrowheads, and *Tsuba*, or sword-guards, exhibited by Prof. A. H. Church, F.R.S.

Prof. A. Newton, F.R.S., exhibited a drawing, the first received in Europe, of *Notoryctes typhlops*, a new form of Marsupial of mole-like habit, and structure accordingly, sent by Prof. E. C. Stirling, of the University of Adelaide, South Australia. The first specimen of this remarkable mammal, one of the most unexpected discoveries for many years, was sent from the interior of South Australia by Mr. A. Molineux to Prof. Stirling, of Adelaide, who contributed to NATURE (vol. xxxviii. pp. 588, 589) such a notice of it as its imperfect condition admitted. He afterwards obtained other examples, which are fully described in a memoir communicated to the Royal Society of Adelaide. "Four or five of the cervical vertebræ are fused, and there is a keeled sternum. An enormously thick and short first rib, which serves the purpose of buttressing the sternum in lieu of coracoids. Eyes mere pigment spots, underneath the skin and temporalis muscle. It has a remarkable habit of burrowing for long distances in the sand with great rapidity." These specimens were obtained about 1500 miles north of Adelaide, but a telegram from Prof. Stirling, dated May 31, 1891, states that he has himself obtained others in the course of a journey, just completed, across the continent from Port Darwin.

Mr. Walter Gardiner, F.R.S., gave demonstrations of certain important phenomena associated with the absorption and the flow of the water taken up by plants:—(1) Root pressure. Water present in the soil, and containing minute traces of nutritive salts, is absorbed by the root-hairs so powerfully and in such quantities as to set up a considerable pressure in the interior of the plant. This "root pressure" may be demonstrated by attaching to the cut end of a stem a manometer containing mercury, or some coloured fluid. Here a solution of nigrocine in water is employed. (2) The transpiration current. Among the more important factors which determine the flow and ascent of water from the root, upwards, is the sucking force induced by the modified evaporation or transpiration of water from the general free surface of the leaves. During transpiration the water escapes as vapour, and the salts are retained for food. In this experiment the existence of a "transpiration current" is shown by allowing a cut branch to suck up milk, when the movement of the fat globules registers the flow of the liquid. (3) The amount of water absorbed by the root. This may be estimated by simple measurement, employing some such form of apparatus as that exhibited.

Engravings to "Travels among the Great Andes of the Equator," exhibited by Mr. Edward Whymper. These illustrations are selections from Mr. Edward Whymper's forthcoming work upon the Great Andes of the Equator (in which he gives accounts of the first ascents of Chimborazo, Cayambe, Antisanal, &c., &c.), and includes views on and about the equator at great elevations; incidents of travel; numerous examples of the new genera and species obtained on the journey; a facsimile reproduction of the map of Don Pedro Maldonado (upon which existing maps of Ecuador are based), and the original route survey, and map of Chimborazo, made by the author. The work, with 200 illustrations and four maps, will be published in the present year by Mr. John Murray.

Mr. W. Bateson exhibited (1) models of double supernumerary legs and antennæ in beetles; (2) mechanical model showing the usual symmetry of double supernumerary appendages in beetles. Supernumerary appendages in beetles nearly always spring as branches from a normal appendage, and are generally double, being made up of two limbs more or less compounded together. The two extra limbs are always *a complementary pair*, one being structurally a right limb, while the other is left. Commonly the symmetry of the parts is arranged as follows:—(*a*) The two extra limbs and the normal one stand in one plane, one of the extra limbs being nearer to the normal limb and one remoter from it. (*b*) The nearer is in structure and position an image of the normal limb in a mirror at right angles to the plane in which the three limbs stand; and the remoter is an image of the nearer in another mirror beyond and parallel to the first. Thus the relations of the parts in their several positions may be represented by the mechanical model exhibited, in which the extra legs, revolving round the normal leg, take attitudes proper to the positions which they occupy relatively to the normal leg.

Prof. A. C. Haddon exhibited the geographical distribution, and the progressive and retrogressive evolution, of art and ornament in British New Guinea. The exhibit is designed to show that savage art can be studied as a branch of biology, and that it is only when so treated that it yields its most valuable results. Most savage and barbaric designs have only a very limited geographical range, and those which have a wide distribution can, in the majority of cases, be proved to be homoplastic and not homogenetic. The evolution of a particular pattern must be sought in the district in which it occurs, and its developmental history can only be safely attempted when a comparison is made of numerous objects from the same locality. The foregoing propositions are illustrated by means of specimens, rubbings, photographs, and sketches of decorated objects from British New Guinea.

At intervals during the evening, the Edison loud-speaking telephone and Bell's receivers were connected with the performance of "The Gondoliers," at the Savoy Theatre, London; the Prince's Theatre, Birmingham; and with vocal and instrumental concert rooms at Liverpool and Birmingham.

Photographs of volcanic phenomena were exhibited by Dr. Tempest Anderson during the evening. These photographs of volcanic phenomena were taken last year during a visit to the Skaptâ Jokul, and other volcanic districts in Iceland. The eruption of the Skaptâ Jokul, in 1783, was one of the largest on record. A mass of lava, estimated to be equal in bulk to Mont Blanc, flowed out in two streams, each forty to fifty miles long. The actual craters situated in the desert interior of the island appear not to have been previously visited.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following are the speeches delivered by the Public Orator (Dr. Sandys, Fellow and Tutor of St. John's

College) on June 16, in presenting for the honorary degree of Doctor in Science Sir Archibald Geikie, F.R.S., Director-General of the Geological Survey of Great Britain and Ireland; Mr. W. H. Flower, C.B., F.R.S., Director of the Natural History Museum; and Dr. Elias Metschnikoff, *Chef de Service* of the Institut Pasteur, Paris.

Salutamus deinceps virum et scientiarum et litterarum laude illustrem, in Academia Edinensi quondam Geologiae Professorem, Britanniae et Hiberniae explorationi geologicae praepositum, societatis Regiae socium, societatis geologicae praesidem, societatis denique Britannicae scientiarum terminis prorogandis praesidem designatum. Geologiae et geographiae studiosorum in manibus sunt scripta eius plurima, scientiis illis aut docendis aut illustrandis destinata. Etiam aliis loquuntur libri eius elegantissime conscripti, quorum in uno Caledoniae montes vallesque per immensam saeculorum seriem causis cotidianis minutatim exsculptas fuisse demonstrat; in altero vitam et res gestas geologi magni, quem Siluriae regem nominaverim, ea quae par est dignitate describit. Viri talis laboribus non modo geologiae fines latius indies propagantur, sed etiam populo universo studia illa praeclara commendantur.

Duco ad vos geologum illustrem, ab ipsa Regina nuper novo honore ornatum, ARCHIBALDUM GEIKIE.

Quod e sapientibus septem unus dixisse fertur, *ἀρχὴ ἄνδρα δείξει*, de hoc certe viro, per honorum cursum satis longum probato, verum esse constat. Regio Chirurgorum in Collegio, primum Museo conservando praepositus, deinde physiologiam et comparativam quae dicitur anatomiam professus, deinceps Musei Britannici aedificio novo rerum naturae studiis dedicato praefectus est. Idem societati et zoologicae, et anthropologicae, et Britannicae, maxima cum laude praefuit. In Museis autem ordinandis quam perspicax; in scientiarum studiis populo toti commendandis quam disertus; hominum in diversis generibus capitis mensura inter sese distinguendis quam subtilis; maris denique in monstris immensis describendis quam minutus. Ergo, velut alter Neptunus, intra regni sui fines etiam "immania cete" suo sibi iure vindicat: idem, anthropologiae quoque in studiis versatus, ne barbaras quidem gentes contempsit, sed, velut alter Chremes, homo est; humani nil a se alienum putat.

Duco ad vos Regiae societatis socium, virum honoribus plurimis merito cumulatum, WILELMUM HENRICUM FLOWER.

Sequitur deinceps vir, qui scientiarum in provinciis duabus, et in zoologia et in bacteriologia quae dicitur, famam insignem est adeptus. Primum Ponti Euxini in litore septentrionali zoologiam professus, multa de morphologia animalium, quae invertebrata nominantur, accuratissime disseruit. Deinde Parisiis rerum naturae investigatori celeberrimo adiutor datus, eis potissimum causis perscrutandis operam dedit, per quas genere ab humano morborum impetus hostiles possent propulsari. Nam, velut hominum in mentibus virtutes et vitia inter sese confligunt, non aliter animantium in corporibus contra pestium exercitus copiae quaedam sanitatis et salutis ministrae concertare perhibentur. Mentis quidem certamen olim in carmine heroico, Psychomachia nominato, Prudentius narravit. Inter eos autem qui corporis certamen experimentis exquisitis nuper explicaverunt, locum insignem sibi vindicat vir quidam summa morum modestia praeditus, qui, velut vates sacer, proelium illud sibi sumpsit celebrandum, in quo tot cellulae vagantes, quasi milites procursantes, morborum semina maligna corripiunt, correpta comprimunt, compressa extinguunt. Talium virorum auxilio febrium cohortes paulatim profligantur, et generis humani saluti novum indies affertur incrementum.

Merito igitur titulo nostro hodie coronatur e salutis humanae ministris unus, ELIAS METSCHNIKOFF.

AT the annual election at St. John's College on June 22 the following awards in Natural Science were made:—Foundation Scholarships, continued or increased: P. Horton-Smith, Hewitt, Blackman, Woods, MacBride, Whipple. Foundation Scholarship awarded: Villy. Exhibitions: Purvis, Trotman. Hughes Prize: MacBride. Wright's Prize: Villy. In the Natural Sciences Tripos, Part II., Capstick, of Trinity, has been awarded "special distinction" in two subjects, Chemistry and Physics. It is many years since this last occurred. MacBride, of St. John's (Zoology, Botany), and Krishnan, of Christ's (Chemistry, Botany), have gained first classes in two subjects. Of the women candidates, Miss Elliot, of Newnham (Zoology), and Miss Tebb, of Girton (Physiology), have gained first class honours.

SCIENTIFIC SERIALS.

American Journal of Science, June.—The study of the earth's figure by means of the pendulum, by E. D. Preston. The author first deals with the history of the subject, then states the quantities involved, and supports the method of study in which the figure of the earth is considered separately from its size as determined by measurement of arcs of meridian. The general results of pendulum work are discussed, and the effect of continental attraction and variations in latitude referred to. The best methods of determining the duration of a pendulum oscillation at a given temperature and pressure are also considered.—On the post-glacial history of the Hudson River valley, by Frederick J. H. Merrill. The result of the action of waves upon a shore depends upon the state of rest or movement of the shore. If the land is subject to alternate periods of rest and elevation, a series of terraces will be formed; if the land is slowly rising or subsiding with respect to sea-level, an inclined plane of erosion may be produced. Arguing from this and other facts, the author states provisionally that, after the retreat of the continental glacier from the Hudson River valley, the land stood for a long time at a lower level than at present. A gradual elevation and extensive erosion of the Champlain estuary deposits in the river valley then occurred, and was followed by a depression amounting to about 100 feet at New York, and which is apparently continuing at the present day.—On alunite and diaspore from the Rosita Hills, Colorado, by Whitman Cross.—Diaspore crystals, by W. H. Melville.—Combustion of gas jets under pressure, by R. W. Wood. Anyone who has watched a burning jet of ether vapour will have noticed that, as the pressure increases, the flame gradually retreats from the orifice and eventually goes out if the pressure is carried beyond a certain point. The author has investigated these phenomena, using various gases. A burning jet of coal gas was extinguished when the pressure was equal to 23 centimetres of mercury—that is, when the velocity of the issuing gas exceeded the speed of combustion for the mixture of gas and air.—Allotropic silver: Part iii., blue silver, soluble and insoluble forms, by M. Carey Lea. From the results given in this and preceding papers, the author is led to believe that allotropic and even soluble silver may be formed in numerous ways. The reducing agents may be either a ferrous or a stannous salt, or any one of a variety of organic substances of very different constitutions. From the solubility and activity of this substance, and the parallelism which many of its reactions show to those of silver in combination, it appears probable that silver in solution, like silver in combination, exists in the atomic form.—Note on the submarine channel of the Hudson River, and other evidences of post-glacial subsidence of the middle Atlantic coast region, by A. Lindenkohl.—Are there glacial records in the Newark system?, by Israel C. Russell. Facts are adduced in support of the negative view.—A reply to Prof. Nipher on the theory of the solar corona, by F. H. Bigelow.—On the recent eruption of Kilauea, by W. T. Brigham. This is a report of the changes that took place in the crater of Kilauea during March of this year.—Turquoise in south-western New Mexico, by Charles H. Snow.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 18.—"Results of Hemisection of the Spinal Cord in Monkeys." By Frederick W. Mott, M.D., B.S., M.R.C.P. Communicated by Prof. Schäfer, F.R.S.

While engaged in studying experimentally the connections of the cells of Clarke's column with the ascending tracts of the spinal cord in the monkey, I was surprised to find that after hemisection in the lower dorsal region the sensory disturbances produced in no way corresponded with those already obtained by eminent observers.

I was therefore led to continue my experiments, and, by the kind permission of Prof. Schäfer, I carried them out in the Physiological Laboratory of University College. My thanks are also due to him for much valuable advice and assistance.

The subject is one of great importance from a scientific, as well as from a clinical, point of view. Some years ago, a case occurred in my practice which tended to shake my faith in the absolute truth of the doctrine of complete and immediate decussation of sensory impulses in the spinal cord, as taught by Brown-Séquard.

The experiments which I have performed exhibit the following principal points of interest :—

(1) Return of associated movements after complete destruction of the crossed pyramidal tract below the lesion.

(2) That all sensory impulses do not decussate in the cord—in fact, they appear to show that certain sensory impulses, *e.g.* touch, the muscular sense, and localization in space, pass chiefly up the same side, painful impressions up both sides. A peculiar condition known as "allochiria" occurs after hemisection.

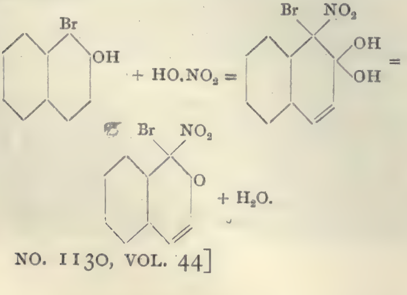
(3) The vaso-motor disturbances are *on the same side as the lesion*, and consist of vaso-dilation, swelling of the foot, and redness with rise of temperature of the skin of the foot (as compared with the opposite side), and fall of temperature in the popliteal space on the side of the lesion, due, no doubt, to paralysis of the muscles.

(4) The degenerations above and below the lesion are limited to the same side when the injury is perfectly unilateral. There are certain facts connected with the degenerations which serve to show the origin and course of certain long and short tract fibres.

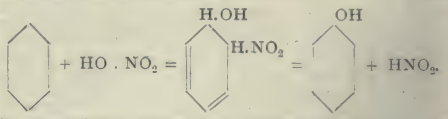
(5) Stimulation of the cortex cerebri on both sides some weeks or months after the hemisection had been performed gave, as a rule, *results* which showed that the block in the spinal cord produced by the hemisection still existed, although there had been a very complete return of associated movements.

(6) In one case ablation of the leg area on the same side as the lesion in the spinal cord was performed many months afterwards.

Chemical Society, May 21.—Prof. A. Crum Brown, F.R.S., President, in the chair.—The following papers were read :—Bromo-derivatives of betanaphthol, by H. E. Armstrong and E. C. Rossiter. The authors have completed the study of the compounds formed on brominating betanaphthol, to which they have referred in two previous notices (Chem. Soc. Proceedings, 1889, p. 71 ; 1890, p. 32). In the present paper they give directions for preparing tri- and tetra-bromobetanaphthol, and summarize the properties of the bromobetanapthols. The entire product of the action of bromine in excess on betanaphthol, has been carefully examined without any substance having been discovered which affords 1 : 2 : 3-bromophthalic acid on oxidation ; the discrepancy between the authors' observations and the earlier experiments of Smith and Meldola, therefore, yet remains to be discovered.—The action of nitric acid on naphthol derivatives as indicative of the manner in which nitration is effected in the case of benzenoid compounds generally : the formation of nitro-keto-compounds, by H. E. Armstrong and E. C. Rossiter. The chloro- and bromo-derivatives of betanaphthol when warmed with nitric acid are converted into derivatives of betanaphthoquinone ; but the formation of these compounds is preceded by that of an unstable intermediate compound. These intermediate compounds, when carefully heated, are converted into derivatives of betanaphthaquinone. Thus, when nitric acid is added to dibromobetanaphthol, suspended in acetic acid, a clear solution is obtained which, after a short time, deposits a crystalline substance ; if quickly evaporated by filtration, this product is almost colourless, but it decomposes when kept, becoming yellow. This compound, when treated with alkali, yields bromonitronaphthol. Bromobetanaphthol, in like manner, yields α_1-nitrobetanaphthol, and the tri- and tetra-bromo-derivatives yield di- and tri-bromonitrobetanaphthol. The authors are of opinion that the intermediate compounds in question are nitro-bromo-keto-derivatives, and that their formation affords evidence that the elements of nitric acid first become added to the bromonaphthol, thus :—



The theory that the formation of such addition-compounds precedes that of nitro-compounds generally, appears to afford a satisfactory explanation of a number of well-known facts which hitherto have remained unexplained. The non-production of nitro-compounds from paraffins and their derivatives appears as the natural consequence of the inability of paraffins to form addition-compounds. The theory affords a simple explanation of the formation of nitro-derivatives of *phenols* on nitrating hydrocarbons, for if the addition-compound lose $H.NO_2$ instead of $H.OH$ a phenol would result, thus—



An agent which would tend to withdraw water from the addition-compound would increase the production of nitro-compound and diminish that of phenol ; and it is known that when a mixture of nitric and sulphuric acids is used, there is less of the phenol derivative produced than when nitric acid alone is employed. A compound like the addition-compound of benzene, represented above, would obviously be unstable, and prone to undergo oxidation ; hence the explanation of the large amount of nitrous fume produced on nitrating benzene. The non-production of resinous matters when sulpho-acids are treated with nitric acid to form the corresponding nitro-compound by displacement of the SO_3H group by NO_2 is also elucidated by the authors' theory ; the addition-compound formed in such a case would very readily break up into sulphuric acid and the nitro-derivative.—A new method of preparing nitro-derivatives, and the use of nitrogen dioxide as a nitrating agent, by H. E. Armstrong and E. C. Rossiter. The authors find that the unstable compounds formed by the addition of the elements of nitric acid to the bromo-derivatives of betanaphthol yield nitro-derivatives of the naphthol on treatment with alkali, a bromine atom becoming displaced by NO_2. On treating the addition-compound with sulphurous acid, a practically theoretical yield of the nitronaphthol is obtained ; this method appears to be of general application. The authors have been naturally led to study the action of nitrogen-dioxide, NO_2, on unsaturated compounds of various kinds, in the hope of obtaining addition-compounds which by loss of HNO_2 would pass over into nitro-derivatives of the substances treated. They find that such addition-compounds are obtained, and on treatment with alkali and reducing-agents yield nitro-compounds. Thus betanaphthol yields 75 per cent. of its weight of nitro-betanaphthol ; alphanaphthol behaves similarly. Phenol yields ortho- and para-nitrophenol. The authors propose to study the action of nitric acid and nitrogen dioxide on unsaturated compounds generally from the point of view indicated in this and the previous note.—Nitrification, by R. Warington. The first section of the paper describes early experiments, showing the existence of an agent producing only nitrites, and the means of separating it from soil. Successive cultivation in ammoniacal solutions made permanently alkaline with disodium carbonate was found to be a certain method of obtaining a purely nitrous agent. Pasture soil yielded the nitrous agent more readily than arable soil. The nitrous organism was isolated by the dilution method. Cultivations were made in an ammonium chloride solution with calcium carbonate. The nitrous organism oxidizes ammonia to nitrous acid, and has no effect on nitrites. It produces nitrous acid in solutions of asparagine, milk, urine, and urea. Grown in broth containing calcium nitrate, it does not reduce the nitrate to nitrite. It requires no organic matter for its nutrition, and is apparently capable of assimilating carbon from acid carbonates. The presence of either calcium or sodium acid carbonate distinctly favours nitrification ; neutral sodium carbonate greatly hinders nitrification. The nitrous organism occurs as nearly circular corpuscles, which stain deeply. It also occurs as oval cocci, the ends occasionally more or less truncated. The remainder of the paper deals with the nitric organism. The results show that the nitric organism develops freely in inorganic solutions containing potassium nitrite, phosphates, &c., especially if supercarbonates are present. Monosodium carbonate, 1–4 grams per litre, exerted a very favourable influence ; 6 grams per litre, a retarding influence. Disodium carbonate greatly hinders the action. The nitric organism produces neither nitrites nor nitrates in ammoniacal solution. In the absence of ammonia, it energetically converts nitrites into nitrates; the presence of ammonia is apparently a great hindrance to its action. An attempt to isolate the organism failed. The nitrification performed by soil thus appears to be the work of two organisms, one of which oxidizes ammonia to nitrite, while the other oxidizes nitrite to nitrate.

Geological Society, June 10.—Sir Archibald Geikie, F.R.S., President, in the chair.—Before the commencement of the general business, Prof. Blake rose on behalf of those present at the meeting to congratulate the President on the honour that it had pleased Her Majesty to confer upon him. No one who knew him could fail to appreciate how thoroughly it was deserved; and the Geological Society would doubtless feel also the honour conferred on their science in the person of their President and the head of the Geological Survey of the United Kingdom.—The following communications were read:—Note on some recent excavations in the Wellington College district, by the Rev. A. Irving.—Notes on some post-Tertiary marine deposits on the south coast of England, by Mr. Alfred Bell. Communicated by Mr. R. Etheridge, F.R.S. The author's object in this paper is to trace the successive stages in the development of the present coast of the north side of the English Channel, and to ascertain the sources of the diversified faunas. The first traces of marine action on the south coast in post-Tertiary times, are found on the foreshore in Bracklesham Bay. The author's reading of the section is somewhat different from that of the late Mr. Godwin-Austen; and he divides the marine series into (1) an estuarine clay with Mollusca common to estuarine flats; (2) a compact hard mud; and (3) a bed of fine sandy silt with many organisms. These beds indicate a change from estuarine to deep-water conditions. A full list of the Selsey fossils is given, including, amongst other animals, upwards of 200 Mollusca. Of 35 species of Mollusca not now living in Britain, the majority exist in Lusitanian, Mediterranean, or African waters; furthermore, nearly 45 per cent. of the Mollusca are common to the older Crags of the eastern counties. The author considers the fauna of the Portland Bill shell-beds to indicate the further opening of the Channel subsequent to the formation of the Severn Straits, and believes that this fauna represents the deposits wanting between the Selsey mud-deposits and the erratic blocks which, according to him, overlie the mud; these Portland shells indicate an intermediate temperature, "rather southern than northern," according to Dr. Gwyn Jeffreys. In conclusion, details concerning still newer beds are given, and lists of fossils found therein; and the author observes that there is no evidence to show when the English Channel finally opened up, beyond the suggestion of Mr. Godwin-Austen that, if the Sangatte beds and the Coombe Rock are of the same period, it must have taken place after their formation. After the reading of this paper some remarks were made by Mr. Etheridge, Mr. C. Reid, Prof. Hull, and the author.

Mathematical Society, June 11.—Prof. Greenhill, F.R.S., President, in the chair.—The following communications were made:—Systems of spherical harmonics, by E. W. Hobson.—On the motion of a liquid ellipsoid under its own attraction, by Dr. M. J. M. Hill.—On certain properties of symmetric, skew-symmetric, and orthogonal matrices, by Dr. H. Taber.—An application of the method of images to the conduction of heat, by G. H. Bryan.—A property of the circum-circle, by R. Tucker.

CAMBRIDGE.

Philosophical Society, June 1.—Prof. G. H. Darwin, President, in the chair.—The following communications were made:—On the part of the parallactic series of inequalities in the moon's motion which is a function of the ratio of the mean motions of the sun and moon, by Mr. Ernest W. Brown.—On Pascal's hexagram, by Mr. H. W. Richmond. The author applies Cremona's method of deriving the hexagram by projection of the lines on a nodal cubic surface from the node. By use of a new form of the equation to this surface the equations of the lines are obtained in a perfectly symmetrical form, and their properties thence developed.—A linkage for describing lemniscates and other inverses of conic sections, by Mr. R. S. Cole.—Some experiments on liquid electrodes in vacuum tubes, by Mr. C. Chree. This paper describes some experiments undertaken at the suggestion of Prof. J. J. Thomson on the electric discharge through vacuum tubes in which one or both of the electrodes were liquid surfaces. The liquids employed were mercury and sulphuric acid. The electrodes when solid were of platinum or aluminium. Observations were taken of the differences presented by the discharge when the substance of an electrode was altered. The experiments were mostly at low gaseous pressures, and included observations on the character of the phosphorescence then accompanying the discharge.—On gold-tin alloys, by Mr. A. P. Laurie.—Note on a problem in the linear conduction of heat, by Mr. G. H. Bryan.

EDINBURGH.

Royal Society, June 1.—Prof. Chrystal, Vice-President, in the chair.—Prof. Tait communicated a paper, by Prof. Piazzi Smyth, on two series of enlarged photographs, one in the visible, the other in the invisible, of the violet of the solar spectrum. The paper was accompanied by the photographs. The observations include part of the spectrum as previously observed by Mr. Smyth in the summer of 1884, and extend to an extreme distance in the invisible violet. The previous observations were included in sixty plates; in the present series, twelve more plates are added in the violet region, and two independent photographs of each part have been taken. The photographs agree with those of Prof. Rowland in indicating that the Fraunhofer line, "little *d*," is either entirely absent now from the solar spectrum, or has become very unimportant.—Mr. R. Kidston read a paper on the fossil plants of the Kilmarnock, Galston, and Kilwinning coal-field in Ayrshire. All the species which are described in the paper belong, with one exception, to the Lower Coal-measures.—Prof. Tait communicated the second and third parts of a paper, by Prof. C. G. Knott, on some relations between magnetism and twist in iron, nickel, and cobalt. Part II. contains a continuation of former experiments on the twists produced in the magnetic metals when they are under the combined influence of circular and longitudinal magnetizations. A rectangular rod of cobalt twists, like nickel, left-handedly, when a current is passed along it in the direction of magnetization. Iron twists right-handedly, unless strong fields are employed. There is no reversal of the twist in nickel when strong fields are used, but a maximum can be reached. The magnitude of the twist which is produced by a reversal of one force depends upon which force is reversed. In general, reversal of the longitudinal field produces the greater effect; but iron and nickel, in low fields, twist most when the current is reversed. Hysteresis is very evident in all the phenomena. Evidence is given in this part in confirmation of the truth of an expression, which was given in Part I., for the twist in terms of the elongations in a thin-walled tube of given radius. Part III. contains a discussion of the magnetic consequences of twisting a magnetized wire—more especially a circularly-magnetized wire. The peculiar manner in which the magnetic change sometimes lags behind the stress, sometimes shoots ahead of it, is fully investigated. This effect is found to depend upon the strength of the current, on the amount of the twist, and on the amount of vibration to which the wire is subjected. The longitudinal polarity which is acquired when a wire carrying a current is twisted appears to be high in comparison with the intensity induced at the circumference of the wire. This seems to indicate the existence of molecular groupings which alter their configuration when subjected to change of stress or of magnetic force. The effects which are observed when an apparently demagnetized wire is subjected to twist suggest that a magnetized wire may in certain circumstances consist of alternate layers of opposite polarities. Any stress which acts differently on these layers will produce powerful magnetic effects. From his own experiments and those of other observers, Dr. Knott concludes that the first effect of a shearing stress on the molecular groupings is not only to increase the average intensity in the direction of the magnetizing force, but also to bring into prominence a relatively high intensity in directions at right angles to it.—Dr. Buchan communicated a paper by Mr. R. T. Omond, Superintendent of the Ben Nevis Observatory, and by Mr. A. Rankin, assistant observer, on the winds of Ben Nevis. The exact determination of northerly winds is not very easy, owing to the shape of the hill. The cliff, 2000 feet in height, which forms the northern face, breaks these winds up, and makes them squally and uncertain. Some may be entered on the record as north when they should really have been entered as north-east or north-west. Southern winds are on the whole slightly more frequent than northerly winds are. At sea-level the most frequent wind is west; and south-west, west, and north-west include nearly half of the total observations—more than half if calms are excluded. These low-level winds are in exact accordance with the distribution of barometric pressure over the British Isles according to the Buys Ballot's law, which asserts that the winds blow counter-clockwise round areas of low pressure, such an area lying to the north of the British Isles. But the Ben Nevis winds do not fit in with such a distribution of pressure at all, which indicates that isobars drawn at the level of Ben Nevis (4400 feet) have directions differing entirely from the directions of sea-level isobars. In other words, the distribution of average barometric pressure which extends over the North Atlantic and North-western Europe, and dominates the surface wind over that area, does not in this country extend to a vertical height of one mile. Precautions were taken to make certain that this difference was not due to a difference between the methods of observation at Ben Nevis and at low-level stations. If a cyclonic storm of small area is lying to the north-eastward, the sea-level winds are west or north-west; but the Ben Nevis winds may be north-east, blowing straight out from the centre of the area of low pressure. In larger storms the Ben Nevis winds are practically identical with the sea-level winds, which indicates that a storm has a vertical extent proportionate in some way to the horizontal area which it covers. The outflowing wind seldom or never occurs when the centre is to the south or west, but only when it is to the north or east; and it is most strongly marked when an anticyclone lies on the other side. The outflowing current seems to carry the ascending air of the cyclone to the descending anticyclonic regions. The non-observation of the outward current when the centre of the cyclone lies on the south or west may be due to the fact that it passes at a higher level than the top of the mountain, for it then consists of air passing from hotter to colder regions, which will presumably rise to a higher level. The veering of the wind at great heights, which should occur according to the usual theory of cyclones, is very rarely observed.—Dr. Crum Brown read a paper, by Dr. A. B. Griffiths, on the blood of the Invertebrata.

PARIS.

Academy of Sciences, June 15.—M. Duchartre in the chair.—On the deformation and extinction of isolated or periodic aërial waves propagated in the interior of delivery tubes without water and of indefinite length, by M. J. Boussinesq.—On a volatile compound of iron and carbonic oxide-iron-carbonyl, and on nickel-carbonyl, by M. M. Berthelot. The author finds that iron, taken in a particular state, combines directly with carbonic oxide at ordinary temperatures (about 45° C. gives the best results) to form a very volatile compound. The required state is attained by reducing precipitated iron peroxide by hydrogen, or by decomposing ferrous oxalate by heat, and completing the reduction with hydrogen. Iron-carbonyl is analogous to nickel-carbonyl, discovered by Mond, Lang, and Quincke (Journ. Chem. Soc., vol. lvii. p. 749, 1890). M. Berthelot has investigated the stability of the latter compound and its reactions with oxygen, sulphuric acid, ammonia, and nitrogen dioxide.—*Résumé* of meteorological observations made at Ecorchebœuf, near Dieppe, from 1873 to 1882 by M. J. Reiset.—Observations of Wolf's periodic comet, made at Paris Observatory (West Tower equatorial), by M. G. Bigourdan. Two observations for position were made on June 12. It is remarked that the comet is a round nebulosity about 20′ in diameter, and having a magnitude 13·3.—Observations of the new asteroid (310) made at Paris Observatory with the East Tower equatorial, by Mdlle. D. Klumpke. An observation for position was made on June 12.—Eclipse of the sun of June 6; observations made at Lyons Observatory, by MM. Gonnessiat and Le Cadet. Measures were made of times of contact.—Observations of Wolf's periodic comet (1884, III.), made at Algiers Observatory with the Foucault telescope of 0·50 metres aperture, by MM. Rambaud and Sy. Eight observations for position were made between May 15 and June 8.—Eclipse of the sun of June 6, observed at the Observatory of the Flammarion Scientific Society at Marseilles, by M. Jacques Léotard.—On the two forms in which the co-ordinates of the surface of the fourth degree, described by the summits of cones of the second order which pass through six given points, are expressed by means of θ functions of two arguments, by M. F. Caspary.—On an electric indicator for the detection of small variations of pressure in currents of gas, by MM. G. and L. Richard.—Researches on the application of the measure of rotatory power to the determination of compounds formed by aqueous solutions of mannite, with acid molybdates of soda and ammonium, by M. D. Gernez. By measuring the proportions of salts in solution which give the maximum rotatory effect on polarized light, the author arrives at the molecular formula of the compounds formed.—On quinethyline, a homologous base of quinine, by MM. E. Grimaux and A. Arnaud.—On ureides derived from normal acids, by M. C. Matignon.—Mode of formation of methyl-campho-carbonates of methyl and ethyl, by M. J. Minguin.—On nitro-cyanacetic ethers, by M. P. Th. Muller.—Bleaching of cotton by oxygenated water, by M. Prud'homme. The addition of calcined magnesia to oxygenated water improves the bleaching properties of the latter. According to the author, the superiority of the results obtained is due to the formation of a peroxide of magnesium.—*Rôle* of the nucleus in the formation of the fundamental muscular reticulum of the larva of Phrygane, by M. G. Bataillon.—On a special disposition of the eyes in *Pulmonaræ basommatophoræ*, M. Victor Willem.—Experimental contribution to the study of growth, by M. Henry de Varigny.—On a cryptogamic disease of the *Acridium peregrinum*, by M. L. Trabut.—On the existence of a little Miocene vertebrate fauna in the rocks of the Saône valley at Gray, and at Mont d'Or Lyonnais, by M. Charles Depéret.—Contribution to the geological study of the environs of Digne, by M. Bachelard.—Fauna in a deposit of Quaternary strata at the environs of Pouillenay, by Don Jehl.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Oyster: W. K. Brooks (Wesley).—De l'Exercise chez les Adultes: Dr. F. Legrange (Paris, Alcan).—Bulletin of the United States Fish Commission, vol. viii. (Washington).—Education and Heredity: J. M. Guyau; translated by W. J. Greenstreet (Scott).—An Introduction to the Mathematical Theory of Electricity and Magnetism: W. T. A. Emtage (Clarendon Press).—Le Pêche et les Poissons des Eaux Douces: A. Locard (Paris, Baillière).—La Plume des Oiseaux: Lacroix-Danliard (Paris, Baillière).—Les Plantes d'Appartement et les Plantes de Fenêtres: D. Bois (Paris, Baillière).—Dictionaire d'Électrité et de Magnétisme: J. Lefévre (Paris, Baillière).—Bibliography of the Chemical Influence of Light: Dr. A. Tuckerman (Washington).—Constance Naden and Hylo-Idealism: E. B. Brewer (Bickers).—A Summary of the Darwinian Theory of the Origin of Species: F. P. Pascoe (Taylor and Francis).—L'Anthropologie, 1891, tome ii. No. 3 (Paris, G. Masson).—Journal of the Royal Microscopical Society, June (Williams and Norgate).

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THURSDAY, JULY 2, 1891.

CRYSTALLOGRAPHY.

Elements of Crystallography for Students of Chemistry, Physics, and Mineralogy. By George Huntingdon Williams, Ph.D., Associate Professor in the Johns Hopkins University. Second Edition, Revised, pp. 246, with 383 Woodcuts and 2 Plates. (London: Macmillan and Co., 1890.)

THE position which crystallography ought to occupy in a scheme of scientific education is far from being generally recognized. Every day the importance of this branch of science, not only to the mineralogist and geologist, but also to the physicist and chemist, is becoming more deeply felt; and yet, as a general rule, the systematic study of crystallography is left quite unprovided for in our schools and Universities.

If we take any standard treatise on physics, we shall find that the subject of the measurement and calculation of crystal forms is almost, if not entirely ignored; and though it is, of course, absolutely impossible to discuss optical and other physical phenomena without reference to the wonderfully suggestive relations which exist between the properties resulting from internal molecular structures, and the crystalline forms which are the "outward and visible sign" of such molecular structure, yet the references are usually vague and, not unfrequently, misleading. In confirmation of this statement, it may be mentioned that in a very widely-used treatise on physics—one that has passed through many editions in this and other countries—there is a hopeless confusion between the terms "hemihedrism" and "hemimorphism" in the account which is given of the remarkable phenomena of pyro-electricity.

Nor, as a rule, have chemists dealt more adequately with the subject of crystallography than their brethren the physicists. In many chemical treatises we find such terms as pyramidal, prismatic, octahedral, rhomboidal, &c., employed so loosely as not to give the student the faintest idea of the real symmetry of the forms which are referred to. This neglect of crystallography by chemists is seen to be the more serious when we remember two important circumstances—first, that crystallization is often the only means which chemists possess of isolating and readily distinguishing many bodies; and secondly, that new substances are being continually formed by the chemist, the study of some of which may throw new and important light upon crystallographic principles.

Mr. Fletcher, in a very suggestive address to the Mineralogical Society, has justly remarked:—

> "Hitherto, at least, the chemists of this country have been too content, either to leave the crystalline forms of their artificial products undetermined, or to impose the task of their determination on the already sufficiently occupied mineralogist. It seems obvious that in a satisfactory system of education every chemist should be taught how to measure and describe the crystalline characters of the products which it is his fate to call into existence. . . . A knowledge of the elements of crystallography, including the mechanics of crystal-measurement, ought to be made a *sine quâ non* for a degree in chemistry at every University."

The consequence of this neglect of crystallography by physicists and chemists has been that the teaching of crystallography has fallen almost entirely into the hands of mineralogists and geologists. But there is no more reason why every book on mineralogy should commence with a crystallographic treatise, than that it should include dissertations on refraction or articles on chemical analysis. "Crystallography should be taught as a special subject," and the student who, after his training in physics and chemistry, takes up the subject of mineralogy, ought to know at least as much of the measurement and symmetry of crystal forms, as he does of the effects of various media on different kinds of radiant energy, or the reactions of the several bases and acids.

It would be easy to show that, much as mineralogists have done for the study of crystallography, the latter science would have been developed more logically, and perhaps more rapidly, if the illustrations of the phenomena of crystallization had not been so exclusively sought among natural products. We find not a few examples in the terminology of the science of the effects of this one-sided growth of crystallography.

Crystallography is based upon purely mathematical considerations, and the study of the principles of crystal-measurement, the discussion of crystal-symmetry, and the calculation of fundamental forms, ought clearly to be one of the first branches of applied mathematics to be taken up by the student of physics; thus the study of crystallography should certainly precede that of physical optics. If this course were followed, the student of chemistry and mineralogy would come to the teachers of those sciences with such an amount of preliminary information as would enable him to profit by their instructions.

In the work now before us, Dr. Williams fully recognizes the importance of the principles for which we have been contending, and has endeavoured to supply English-speaking students with a short and clear treatise on the principles of crystallographic science. It is certainly remarkable that the countrymen of Wollaston, Whewell, and Miller should have had to wait so long for a work of this character; though every student of the subject must gratefully remember the aid afforded by the admirable little primer prepared some years ago by Mr. Gurney, and published by the Society for Promoting Christian Knowledge.

Of Dr. Williams's qualifications for undertaking a work of this kind it is unnecessary to speak. His numerous original researches afford abundant evidence of his devotion to crystallographic study, and in the preparation of the work he has had the advice and assistance of one of the first crystallographers of the United States, Prof. S. L. Penfield, of New Haven.

In order to keep the work within the smallest possible limits, it has been restricted to geometrical crystallography, but otherwise the work has been modelled upon the same lines as Groth's standard work, "Physikalische Krystallographie." The plates and very numerous woodcuts afford the greatest possible aid to the reader, and the typography leaves nothing to be desired. In looking through this revised edition, we are struck with the almost entire absence of those typographical errors that so easily creep into a work of this kind, and which, though so obvious to an expert, often prove to be a source of infinite trouble to the beginner.

In dealing with the vexed question of crystallographic notation, we think Dr. Williams has exercised a very wise discretion. The simple and easily understood symbols of Naumann have been employed in the first instance, but in almost every case the corresponding symbol of Miller's system has been added in brackets. While all students of physics, chemistry, mineralogy, and geology ought to equip themselves with such an amount of crystallographic knowledge as may be derived from the study of this book, only a very small proportion of them are likely to be called upon to deal with the higher and more complicated problems of the science. The small minority of students who devote themselves to purely crystallographic researches may be fairly recommended to employ from the first the beautiful method of notation devised by Whewell and perfected by Miller; but it is more than doubtful if the student with a smaller amount of mathematical training would gain any real benefit from such a course. In an appendix, "on zones, projection, and the construction of crystal figures," the author of this work has indicated to such a beginner the nature of some of the methods of investigation which are pursued by more advanced students.

In any future edition of the work—and such, we feel sure, will certainly be called for—we think that the author would do wisely to add a table showing the symbols of the chief forms according to all the different systems of notation commonly employed. The student who turns to the classical memoirs of Des Cloizeaux, Mallard, Bertrand, and others of the French school of crystallography, would thus be enabled to avail himself of much valuable literature, which, owing to the employment of an unfamiliar notation, must otherwise remain a sealed book to him.

We have spoken regretfully at the outset of this notice of the general neglect of crystallographical studies; but we are compelled to admit that, for this neglect, crystallographers themselves are largely to blame. The confusion produced by numerous rival systems of notation is answerable for much of that feeling of despair among those who attempt to make themselves acquainted with the subject. If the time has not yet arrived when a uniform crystallographic language can be agreed upon, much might be accomplished if the plan adopted by the author of this work of giving in every case the symbols according to *two* systems were followed. This is already done in the *Zeitschrift für Krystallographie*, the *Neues Jahrbuch für Mineralogie*, &c., the Journals of the English and French Mineralogical Societies, and several other well-known periodicals. If a conference of the leading crystallographers of Germany, France, and England could be held to decide upon the order in which the axes should be taken in writing symbols and other similar arrangements which are purely conventional and arbitrary, we might hope to see much of the confusion removed that has so long been a bar to the progress of this most fascinating and important branch of science.

We feel assured that the simultaneous publication in this country and in America of so simple and at the same time so accurate a text-book of the subject as the work we are now considering will do much towards reviving and diffusing a taste for the study of crystallography The student who masters the contents of this little book will undoubtedly have much more to learn before he is competent to deal with all the higher problems of crystallographic science; but, however far his researches may be carried in the future—and this is, perhaps, the very highest praise we can give to the book—he will certainly have little, if anything, to *un*learn.

JOHN W. JUDD.

PHOTOGRAPHY IN COLOURS.

Photographie des Couleurs par la Méthode Interférentielle de M. Lippmann. By Alphonse Berget. (Paris: Gauthier-Villars et Fils, 1891.)

THIS interesting little *brochure* contains an account of the recent achievements in colour photography which have been made so widely known to the English public through the daily papers. Coming from the pen of an "attaché au Laboratoire des Recherches (Physique) de la Sorbonne," we may take this contribution as an authorized exposition of M. Lippmann's work, and as such it will be found useful by physicists, chemists, and photographers, as well as by the general reader who wishes to know the real state of the case concerning this important departure in photographic methods. In a short historical introduction the author calls attention to the previous photochromatic attempts by Seebeck in 1810, by Herschel in 1841, by Edmond Becquerel in 1848, by Niepce de St. Victor in 1851 to 1866, and by Poitevin in 1865. It is stated that these and all similar attempts were based upon purely chemical methods, the investigators seeking for some sensitive compound which would give chromatic impressions corresponding to the colours impinging on the film. M. Berget adds the important remark: "*a priori*, ce problème est irréalisable."

Chapters ii. to v. are devoted to elementary optical principles. Chapter ii. deals with vibratory movements and their propagation, wave-length and period, and sonorous waves. In the third chapter the phenomenon of interference is described and explained; in the fourth chapter we have sections on the luminiferous ether, the velocity of light, the decomposition of white light by a prism, and Fresnel's theory of the spectrum colours. The subject of complex colours, as distinguished from the pure colours of the spectrum, is also dealt with in this chapter, and is of special importance in connection with the colours of natural objects, to which the author devotes a short section. It is pointed out that the principle of superposition of vibrations holds good in optics as in acoustics, and that just in the same way that the diaphragm of a phonograph can take up and faithfully transmit the extremely complex system of superimposed aërial vibrations produced by the human voice, so the ether transmits the complex superimposed vibrations emanating from coloured objects. In connection with the history of the undulatory theory, the whole credit is given to Fresnel: "L'honneur de donner la première théorie rationelle de la lumière, en la considérant comme résultat d'un mouvement ondulatoire, était réservé à un savant français: Fresnel." We should like to have seen Thomas Young receive at least an honourable mention.

The subject of interference receives more detailed treatment in chapter v., the interference of direct and reflected waves, and the theory of Newton's rings, being specially dealt with. It is not till we come to the sixth chapter that we are introduced to the main subject of the *brochure*. The principle which guided M. Lippmann in his experiments is well and tersely given. Imagine a plane metallic mirror with its reflecting surface coated with a transparent, homogeneous film of a silver haloid in albumin or collodion. Supposing a coloured ray of definite wave-length to fall on such a film, the undulations would traverse the transparent sensitive film, and being reflected from the polished surface of the mirror, and meeting the incident waves, would produce interference. The space in front of the mirror would thus be occupied by parallel planes alternately light and dark, and separated by half wave-lengths, *i.e.* by spaces of 1/4,000,000 of a millimetre. There is therefore ample space, even within the thickness of the film, for several of these planes of interference. On development, the planes corresponding to the light intervals would alone give films of metallic silver, while the dark intervals would remain unaffected. On fixing, there would thus be left in the film a series of parallel films of metallic silver separated by half wave-lengths. Any pair of such films constitute a thin plate in the Newtonian sense, and will give by interference a colour corresponding to that which produced the original deposition of the films when viewed by reflected light.

To realize the foregoing principle experimentally, M. Lippmann has found it necessary to use dry films of collodion, or albumin, or gelatine sensitized by immersion, as in the old wet collodion process: emulsions are granular and opaque, and contain particles which are gross in comparison with the half wave-length of a spectrum colour, and cannot be used. Moreover, it has not been found practicable to coat the reflecting surface of the mirror directly with the sensitive film, because the free iodine tarnishes the silver and destroys its reflecting power. This difficulty has been surmounted by making the coated glass plate one side of a shallow trough with parallel sides filled with mercury, the coated side being inwards, and in close contact with the mercury. The conditions for reflection and interference are thus fulfilled. The image of the spectrum is focussed on a glass plate with a ground surface, which is temporarily fixed to the side of the cell or trough in the same position as that occupied by the sensitive plate, *i.e.* with the ground surface inwards. After focussing, the ground glass is removed, and the sensitive plate substituted for it in the position described.

The spectrum was produced by an electric arc light of 800 candle-power, and the time of exposure for the different parts of the spectrum was regulated by interposing cells with coloured solutions, beginning with a solution of helianthin which transmits only the red and yellow, then replacing this by a cell of potassium dichromate which transmits the red, yellow, and green, and then finally exposing for a few seconds without any screen, so as to impress the blue and violet. The whole time of exposure varies, according to the sensitiveness of the film, from half an hour to two hours. The details of development and fixing are given by M. Berget, and do not differ fundamentally from the ordinary methods. The finished image, *when dry*, shows the spectrum colours by reflected light with metallic brilliancy, and as the colours are purely optical, depending only on reflection and interference, they are permanent. As the author points out, it is certainly a marvellous tribute to the fidelity of the photographic method that a series of laminæ of metallic silver separated by intervals of only about 1/4,000,000 of a millimetre should retain their positions with optical accuracy during the processes of fixing and development.

There can be no doubt—as will be admitted by all who have seen the results—that M. Lippmann is to be congratulated on having made a most important advance in the methods of photochromy. How far his experiments go towards the realization of the great problem of photographing objects in their natural colours is a question quite distinct from his present achievement. M. Berget tells us that satisfactory reproductions of coloured glasses illuminated from behind by the electric light have been obtained, but this is only a very little step in the desired direction.

"Que reste-t-il à faire pour rendre *absolument usuel* le procédé photochromique de M. Lippmann?" There remains a great deal! Not the least of the requirements is a transparent sensitive film equally sensitive to every colour of the spectrum, and sufficiently sensitive as a whole to enable the impression to be secured with a moderate exposure, instead of 30 to 120 minutes. Till this is accomplished we are not much nearer the solution of the problem of photography in natural colours than we were before. M. Berget speaks hopefully of the prospects in this direction, and we wish every success to his anticipations. But it is no detraction from the merit of M. Lippmann's results if these have no immediate bearing on practical photographic processes. As a triumph of physical science these experiments will live.

"C'est aussi un triomphe pour la science française, car ce mode de reproduction des couleurs du spectre à l'aide des lames minces limitées par des plans d'argent constitue une matérialisation, réalisée par un savant français, de ces ondes lumineuses conçue pour le première fois par le puissant génie d'un autre Français illustre: j'ai nommé Augustin Fresnel."

With this patriotic outburst M. Berget concludes his pamphlet, and the compatriots of Niepce and Daguerre may well be gratified with this latest emanation from the physical laboratory of the Sorbonne.

R. Meldola.

OUR BOOK SHELF.

Geometry of Position. By R. H. Graham, Author of "Graphic and Analytic Statics." (London and New York: Macmillan and Co., 1891.)

This work essays to fill an existing want by providing an English text-book on the important subject of geometry of position in relation to graphical statics.

The author gives an introductory chapter on anharmonic pencils and ratios, followed by an interesting chapter on projective conics, and devotes the remainder of the book to the application of graphic methods to statical problems, including, amongst others, the discussion of Maxwell's theory of reciprocal figures.

The chapter on anharmonic pencils and ratios would have been considerably improved by the introduction, at the beginning, of more definitions and explanations of the nomenclature adopted. The proofs of Desargue's theorem and its converse, given on p. 3, are unduly compressed, considering the early stage at which they are introduced; and the student's preliminary difficulties will be increased by the fact that the enunciations have been given in succession, while there is nothing to indicate which is to be treated first.

In the chapter on reciprocal figures, we would suggest that the proof given of Theorem I., Art. 50, might with advantage have been dispensed with. In Art. 52 it is erroneously assumed that OB′ is equal to force (1); this assumption mars a proof which would be otherwise good.

The work exhibits evidence of originality, and it is, perhaps, to be regretted that the proof-sheets have apparently been revised only by the author himself. Their revision by one who had no part in compiling them would probably have contributed to a better arrangement, and to the exclusion of much that is vague.

The carefully drawn diagrams of different problems contained in the book form admirable illustrations to the non-technical reader of the nature of the operations involved in the application of the graphical calculus, and of the character of the results obtained by it. They are the more welcome as such information is not readily available in English text-books, while in foreign treatises it is often developed in such minute detail as to make the foundations nearly inaccessible to the general reader.

A word of praise is due to the interesting collections of examples at the ends of the chapters, which are, it seems, mostly original, but partly drawn from sources not often laid under contribution in the ordinary text-books.

ALEX. LARMOR.

The Species of Epilobium occurring North of Mexico. By Dr. Trelease, Director of the Missouri Botanic Garden. From the Second Annual Report of the Garden, issued April 1891. 48 pages, 48 plates.

EPILOBIUM is not a very large genus, but is spread universally through the north temperate zone, both amongst the plains and mountains, and reappears in plenty in New Zealand. The species are very difficult of delimitation and definition, and great diversity of opinion has prevailed as to their number, and the validity of the characters which have been used to characterize species. It is evident, moreover, that many of them hybridize freely in nature. Passing over the earlier well-known writers, such as Pursh, Muhlenberg, Hooker, and Gray, in 1876 Barbey contributed a monograph of the Californian species to Brewer, Watson, and Gray's "Flora of California," and later published excellent figures of the new species which he there described. In 1884, Haussknecht published a monograph of the whole genus. Of the 38 species dealt with in Dr. Trelease's paper, 13 have been proposed by Haussknecht, 3 by Barbey, 4 by himself, and one by Parish, so that more than half the 38 have been lately described for the first time. Dr. Trelease describes fully all the species known in Temperate North America, gives an octavo plate of each of them, and a detailed account of their geographical distribution, citing the numbers of all the recent collectors. Of the 38 species only 9 extend their range beyond the American continent. The paper will be a very acceptable contribution to our knowledge of a difficult genus, and will no doubt be incorporated in the new "Flora of North America," of which the second volume is already published, and the first and third of which we anxiously wait for.

J. G. B.

A Guide Book to Books. Edited by E. B. Sargant and Bernhard Wishaw. (London: Henry Frowde, 1891.)

THERE are so many books of all kinds that ordinary readers may be excused if they are sometimes at a loss as to the works which they ought to select for study. The editors of the present volume have come to the aid of such readers, and may be congratulated on the manner in which they have accomplished a useful but most troublesome task. They make no attempt, in a philosophical sense, to classify the various subjects with which authors have dealt; they simply take these subjects one after the other, in alphabetical order, and set down what seem to them the best books relating to each. Taking into account the amount of space at their disposal, they probably could not have chosen a plan that would have been more readily intelligible. Of course opinions will differ about the value of the works included in the several lists. Everyone who consults the volume will be of opinion that the editors have omitted some things which they ought to have noted, and that they have noted some things which they ought to have omitted. But there cannot but be a general agreement that, upon the whole, the selection has been made on sound principles, and that it is likely to be of real service to very many of those who may have occasion to refer to it. A large number of eminent writers have helped the editors, not only by drawing up lists of books, but by giving them much valuable advice.

Tasmanian Official Record, 1891. By R. M. Johnston, F.L.S. By Authority. Second Year of Issue. (Tasmania: William T. Strutt, Government Printer, Hobart, 1891.)

ANYONE who may wish to obtain information about Tasmania will be hard to please if he does not find what he wants in this elaborate volume. It begins with an account of the general physical outline of the island, and then we come to Tasmanian history, and to the Tasmanian constitution and government. After a chapter on Crown lands we are invited to consider the geology and mineral products of Tasmania, its flora and vegetable products, fauna and animal products, population, vital statistics, trade and interchange, accumulation, finance, production, law, crime, and protection, and "intellectual and social provision." The work is wound up with a view of the progress of Australasia, and a summary of general statistics. In the present issue some important additions have been made to the book as originally published, and by devoting attention to classification the editor has tried to "obviate any difficulties that might arise from the necessity of bringing together in one volume such a variety of subjects."

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Albert University.

PROF. LANKESTER, in the interesting letter published in NATURE for May 28 last (p. 76), expresses his desire to have "a genuine professorial University set on foot in London, not because it is London, but because University and King's Colleges are there, and respectfully petition Her Majesty to do for them what the monarch has done in past days for other Universities."

I have not seen the petition of the Colleges. But I have before me the draft charter adopted by their Councils, which I presume is intended to give effect to the prayer of the petition. I can hardly imagine that Prof. Lankester was acquainted with its contents when he penned the sentence which I have quoted.

If the Albert University is called into existence—and it seems very probable that its charter will be granted—it will be an institution very similar to what the University of London was in the early years of its existence, when it drew its candidates only from the so-called affiliated Colleges.

The charter commences by reciting "that it is expedient there should be constituted in and for the London district (defined as 'a radius of fifteen miles from Somerset House') a University . . . commending to its students systematic courses of teaching and methods of study." But "commending" is what we all do now.

The new University is to be of the federal type. Beginning with University and King's, "other Colleges may from time to time be admitted." This was inevitable, though my pointing out the fact made my friend Prof. Lankester somewhat angry.

Any medical school may be admitted which is recognized as efficient by any qualifying body under the Medical Acts. But while Colleges will have representatives on the Council, the medical schools will only have representatives on the Faculties.

Degrees may be granted apparently in any subject the Council please, subject to a regular course of study and examination. This will apparently admit theology, which is probably a desirable thing, provided it be unsectarian.

The powers to grant degrees are rather large, and deserve careful consideration. The London radius at once, as has been the case with the existing University, goes off into Imperial infinity in the provision that anyone who has been a resident student in any University in the Empire may count his time and examinations, except that a "final portion of the period of study" and the "final examination" shall be passed in the University.

There is an unlimited power to grant *ad eundem* degrees as well as honorary degrees at the discretion of the Council. Fellows of University and King's Colleges (a purely honorary distinction in itself) are indicated as fitting recipients, and also "past students of the said Colleges," a rather large door to open if in the future a degree is to have any meaning at all.

Power is taken to examine into the efficiency of schools or any academic institutions—work already in the hands of other Universities—and apparently the London radius again becomes infinite.

Independent University lecturers may be appointed.

The Council will consist of members appointed for five years by (1) the Crown (Lord President); (2) Convocation; (3) Colleges; (4) Colleges of Physicians and Surgeons; (5) Faculties. The Faculties are to be constituted (1) of teachers in the Colleges; (2) of examiners; (3) of persons who are or have been engaged in University teaching in London. The Boards of Studies are delegations from the Faculties, as they should be. All this is much on the lines sketched out in my own letter in NATURE.

A rather remarkable feature in the scheme is the creation of a Convocation of graduates. Whatever may be the function of this body in other Universities, it is somewhat surprising to meet with its existence in what professes to be a teaching University.

The examinations are to be conducted by examiners who are members of the respective faculties associated with external examiners; the teacher-examiner seems not to be insisted upon.

These are the essential elements of the proposed constitution. If it is asked what distinctive character the Albert University will possess which will mark it off from the existing University, or from that body as it might be conceivably reconstituted, I must confess that it seems to me to lie in a very small compass. Notwithstanding the use of the ambiguous word "commending," when one would have expected "prescribing," I take it for granted that the essential feature in the whole scheme is the enforcement upon candidates for degrees of attendance upon a curriculum. But in the existing University, this is already required in the Faculty of Medicine. Prof. Huxley has further urged it in the Faculty of Science; and for my part I believe that the time has arrived when it might be demanded without difficulty. The prominence given to practical work in the science examinations has made it all but impossible for a candidate to acquit himself successfully who has not attended a competent course of instruction. To insist upon a curriculum would be now scarcely more than the practical recognition of this fact. The only real point of divergence is in the Faculty of Arts; about this I speak with some hesitation. It may be that the enforcement of a curriculum is desirable; I am not satisfied that in this faculty it is so, or at any rate absolutely essential, as I think it is in the Faculty of Science. With this exception I can see no net public gain in the new scheme to justify the creation of the cumbrous machinery of a new federal University.

Seeing that the existing University is a State institution in actual possession of the field, I think the public at large might have reasonably expected from the Senate some statesmanlike criticism, rising above the petty level of supposed self-interest in the very serious action which the Government is apparently about to take.

They content themselves, however, with a sort of half-sulky acquiescence in the scheme "so far as it proposes to confer on the petitioning Colleges the power of granting degrees in arts and science to students of the Colleges who have pursued their entire academic curriculum within the Colleges." The Senate, a little maliciously, proceeds to point out that "the petition of the Colleges lays great stress upon the paramount importance of close association of students and teacher-examiners, and of placing the power of granting degrees in the hands of those teachers who have instructed the candidates." It not unnaturally insists upon the inconsistency with this position of the proposal "to accept residence and examinations at other Universities," if only a final period of study, "which might be a short attendance at evening classes," be passed at the new University.

It also objects to the honorary and *ad eundem* degrees. But its criticism is even more destructive in regard to the Medical Schools. It is quite obvious that if the Medical Schools joined the Albert University, the teacher-examiner system would disappear, and the new and the old Universities would be simply competing agencies for doing the same kind of work in the same kind of way. The same argument applies more or less to the other faculties as soon as the number of constituent Colleges becomes numerous.

Yet so great is the magic of a phrase that the daily papers in reporting the proceedings in the Privy Council describe the scheme as that of a Teaching University. A University of the Scotch or German type may have some claim to that title; but no federal University can ever possess a valid one, for the simple reason that there will always be a morphological distinction between the Colleges which teach and the University which examines and grants degrees.

Prof. Lankester contended in his letter that the question whether University and King's Colleges should have a University Charter was a sort of private affair between them and the Government. But I do not think this view can be accepted. Whether we like degrees or whether we do not, they have a certain value in the eyes of the public. Personally, I have no objection to the multiplication of Universities, if each has a proper geographical area assigned to it. But the multiplication of Universities in the same place seems to me a great evil. It cannot be assented to without the necessity being shown to be overwhelming. And in the present case it appears to me that it cannot be so shown. If the existing University is so injurious to the best interests of the higher education that another is imperatively demanded to do the work in which it fails, then it appears to me that two obvious points present themselves:—

(1) The new University should be free from the defects that attach to the old one. Prof. Lankester speaks of the "thraldom" of "the Imperial centralizing institution"; but when the matter comes to be looked into, the new institution also proposes to be Imperial and centralizing, and will be found to exercise the same or even greater thraldom on the individual teacher.

(2) If the old University is really doing mischief, it is the paramount duty of the supreme Government, whose creature it is, to reform it. The fact that the Senate and Convocation are at loggerheads how this is to be effected is really beside the question. When public opinion demanded the reform of the older Universities, new ones were not created alongside the unreformed old ones; but a Commission with executive powers effected the changes which were necessary. And for a similar procedure there is still time at Burlington Gardens.

W. T. THISELTON-DYER.

Royal Gardens, Kew, June 30.

The Holarctic Region.

REVIEWING the recently-published "Introduction to the Study of Mammals" by Prof. Flower and Mr. Lydekker, Prof. Lankester states (*suprà*, p. 122) that "The authors of the present work mention Dr. Heilprin's opinion that the Palæarctic and Nearctic regions should be united and called the Holarctic region. But they do not adopt this opinion, nor refer to Huxley's proposal to term this same area Arctogæa," and so on. Now, in this last statement my good friend the reviewer, perhaps writing from memory, is mistaken. Had Prof. Huxley proposed to limit his "Arctogæa" to the Palæarctic and

Nearctic regions of Mr. Sclater and Mr. Wallace, I should certainly not have suggested to Prof. Heilprin a new name for that combination. Anyone looking to the passage (Proc. Zool. Soc., 1868, pp. 314, 315) in which Prof. Huxley defined his "Arctogæa"—a name to which, let me say, I have not the least objection—will see that it signifies that part of the world which is not "Notogæa," and therefore includes the Ethiopian and Indian regions of Mr. Sclater, whereas my "Holarctic" region expressly excludes them, and is therefore a very different thing from "Arctogæa" in its true sense.

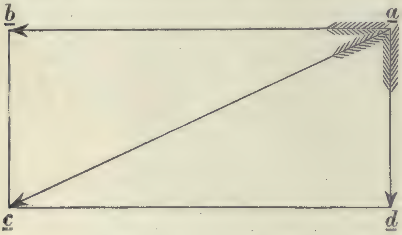
ALFRED NEWTON.

Magdalene College, Cambridge, June 12.

Force and Determinism.

IN your issue of March 12 (vol. xliii. p. 491), Dr. Oliver J. Lodge characterizes as "perfectly correct" the statement "that, although expenditure of energy is needed to increase the speed of matter, none is needed to alter its direction." I have looked in vain for some notice of this apparently strange doctrine in your subsequent issues, with the exception that Prof. C. Lloyd Morgan (April 16, p. 558) objects that the direction of motion cannot be changed by purely metaphysical means, or will-power. But passing over this rather important and interesting point with only the observation that Sir John Herschel thought differently—thought, in fact, that "without the power to make *some* material disposition, to originate *some* movement, or to change, at least temporarily, the amount of dynamical force appropriate to one or more material molecules, the mechanical results of human or animal volition are inconceivable" (*Fortnightly Review*, July 1, 1865, vol. i. p. 439)—I desire to call a moment's attention to the first statement alluded to.

Dr. Lodge admits that "expenditure of energy is needed to ncrease the speed of matter." But, as a matter of fact, is it not very difficult, if not indeed practically impossible, to change the direction of a moving body without affecting its speed? "A force at right angles to motion does no work," says Dr. Lodge. Let us examine this statement for a moment. Let a body be moving in the direction *a* to *b* with a speed sufficient to traverse the distance in one unit of time. Then let a force be applied to the body at *a*, at right angles to the direction of its motion, sufficient, if acting alone, to carry the body to *d* in the same unit of time. By the composition of forces, the body, at the end of the unit of time, would, therefore, be found at *c*. But the distance *ac* is greater than *ab*; and as, by the interposition of a force at right angles to its motion, the body has thus traversed a greater distance in the same time, has not its speed, as a matter of fact, been increased? and is not this increase of speed actual work? and does not this work require actual energy to perform it?



EVAN MCLENNAN.

Brooklyn, Iowa, U.S.A., June 9.

I AM glad to see my statement called in question, and hoped that it would have aroused more antagonism than has yet been expressed; because I do believe that it has important psychological or metaphysical consequences, and should therefore either be repudiated by physicists or after due discussion be accepted by non-physicists.

With regard to the special objection raised by Mr. McLennan, it may be sufficient to remark that, in his diagram, *ac* is the line of motion, *ad* the direction of the force, and that *ad* is not at right angles to *ac*. His difficulty seems to be the one that some people always feel with regard to the use of infinitesimals in general. He must remember that his diagram will not apply to the case of curvilinear motion unless the impulses contemplated are momentary and infinitesimal.

OLIVER J. LODGE.

The Scorpions at the Zoo.

YOUR contributor of the notice, published in NATURE on June 18 (p. 163), on the contents of the Insect-house at the Zoo, who laments the unfortunate circumstance that the scorpions there in captivity remain unnamed, may be glad to learn that these creatures may be easily identified, and, with a little dexterity, fearlessly handled.

During a recent visit to this house, the keeper obligingly showed me the two Egyptian scorpions, one of which—the black individual with the thick tail—was easily recognizable as *Prionurus crassicauda*, Oliv., a tolerably common North African and Syrian form.

To the other, however, I could not so readily assign a name; partly owing to its partial concealment, and partly to the fact that critical inspection is required to distinguish between the species of the genus to which it belongs. It appeared, nevertheless, to be a specimen of *Buthus europæus*, Linn., the commonest of all the Mediterranean scorpions. But my attempt to verify this point by closer examination was immediately frustrated by the keeper; who, evidently thinking that I was qualifying for incarceration in Bedlam, hastily interposed when I stretched out my hand to pick up the noxious animal.

The third scorpion I did not see; but doubtless it is a specimen of one of the species of *Euscorpius*. This, too, can be easily named, no doubt; but it will be necessary to handle the specimen in order to be certain on the point.

I would warn your contributor not to be too sanguine of the permanence of the amicable relations that appear at present to be established between these three Arthropods. If the supply of dead mice runs short, there will, of a surety, soon remain nothing but a few fragments of *Euscorpius*. Such thoroughgoing cannibals are not likely to be squeamish, when a member of another genus is before them.

In conclusion, some of your readers may be interested to know that the spider referred to as *Lycosa portosantana*—which, by the way, should be styled *Tarantula maderiana*—is a very near ally of the famous and historical Tarantula of Italy; and that the hairy Brazilian monster, the so-called Mygale, who squats under a broken flower-pot in the next cage, has no more claim to the title Tarantula than any other Arachnoid with a formidable aspect.

R. I. POCOCK.

Natural History Museum, June 18.

Cetaceans in African Lakes.

WITH reference to Mr. Sclater's inquiry (NATURE, June 11, p. 124) as to the occurrence of porpoises in the Victoria Nyanza, the following extract from Bernier, who wrote about 230 years ago, will probably prove of interest.

I may add that in another passage Bernier gives further information regarding the sources of the Nile.

It would seem from the passage quoted that the occurrence of a Cetacean in the Abyssinian sources of the Nile was probably known to early travellers, and, like the occurrence of diamonds in other parts of Africa, cannot be regarded as a new discovery.

Science and Art Museum, Dublin, June 22. V. BALL.

An Armenian named Murat and a Mogul who came as ambassadors from the Christian King of Ethiopia (*i.e.* Abyssinia) to Aurungzeb shortly after his accession to the Mogul Empire, in 1659, told the French physician Bernier, who then resided at the Mogul's court, "that the Nile had its origin in the countrey of *Agaus*, that it issued out of the earth by two springs bubbling up near to one another, which did form a little lake of about 30 or 40 paces long; that, coming out of this lake, it did make a considerable river; and from space to space it received small rivers increasing it. They added that it went on circling and making as 'twere a great isle, and that afterwards it tumbled down from steep rocks into a great lake in which there were divers fruitful isles, store of crocodiles, and (*which would be remarkable enough if true*) *abundance of sea calves, that have no other vent, &c., than that by which they take in their food*, this lake being in the country of Dambea, three small days' journey from Gundar and four or five days' journey from the source of the Nile, &c., &c." ("The History of the Empire of the Mogul," English translation of 1684, p. 44).

ON SOME POINTS IN THE EARLY HISTORY OF ASTRONOMY.[1]

V.

It is imperative to be perfectly definite and clear on the question of the amplitudes above 26° at Thebes. Any amplitude within 26° means that up to that point the sun at sunrise or sunset could be observed some day or days of the year—once only in the year if the amplitude is exactly at the maximum, twice if the maximum is not reached. But in the case of these temples with greater amplitudes than 26°, it is quite clear that they can have had nothing to do with the sun. Is there, then, any additional line of evidence that the Egyptians used these temples to observe the stars? Here a very interesting question comes in; a temple built at one period to observe a star could not go on for ever serving its purpose, for the reason that the declination of the star must change by precession. Therefore a temple built with a particular amplitude to observe a particular star, useful for one period would be useless for another.

We have here possibly a means of testing whether or not any of these temples were used to observe the stars. In those very early days, 3000 or 4000 years B.C., we must assume that the people who observed the stars had not the slightest idea of these possible precessional changes; they imagined, that they were just as safe in directing a temple to a star as they were in directing a temple to the sun But with a star changing its declination in an average way, the *same* temple could not be used to observe the same star for more than 200 or 300 years; so that at the end of that time, if they still wished to observe that particular star, they must either change the axis of the old temple, or build a new one.

As a matter of fact, we find that the axes of the temples have been changed and have been freely changed; that there has been a great deal of work done on many of these temples which are not oriented to the sun, in order to give them a twist.

Once a solar temple a solar temple for thousands of years; once a star temple only *that* star temple for something like 300 years, so that the conditions were entirely changed.

We get cases in which the axis of a temple has had its direction changed, and others in which, where it has been difficult or impossible to make the change in a temple, the change of amplitude has been met by putting up a new temple altogether. We are justified in considering such temples as a series in which instead of changing the orientation of a pre-existing temple, a new temple has been built to meet the new condition of things. That, I think, is a suggestion which we are justified in making to Egyptologists on astronomical grounds.

We cannot, of course, make it with absolute certainty, for the reason that in the case of most of these temples the best Egyptologists cannot give us the most precious piece of information which we require from the astronomical point of view. That is the date of the *foundation* of the temple. If in the case of these temples it were absolutely certain that each temple was built at a certain time with a certain orientation, the use of the precessional globe would tell us at once whether or not that temple was pointed to any particular star. Some other astronomical considerations may here come to our help. If the north polar distance of a star is increasing—that is, if it is increasing its distance from the north pole—its declination is being reduced, and the orientation of the temple would be gradually becoming more and more parallel to the equator; if the declination of the star be increasing, then the orientation of the temple would have had to be more and more north or south. The change in the orientation, therefore, could give us important information, and ultimately we might be able to determine what the name of that particular star was. At present the matter must remain more or less as a suggestion; but if anything like approximate dates can be given, then astronomy really may come to the rescue of the Egyptologist and archæologist generally, and repay that debt to which I have referred, which she owes to so many other sciences.

Although, however, these matters can be discussed in a way that will indicate that the inquiry is raised, I do not wish for one moment to speak of it as being settled, because the observations which have been made already in Egypt with regard to the orientation of these temples have not been made from such a very special point of view; and further some alteration in the amplitude would be made by the presence of even a low range of hills miles away from Thebes in the case of a star rising or setting pretty nearly north or south. No one would care to make the assertion with absolute definiteness until it was known whether or not the horizon in each case was interfered with by hills or any intervening objects—was or was not one, in fact, which might be regarded as a sea horizon from the point of observation; if there were impediments, the angular height of them must, of course, be exactly known.

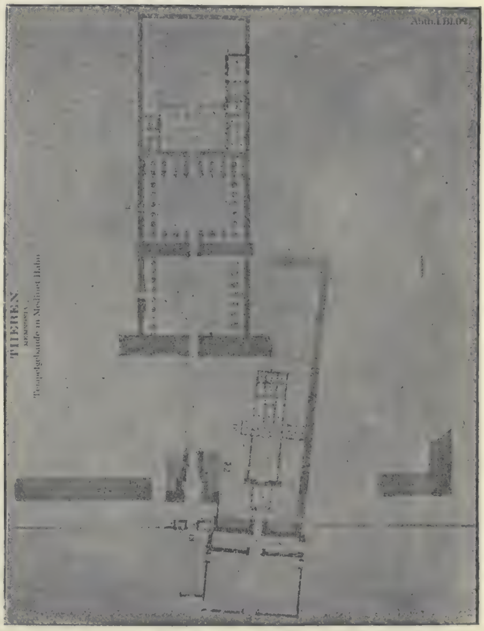


Fig. 14.—The two temples at Medinet Abou, showing the change in their orientation.

To continue this observation and this kind of thought a little further, we will go back to Karnak generally. In the first place we have the magnificent solar temple.

Next we have two parallel temples, one of them a late addition to the solar temple itself, and another one parallel to it, each of them with an amplitude of 63°, one N. of E., the other S. of W. We have then two parallel temples *at right angles* to the solar temple at Karnak. We have also a temple, with an azimuth of 68° N. of E., and one, probably older still, with an amplitude of 70° or 71° N. of E.; both these temples face northerly, and nearly in the same direction. Near the last temple we have the ruins of another one at right angles to it, and this points to the westward amplitude 19° N. of W. We may assume from the plan of the ruins that the Naos is at the east end of the temple, therefore the chief pylon would have been to the west, and therefore the axis will be in that direction. In the row of sphinxes, a double row connecting the temples of Maut and Karnak, the line is absolutely complete as far as their bases are concerned, with the exception of two where there is a gap, and that gap is exactly in the axis of this temple prolonged. Here is another instance of the rights of the line of sight of a temple being strictly preserved.

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[1] Continued from p. 110.

The Egyptians have been accused of hating every regular figure, and even in the boundary walls of the temple of Ammon there are two obtuse angles. Round the Maut temple we also have walls, and there again this hatred of similarity seems to come out, for we have one obtuse and one acute angle. But if we examine the thing a little carefully, we find that there is a good deal of method in this apparent irregularity. The wall of the temple of Ammon is parallel to the face of the temple or at right angles to its length. One wall of Maut is perfectly parallel to the face of the temple or at right angles to the sphinxes. And the reason that we do not get right angles at one end of the wall is that the walls of the temple at Maut are parallel to the chief wall of the temple of Ammon. Surely it must be that, before these walls were built, it was understood that there was a combined worship, that they stood or fell together. One thing was not attempted in one temple and another thing in another, but the worship of each was reflected in the other. And if this be true you see that there was no hatred of symmetry, but a definite reason why these walls should be built as they were.

We can depend, and no doubt depend very completely indeed upon the labours of the Egyptologists, in the case of the temples of Rameses and of Khons. No Egyptologist so far, I believe, has ventured to tell us the date of the foundation of Karnak, but what Egyptologists have stated is that those two temples were built by the same king; their architecture is exactly similar, they are parallel to each other, and they altogether bear reference to apparently the same period of Egyptian history. Now that king was Rameses III., and the year according to Brugsch was 1200 B.C. Here then we have a definite basis of work. There is a temple with an amplitude of 63° N. of E., built 1200 B.C.; there is a temple with an amplitude of 63° S. of W., built 1200 B.C. From these amplitudes we determine as before the declinations; they come out 53° N. and 53° S.

Was there an important star with a declination of 53° N., was there another with a declination of 53° S. in the year 1200 B.C.? There *were* two important stars, one with a declination of 53° N. and another of 53° S. at that time. The north star was γ Draconis, the south star was Canopus. This strengthens the view that there was really some astronomical object in the plan and direction of these temples.

Thus, at the time when these two temples were stated to have been built, each *might have been used* to observe one the rising, the other the setting, of an important star. We have long ago seen that so far the Egyptians, like the Babylonians at a later date, only had an idea of observing a heavenly body and the position of other bodies in relation to it, so long as it was rising or setting, so that it was absolutely essential that the body which they were to observe should rise and set. You know perfectly well that in London there are many stars which neither rise nor set. The latitude of London being 51°, the elevation of the pole therefore is 51°, and from the pole to the north point of the horizon being 51°: of course any star which lies at that distance from the pole cannot set, but sweeps round without touching the horizon at all. The latitude of Thebes being 25°, the distance from the pole to the horizon is much smaller, and so the number of stars which do not rise and set is much smaller. The stars which did not rise or set were stars which were moving very slowly and the stars which rose most to the north and most to the south were those bodies which were moving most slowly while they yet rose or set. Can this slow rate of motion have had anything to do with such stars being selected for observation, the brightest star to the north, most slowly moving, the brightest star to the south most slowly moving? It is possible that observations of these stars might have been made in such a way that at the beginning of the evening the particular position of γ Draconis might have been noted with regard to the pole star, if there were no other reason; and seeing that the Egyptians thoroughly knew the length of the night and of the day in the different portions of the year, they could at once the moment they got the starting point of the rising of this star practically use the circle of the stars round the north pole as the dial of a sort of celestial clock. May not this really have been the clock with which they have been credited? However long or short the day, the star which was at first above the pole star, after it had got round so that it was on a level with it, would have gone through a quarter of its revolution.

So much then for the possible use of the temples built by Rameses III. in the year 1200 B.C. It has already been pointed out that although we have in one an amplitude of 63° N. of E. we have other temples with amplitudes of 68° N. of E. and 71° N. of E. Everybody agrees that the temple, with amplitude 63° N. of E., was built 1200 years B.C. I have shown that that temple could have observed the most northerly star which did not set. May it not have been that the 68° temple and the 71° temple were temples built to observe the same star *before* this one was built, because we know they could not have observed the star *after* this one was built, since γ Draconis was decreasing its declination, therefore in previous times its declination would have been *higher*, and the amplitude therefore of a temple to observe it would have been greater.

Looking back to the German tables and other calculations, we find that with an amplitude of 68° we get a declination of 56°, and the same tables tell us that that declination was the declination of the same star γ Draconis 2000 years B.C. It does look as if in all probability we are dealing with a series of temples not twisted but built in different places.

Can we consider that the temple with an amplitude of 71° might have been used to observe that same star long before the temples were built with amplitudes of 68° and 63°? The amplitude of 71° gives us a declination of 58°, we then find the year in which that same star γ Draconis had that declination to have been about 3000 years B.C. So that it is not impossible that temple was built first of all to observe γ Draconis 3000 years B.C., that after a time the star changed its declination so much that another temple became necessary, and 1000 years afterwards the change again became large, and still another temple was built to observe it. The three temples may form one series.

The discussion is a little difficult because the orientation is very far towards the south and north, and therefore a hill a few miles off would make a difference of 2° or 3° in the orientation of the temple, and as yet we have no observations that throw light on this point.

We have then at Thebes alone three converging lines of evidence which all go to strengthen the view that these temples were really—whatever else they might have been—usable as solar and stellar observatories. The difference being of course that in the case of the solar temple no large change of amplitude was necessary, but that in the case of every stellar temple after a lapse of a certain number of years depending upon the position of the star, the temple must be twisted round if it were wished to continue to make observations of the same star.

That raises an interesting question by the way. Long after the temple had been used for observation of a particular star, long after that temple line was blocked by extended building, if the horizon of these temples was left open it looks very much as if when another bright star came along it was laid hold of for a new set of observations. However that may be, it is rendered extremely probable, by the considerations I have brought before you, that the Egyptians 3000 years B.C. had been rendered practically conversant with the result of the precession of the equinoxes by the fact that they had to rebuild and alter their temples from time to time because the stars changed their declination. If that be confirmed by subsequent investigations, it will show that these Egyptians possessed a very much more profound knowledge of astronomy than they have received credit for, because it is stated that the precession of the equinoxes was discovered by Hipparchus. It looks as if the precession of the equinoxes was probably published by Hipparchus as the result of an examination of the untold wealth of Egyptian astronomical observations which has been unfortunately lost to the world.

This question of orientation is after all one which survives among ourselves. All our churches are more or less oriented, which is a remnant of old sun worship, and the church is not always oriented exactly to the east, but so that the light of the sunrising upon the Saint's day to whom the church is dedicated may be thrown along the chancel.

It has long been known that Stonehenge is oriented to the rising of the sun at the summer solstice. Its amplitude instead of being 26° is 40°; with a latitude of 51°, the 26° azimuth of Thebes is represented by an azimuth of 40° at Stonehenge.

The first of January is very near the winter solstice, but is not quite the winter solstice. If you look up the old records of the races that lived 2000 or 3000 years B.C., you will find that the different races began their year at different times, and even that the same race at different times began their year differently; the choice lay among the equinoxes and the solstices, and seeing that one of the very oldest temples at Thebes is oriented to sunset at the summer solstice we should not be at all surprised if investigation shows that when that temple was built more than 3000 years B.C., the Egyptian year really began in what we should call our summer. We have ample evidence of this. And I think there is little doubt that when Stonehenge was built it certainly was built by people who began their year with the summer solstice, which you will remember is the time of the year in which in many countries it is the habit still to light fires upon hills and so on.

The next point is, what was probably the use made of these temples besides determining the length of the year and regulating so far as they could the seasonal changes, the times of the solstices, the times of the equinoxes, and the various celestial phenomena?

We understand that in the very beginning of observations in all countries, the moment man began to observe anything, we saw that he began to observe the stars, and the moment men began to talk about anything they had seen they must have started by in some way or other defining the particular stars they meant.

They would obviously talk first of the brightest stars, and separate them from the dimmest ones; they would then discuss the stars which never set, and separate them from those which did rise and set; then they would take the most striking configurations, whether large or small; they would choose out the constellation of Orion or the Great Bear, and for small groups the Pleiades. These would attract attention, and be named before anything else. Then later on it would be imperative in order to connect their solar with their stellar observations that they should name the stars which lay along the sun's path in the heavens. They would confine their attention to a belt round the equator rather than consider the configuration of stars half-way between the equator and north pole. In all countries—India, China, Babylonia, Chaldæa, Egypt—they had a sort of girdle round the heavens, called by different names in different countries, and the use of this girdle of stars, which sometimes consisted of twenty-eight stations, sometimes of twenty-seven, and sometimes of only ten, was to enable them to define the place of the moon or of any of the planets in relation to any of these stars. That condition of things, that stage of thought, is brought well before us in the Jewish Scriptures.

In the Book of Job we read, "Canst thou bind the sweet influences of Pleiades, or loose the bands of Orion? Canst thou bring forth Mazzaroth in his season? or canst thou guide Arcturus with his sons?"

Here we have the difficulty which has met everybody in going back into these old records, because there was no absolute necessity for a common language at the time; it was open to everyone to call the stars any name they chose in any country, therefore it is difficult for scholars to find out what particular stars or constellations were meant by any particular words. In the revised version, Arcturus has given place to the Bear with its train, and even our most distinguished scholars do not know what Mazzaroth means. I wrote to Prof. Robertson Smith the other day to ask him to give us the benefit of his great knowledge, and he says that Mazzaroth is probably that band of stars round the ecliptic or round the equator to which I have referred, but he will only commit himself to the statement that it is a probable enough conjecture; other people believe that it was a reference to the Milky Way.

I mention this to show you how very difficult this inquiry really is. The "seven stars" undoubtedly mean the Pleiades and not the Great Bear. Among the brighter stars, Arcturus, the Pleiades, &c., are referred to by Homer and still earlier writers. So far as Egyptian and Chinese astronomy goes, practically the first reference to a constellation appears in Egypt with reference to the equinox which happened 3285 years B.C., and in China with reference to the Pleiades in the equinox of 2357 B.C.

In observing stars nowadays, we use a transit circle which is carried round by the earth so as to pick up the stars in different circles round the axis of the earth prolonged, and by altering the inclination of the telescope of this instrument we can first get a circle of one declination and then a circle of another.

The Egyptians did not usually employ meridian observations. Did the Egyptians make star maps? They certainly did. In the temple of Denderah, which is a comparatively modern temple, there is a very precious series of records which is certainly not at all modern. It represents a good many of the Egyptian constellations. The central part was in all probability the zenith point of Denderah itself, and at a certain distance from the centre point we have the zodiac represented excentrically. The constellations round the edge are those nearest the horizon; the central ones are those nearest the north pole; instead of having the Great Bear, we have the constellation of the Thigh, representing the well-known seven stars; in addition we have the constellation Hippopotamus, which has now entirely disappeared. There is also a Babylonian zodiac, which will show you that, although Babylonia and Egypt were adjacent countries, yet that they had a perfectly different set of constellations. Our present constellations came not from Egyptian times, but from much later—from Greek times. It is almost impossible to hope to recover the names of the constellations used by people earlier than the Greeks, but still much is to be hoped from the study of the Babylonian records. In these we have a snail being drawn along by the tail of a snake or dragon. It is quite possible that we may have there the origin of our constellation Draco, which is the northern constellation, and it is quite possible that this snail may indicate that the stars in it moved with very great slowness. But it is impossible at present to co-ordinate these different fancies together.

A very important paper has recently been published by Mr. Le Page Renouf suggesting that before the year 1500 B.C. the Egyptians really had an idea of meridional observations. These observations are recorded in several manuscripts found in tombs; they seem to have been given as a sort of charm to the people who were buried in order to enable them to get through the difficulties of the way in the nether world.

The hieroglyphs state that a particular star of a particular Egyptian constellation is seen at a particular hour of the night; we have twelve lines representing the twelve hours of the night, and it is stated that we have in these vertical lines the equivalent of the lines in our transit instruments, and that the reference "in the middle," "over the right eye," "over the right shoulder," or "over the left ear," as the case may be, is simply a reference to the position of the star.

If this should be confirmed, one of the remarkable things about the inquiry will be that the Egyptians did not hesitate to make a constellation cover very nearly 90°. In those days evidently they wished to have as few constellations including as many stars as possible, in order perhaps that things might be more easily remembered.

When the zodiac of Denderah was mentioned, I pointed out the constellation of the Hippopotamus very near the north pole. This constellation is referred to in the records in question.

Such then are some of the ideas which are suggested by the recent work of the Egyptologists. You see, I trust, that it is important that this work should be continued as closely associated as possible with astronomical ideas, because, merely taking a very small part of the area of which they have begun the consideration, we have come to the conclusion that, dealing with the temples alone, there seems a very high probability that 3000, and possibly 4000 B.C. the Egyptians had among them men with some knowledge of astronomy, and that 6000 years ago the course of the sun through the year was practically very well known, and methods had been invented by means of which it might in time be better known, and that not very long after that they not only considered questions relating to the sun, but began to take up other questions relating to the positions and the movements of the stars. It is quite probable that 1500 years B.C. at least they had an idea of meridional observations. If this be so, and if more and more can be proved, I think you will agree that, as I said before, astronomy will have a slight opportunity of repaying some of the great debt which she owes to the other sciences.

J. NORMAN LOCKYER.

THE LATER LARVAL DEVELOPMENT OF AMPHIOXUS.

THE memoir by Mr. Arthur Willey, B.Sc., of University College, London, on this subject, in the *Quart. Journ. Microsc. Science*, March 1891, deserves more than a passing notice. It is one of the most important contributions which have been made to a knowledge of this very interesting animal. In the summer of 1889, Mr. Willey was sent by Prof. Ray Lankester with the aid of a Government grant to collect the larvæ and embryos of Amphioxus at Faro, near Messina. He returned with a large series, and in the winter 1889–90 worked out in the laboratory of University College, chiefly by means of sections, the history of the formation of the atrial cavity in this animal. In a paper published jointly by Prof. Lankester and Mr. Willey (*Quart. Journ. Micr. Sci.*, August 1890), it was shown that the atrial cavity does not form, as supposed by Kowalewsky and by Rolph, as the result of a down-growth of lateral epipleura; but that it forms as a longitudinal groove which sinks inwards along the ventral surface, becoming floored in by a small horizontal growth on each side corresponding merely to that portion of the adult animal's ventral surface which lies between the two metapleura. The groove, now become a narrow tube, expands right and left, until it acquires the proportions of the adult atrial chamber.

The preserved material brought home by Mr. Willey in 1889 did not enable the observers to determine the mode of origin of the second row of gill-slits. Stages were noted in which there were as many as fourteen gill-slits of the first series (which are placed anteriorly on the animal's right side), and stages were observed, of no greater size, in which two rows of gill-slits were present—one series on the right side and one on the left side of the pharynx; whilst the mouth, which in the specimens with a single series was completely lateral (on the left side), had now taken up a median position.

Mr. Willey again visited Faro in the summer of 1890, for the purpose of determining, by the study of living transparent larvæ, exactly the mode of origin of the second row of slits, and the steps in the "symmetrization" of the larva. The brief account and few unconvincing figures given by Kowalewsky, in 1866, in relation to this matter had not commanded general confidence, although it was felt that so accurate and accomplished an observer could not have been completely mistaken. Balfour had said, in reference to Kowalewsky's observations on this matter, that he was "tempted to suppose that his observations were made on pathological specimens."

Mr. Willey completely and most successfully accomplished the object which he set before himself in his second visit to Faro, and the results obtained are given in the paper under notice, illustrated by three folding-plates. He confirmed the main feature of Kowalewsky's observations, viz. that the first row of gill-slits, after having (so far as the first eight are concerned) taken up a position on the *right* side of the pharynx, rotate downwards across the median ventral line, and rise up into position on the *left* side, whilst, simultaneously, a new series appears on the right side, not one by one, but as many as six being formed at approximately the same moment. Mr. Willey corrects Kowalewsky's brief account in one or two numerical details, and adds some very important facts, which are quite new. He shows: (*a*) that the anteriormost slit of the primary series closes up and disappears during the process of rotation; (*b*) that some of the hinder slits of this series, which are not far advanced when the rotation begins (there being usually fourteen, of which the last six are very small, and lie in the median ventral line), also close up; so that, when the rotation is complete, and the second series of gill-slits has advanced in development to the number of eight, a "critical phase" is reached in which there are only *eight* gill-slits on each side of the pharynx, all fairly well developed. From this time forward new gill-slits are formed on each side behind the last formed, and continue to increase in number so long as growth continues, which appears to be as long as the Amphioxus lives.

But the most important discovery made by Mr. Willey is as to the origin of the endostyle, a structure which has great importance from the fact that it can be clearly identified, on account of its minute histological structure, with the endostyle of the Ascidians.

In the anterior region of the buccal cavity, previous observers have described in very young Amphioxus larvæ (with only one gill-slit) an elongated gland; "the club-shaped gland." It opens to the exterior on the left side, just in front of the big laterally-placed mouth, whence it can be traced, bending down across the median line and passing up at *right angles* to the long axis of the body along the deep surface of the right wall of the buccal cavity. It opens at its apex, as Mr. Willey has shown, into the buccal cavity. Its earliest appearance (as described by Hatschek) resembles that of a gill-slit, though it precedes both the mouth and the first gill-slit in date. Mr. Willey suggests that it is a modified gill-slit. By the side of this club-shaped gland and in front of it, immediately associated with it, is a band-like tract of modified intra-buccal epithelium. When there are about eight gill-slits of the primary series present, it is noticeable that the apex of the club-shaped gland is bent over, so that the gland tends to become <-shaped, with the angle directed backwards; the adjoining epithelial tract faithfully follows the bend. At first the upper limb of the < is a good deal smaller than the lower, but as the primary series of gill-slits move from the right side of the pharynx to the left, the two limbs of the < become nearly equal in length, and the angle takes up a position between the primary and the new secondary series of slits. The club-shaped gland-tube now atrophies entirely, but the <-shaped epithelial tract does not; it grows rapidly at its angle along the line or interspace between the two series of slits, forming a double tract of modified epithelium consisting of parallel extensions of the two limbs of the <. It is now the epithelium of the hypopharyngeal ridge or endostyle.

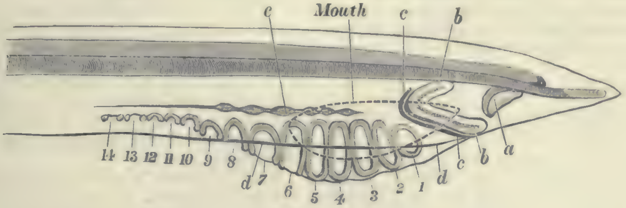
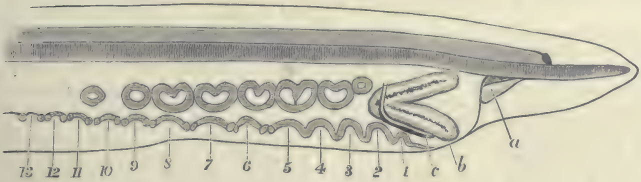


Fig. 1.



[Fig. 2.

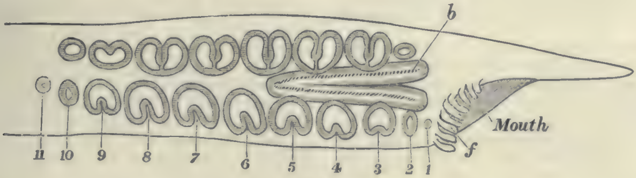


Fig. 3.

Figs. 1, 2, 3.—Diagrams showing three stages in the development of the gill-slits and endostyle of Amphioxus. Figs. 1 and 2 are seen from the right side; Fig 3 from the ventral aspect. In Fig. 1 the position and shape of the mouth, which lies on the left side of the animal, are indicated by a dotted oval. The primary series of gill-slits are numbered in all the figures. The secondary series are not numbered. Fig. 2 shows the rotation downwards of the primary series of gill-slits and their nearly complete disappearance from view on the right side; at the same time the secondary series have developed to the number of eight, and the endostyle has become <-shaped, and is pushing its angle between the two rows of gill-slits. Fig. 3 shows the atrophy of the most anterior primary gill-slit, whilst some of the hindermost have disappeared and numbers 10 and 11 are in course of closure and disappearance. *a*, præoral ciliated pit, opening on the animal's left side, but seen through the transparent integument; *b*, the endostyle (<-shaped tract of modified epithelium); *c*, the club-shaped gland; *d*, the edge of the right meta-pleur (the atrial cavity is not yet formed in the anterior pharyngeal region); *e*, the six thickenings which develop the six anterior gill-slits of the secondary (permanent right-side) series; *f*, the præoral tentacles.

Mr. Willey regards the club-shaped gland so intimately associated with the first stages of the endostyle as a modified gill-slit belonging to the secondary (the permanent right-side series). Its early development in *front* of the mouth indicates this; since, when the mouth acquires a median position (passing from the left towards the right by a relative growth, the reverse of that which brings the primary gill slits from the right to the left!), structures just in front of it would be thrown round to the right side, the side of the secondary series of slits. He suggests that it is the early-developed anterior member of the secondary series of gill-slits; and points out that just as this modified gill-slit atrophies, so does its pair in the primary series, viz. the first.

Mr. Willey points out the possible importance of these facts in reference to the views of Dohrn and of Van Beneden, and makes an interesting comparison between the Ascidian tadpole and the Amphioxus larva, with a view to suggesting some explanation of the extraordinary asymmetry of the latter. Mr. Willey thinks that a cause of the one-sided position of the mouth and of the primary series of gill-slits in the Amphioxus larva may be found in the excessive anterior prolongation of the notochord at an early period of development, necessitating a pushing to either one side or the other of the mouth. There appears to be nothing in the mode of life of the larva—a free-swimming ciliated creature—which can be correlated with its asymmetry. The gradual process of "symmetrization," by which the Amphioxus establishes more or less completely a bilateral symmetry on its way to the adult form, is exactly the converse of that process by which the symmetrical larva of the Pleuronectid fishes becomes one-sided; but in the latter case the asymmetry is clearly correlated with a peculiar life on the sea bottom, whilst in the former case we can discover no such relation to environment. E. R. L.

THE CARDIFF MEETING OF THE BRITISH ASSOCIATION.

TO arrange for the reception of the members of the British Association who will visit Cardiff in August next, an influential Local Committee has been formed, with the Most Honourable the Marquis of Bute, K.T., Mayor of Cardiff, as Chairman, and a substantial sum has been subscribed for the purpose of defraying the cost of the meeting.

Several sub-committees have been formed, all of which report to the Executive Committee, to which also the Council of the British Association has assigned the duty of electing new members and associates. Up to the present time 7 life members have been added, and over 200 annual members and associates, and as the time for the meeting approaches the number of new members and associates will be largely increased.

It may be convenient to describe what has been done by the sub-committees, so as to give a systematic account of the preparations already made and in progress to provide for the comfort and entertainment of our expected visitors.

(1) *Hospitality and Lodgings.*—Many of the principal residents in Cardiff and the neighbourhood have signified to the Committee their desire to entertain members of the Association, and as the date of the meeting draws nearer numerous additional offers will be made by those of the townsmen who are unwilling or unable to fix their engagements so long beforehand. It is understood that those ladies and gentlemen who have offered to invite guests will send out invitations as soon as it is known to the Committee who are coming.

The hotel and lodging accommodation is not so great as in some other towns, but the Committee feel sure that with the private hospitality which will be offered there will be enough for the needs of our visitors. The list of hotels and lodgings will be ready for distribution about the middle of July, it having been delayed to make the list as complete as possible. The list will be accompanied by a map of Cardiff taken from the most recently executed ones.

(2) *Reception and Section Rooms.*—The reception room will be at the Town Hall, practically the whole of which has been placed at the disposal of the Local Committee for the use of the Association. The vestibule will be devoted to the sale of tickets, the distribution of programmes, and other information, whilst the Assembly Rooms will be fitted up as a drawing-room with writing-tables, post-office facilities, and a book-stall. The Council, Committee of Recommendations, and General Committee will meet in various rooms, and others will be set apart for the officers of the Association.

As the Town Hall is about half a mile from the Section room furthest away, a portion of the Drill Hall, the use of which has been kindly granted by Lord Bute, Colonel Gaskell, and Colonel Page, will be fitted up as a drawing-room, and the remainder will be used as a luncheon-room. As the Drill Hall is situated within very easy distance of almost all the Section rooms, the members of the Association will doubtless appreciate the advantage of having a drawing-room and dining-room so close at hand.

The majority of the Section rooms are very close together, and the greatest distance is not more than half a mile; tramcars and busses, however, run frequently between the extreme points, so that even that distance should offer no difficulty in the way of members wishing to attend different Sections.

(3) *Entertainments.*—The usual *conversazioni* will be given on Thursday, August 20, and on Tuesday, the 25th, and it is hoped that scientific men will aid the Committee in contributing towards the entertainment of our guests by the exhibition of novel experiments or specimens. The Park Hall, in which the *conversazioni* will be held, is well suited to this purpose, and it is the desire of the Committee to introduce as many scientific novelties as possible.

A garden party, to which all members of the Association will be invited, will be given by Lord and Lady Bute, probably on the Friday afternoon, though the date may be subject to alteration. Other social entertainments are projected by Lord Windsor and others, and Cardiff will probably in this respect not fall behind what the members have been accustomed to at other places of meeting.

(4) *Excursions.*—A considerable variety of excursions has been provided for both the Saturday and the following Thursday. For the former, arrangements are being made by Sir W. T. Lewis for a party of members to visit the Cardiff Docks; by a committee appointed by the Board of Directors to visit the Barry Docks; by the Mayor of Newport and the Chamber of Commerce for a party to visit Newport and Caerleon. A special excursion is being arranged by the Colonel commanding the Severn Valley division of submarine miners for officers of the British Army to inspect the Severn Valley defences. The steamer will land the officers at the steep and flat holmes, and will continue with the civilians on board to Weston, from which they will visit Worlebury Hill and camp.

Other excursions will be of geological and archæological interest, and will include excursions to Penarth and Lavernock, where the finest section of Rhætic beds in England is exposed; to the interesting dolmens at St. Nicholas and St. Lythan's; to Llantwit-major, where a year or two ago the remains of a Roman villa were unearthed, and where a college is said to have existed in the fourth century; to Tintern Abbey and Raglan Castle, the Forest of Dean, Merthyr, Brecon, and to some of the numerous collieries and iron-works in the South Wales coal-field. A practical natural history excursion is being organized by the Cardiff Naturalists' Society to the Vale of Neath, which from the beauty of the spot should prove attractive. Several owners of works in the neighbourhood of Cardiff have expressed their willingness to throw them open to the members, and arrangements will be made for visits to some of them.

(5) *Publications.*—A guide-book to Cardiff is being prepared for distribution to all members and associates, and the descriptive articles have been intrusted to the gentlemen who were best fitted to write them. The article on the history and archæology of Glamorganshire has been written by the veteran G. T. Clark, of Dowlais, whilst that on the topography of Cardiff was undertaken by the late James A. Corbett, who, unfortunately, died before it was quite complete. Mr. T. Forster Brown, President of Section G, has undertaken the description of the mining, geological, and statistical features of the district; the industrial portion being in the hands of Mr.

Galloway. The geological, zoological, and botanical descriptions have been written by Mr. T. H. Thomas and Prof. W. N. Parker, with the help of many others. The account of the educational arrangements of Cardiff will be treated of by Mr. Whitmell, Inspector of Schools, and Principal J. V. Jones.

The excursions hand-book will contain a map, on a scale of four miles to the inch, of the whole of the district in which the excursions will be held, specially prepared for the Committee by Messrs. Bartholomew and Co., Edinburgh. As detailed accounts as possible of the various points to be seen in the excursions will be given by those having special knowledge: taken together with the guide-book, it is thought that a very complete description of everything connected with this portion of South Wales will be furnished to the visitors.

Other Committees have been formed for the evening lectures and the working men's lecture, but little more can be said about them than that they will provide to the fullest extent for the wants of the Association. The Local Committee are anxious that this shall be the case in every particular, so that the first visit to the metropolis of Wales will not suffer in comparison with previous meetings of the Association. R. W. ATKINSON.

MARINE BIOLOGICAL ASSOCIATION OF THE UNITED KINGDOM.

WE have received the annual report of the Council of this Association, presented at the general meeting on June 24—the President, Prof. Ray Lankester, F.R.S., in the chair. In the sea, as well as on land, the severe winter appears to have had a marked effect on the fauna, and there is also a complaint of mortality in the aquarium attached to the Laboratory during the colder months; a result perhaps somewhat unexpected, considering the comparatively high winter temperature of the sea. We are glad to learn that a self-sown fauna is springing up in the tanks, the condition of which is said to be steadily improving, as is the case with all aquaria after one or two years of use.

Under the head of the library (which ought to be represented in the balance-sheet by a larger sum than is at present debited to it) the Association is to be congratulated on having received the gift of the late Mr. Spence Bate's library, constituting an exceedingly valuable collection of the literature of Crustacea.

Some of the changes made in the permanent staff have been chronicled already: Mr. Calderwood has replaced Mr. Bourne as Director, and has appointed Mr. H. N. Dickson to succeed Mr. Garstang, who took up a Fellowship at the Owens College in December last. Two temporary members have been added to the staff: Mr. F. Hughes, to carry out from the chemical point of view an inquiry into the possibility of manufacturing an artificial bait; Mr. E. W. L. Holt, known already as the author of some papers on Teleostean development, to conduct investigations into the immature fish question as regards the Dogger Bank and the region eastwards of it—the lines of this latter inquiry are sketched in an appendix to the report. Among the fishery investigations of the past year are quoted experiments on the rate of growth and the age of sexual maturity in food-fish, oyster and lobster culture, and the anchovy fishery which the Association desires to initiate. We are glad to see that systematic physical observations are to be taken at the Laboratory in future.

Eleven gentlemen have visited the Laboratory during the year for the purposes of research, some of them on more than one occasion. This number, however, is by no means as large as it should be.

The balance-sheet shows a satisfactory, if small, increase in receipts, the items pointing to an increased use of the Laboratory, both for research and for the purchase of material for teaching purposes. A sum of £500 (in addition to the annual grant of £500) has been placed on the Civil Service estimates for the current year, which will, if passed, place the Association in a position to carry on its work with less difficulty than has hitherto been the case.

UNIVERSITY EXTENSION STUDENTS AT CAMBRIDGE.

THE work done by University Extension students at Cambridge last year was so satisfactory that the Syndicate for local lectures are encouraged to repeat the experiment this year. They will be prepared to receive a larger number of students, say from 60 to 80, most of whom will be lodged either at Selwyn College or at Newnham College. The period of study will last from July 28 to August 22, or nearly a month in all. The Syndicate have just issued a detailed programme of the various courses of study; and we are glad to see that due attention has been paid to the claims of science as well as to those of literature and art. At the chemical laboratory, on alternate days, there will be a course of demonstrations illustrating the methods of chemical manipulation in a short series of typical experiments. The pupils will be first shown each experiment, and will then be expected to repeat it for themselves. At the Cavendish Laboratory, on alternate days, a course of short experimental lectures, chiefly on electricity and magnetism, will be delivered; and most of the experiments shown in the lectures will afterwards be performed by the students for themselves. Geology will be studied, on alternate days, at the Woodwardian Museum, where there will be a course of demonstrations on the leading fossil types of the animal kingdom, from the specimens in the Museum. A course of demonstrations, followed by practical work, will be given, on alternate days, in the physiological laboratory; and Mr. Graham, chief assistant at the Observatory, will receive students and explain the uses of astronomical instruments. Arrangements will also be made for taking small parties of students to the Observatory at night. Single lectures will be delivered by various eminent Cambridge men, and in this part of the work science will be represented by Prof. G. H. Darwin, who will lecture on the history of the moon or some allied subject. We may note that the students in science will be allowed to read in the Philosophical Library.

NORMAN R. POGSON, C.I.E.

WE regret to have to announce the death of Mr. Norman Pogson, for thirty years the Director of the Observatory at Madras. Mr. Pogson has been so long absent from England that, in a sense, he may be said to have outlived his reputation; but those who can recall the condition of astronomy in this country some thirty years since will remember him as a rising astronomer of considerable promise, and as one of the most indefatigable observers at that time. If his subsequent career has not entirely fulfilled his early promise, perhaps the condition of the Madras Observatory is to some extent the cause. We believe that its astronomical equipment is very old and inadequate, and possibly Mr. Pogson has accomplished all that could be done with his instruments and his staff.

Mr. Pogson's astronomical career commenced at Mr. Bishop's Observatory in Regent's Park, at that time under the direction of Mr. J. R. Hind, and he there took part in the observations for forming the ecliptic charts published from that Observatory. In 1851 he left London to assume an assistantship in the Radcliffe Observatory, Oxford, under the late Mr. Johnson; and there his zeal was rewarded by the discovery of several minor planets, in days when the number of the known asteroids was comparatively small, and their discovery conferred some little distinction upon their fortunate discoverer. Of greater importance to astronomy was his subsequent devotion to variable stars and photometry, the latter carried out, we believe, with the apparently inappropriate instrumental means of the heliometer of the Radcliffe Observatory, Oxford. But the result of his investigation of the amount of light that separates two consecutive magnitudes has never been displaced, and the fortunate employment of the number, whose logarithm is 0·4, to express this ratio will probably long connect Mr. Pogson's name with the history of accurate photometry.

After a somewhat short stay at the Hartwell Observatory, Mr. Pogson left England in 1861 to take charge of the Madras Observatory. His direction of that institution will always be remembered in connection with the extraordinary discovery of a telescopic comet, effected in consequence of the telegraphic communication he received from Prof. Klinkerfues, who expected that Biela's comet might be seen in the constellation Centaur, after the brilliant meteoric shower to which that comet had given rise in November 1872. Mr. Pogson looked in the direction indicated, and by a remarkable coincidence found a comet, which he observed on two, and only two, occasions. The orbit remains, therefore, indeterminate, but there is good reason to believe that the object seen was in no way connected with either of the two condensations which together make up the lost comet of Biela. And thus another and not uninteresting chapter was added to the history of this comet. Several volumes of observations have been published under Mr. Pogson's direction; the last bears the date of 1870, so that probably, and as the Director has often lamented, the reductions are considerably in arrear.

It will be interesting to watch the future of this Observatory. It is to be hoped that some steps will be taken to place it more in accordance with the requirements of the present time. We believe that its abandonment has even been canvassed, but it cannot be sufficiently regretted if an Observatory, possessing as that does many historical associations, and occupying a very favourable position on the earth's surface, be allowed to disappear.

W. E. P.

NOTES.

The death of Wilhelm Weber, the illustrious physicist, is announced. He died at Göttingen on June 23. On a future occasion we shall give some account of his services to science.

The second lecture in connection with the Faraday Centenary was delivered by Prof. Dewar, F.R.S., at the Royal Institution on Friday evening last.

On Tuesday, Lord Cranbrook, in the House of Lords, moved the second reading of a Bill the object of which is to allow the managers of science and art schools to transfer them to local authorities when they desire to do so. Lord Cranbrook explained that at present there were considerable difficulties in the way, and that the process was a very long and tedious one. The Bill proposed to make these schools transferable in the same way as ordinary schools could be transferred to School Boards. The Bill was read a second time.

Drs. J. Bornmüller and P. Sintenis propose to occupy the present summer with an investigation of the flora of the islands Samothrace and Thasos, from which very few collections are to be found in European herbaria; also of Mount Athos and of the Bithynian Olympus. They then intend to take up their winter quarters in Mossul, and to spend the following spring and early summer in the comparatively unknown mountainous region of Djebel Hamzin near Bagdad, and the mountains to the north and east of Mossul.

The distinguished Italian botanist, Prof. O. Penzig, is about to start on a botanical expedition to Massowah and Bogos.

Mr. J. T. Nicolson, at present Prof. Ewing's demonstrator in the University of Cambridge, has been appointed to the Chair of Mechanical Engineering in the McGill University, Montreal.

A stall for the sale of "zoological photographs" has just been opened in the Zoological Society's Gardens. It is placed in the centre of the Gardens, near the band-stand, and has an attractive exterior. The photographs sold are mostly representations of animals in the Society's Gardens, but also include some taken in the Jardin d'Acclimatation of Paris, and in other similar establishments.

The marine laboratory of the Johns Hopkins University will be open this summer at Port Antonio on the north-east coast of Jamaica. According to *Science* of June 19, Prof. Brooks and some members of his party had already started for the station.

There has been lately formed in Berlin (we learn from *Naturw. Rdsch.*) a "Union of friends of Astronomy and Cosmical Physics," with the view of organizing practical co-operation in these subjects of research in Germany, Austria, Hungary, Switzerland, and neighbouring countries, and also in the colonies, and where membership may be desired. The object is to be sought by means of free communications of the members or groups of members to head-quarters, whence advice and results of observations, &c., will be issued. Sections are formed for observations (1) of the sun; (2) of the moon; (3) of the intensity and colour of starlight and of the Milky Way; (4) of the zodiacal light and meteors; (5) of polar light, terrestrial magnetism, earth currents, and atmospheric electricity; and (6) of clouds, halos, and thunderstorms. Prof. Lehman-Filhés has been elected President of the Union, and the presidents of the sections are Förster, M. W. Meyer, Plassmann, Jesse, Weinstein, and Reimann.

According to a telegram sent through Reuter's Agency from San Francisco on June 29, a series of sudden sharp earthquake shocks, accompanied by subterranean rumblings, passed through San José, California, that morning. The first shock was so violent that the electric-light tower, two hundred and forty feet high, swayed for at least ten feet. A panic prevailed in the town; and in two of the principal hotels, which were filled with tourists from the East, men and women rushed half-dressed from their rooms into the corridors in a great state of alarm. The city rocked like a ship in the trough of the sea, and when the second shock occurred, buildings rose and fell with a slow undulating motion, one partly erected brick building tumbling to the ground. Many chimneys fell, and a large number of windows were broken, while considerable damage was done to crockery and other fragile articles in the houses.

Germany had very heavy rains on November 22 to 24 last year, causing floods at a rather unusual time in the region of the Elbe, Weser, &c. It is shown by Prof. Hellmann, that Middle and West Germany were then on the front side of a deep depression, which passed very slowly from north to south, taking about 90 hours from the North Sea to Central Germany, less than half the usual speed from west to east. A region of high pressure with cold lay to the east, blocking the course in that direction, and this afterwards spread over the flooded country, covering it with ice.

THE Central Meteorological Observatory at Tokio, Japan, has begun the publication of hourly meteorological observations, commencing with January 1890. The observations are contained in monthly Bulletins, and include all the usual elements, together with vapour tension, humidity, earth temperature, bright sunshine, and hourly and daily means. Meteorological observations have been made for some years in various parts of Japan, including hourly observations at Tokio since January 1, 1886, but have hitherto only been published for certain hours. The observations are all made without self-recording instruments, excepting those of wind and sunshine. Some years ago the Director of the Service, I. Arai, visited this country, and other European countries, for the purpose of studying the various meteorological organizations, and we have no doubt that this important publication will be very valuable for meteorological researches referring to the North Pacific Ocean, where information is comparatively scanty.

M. MASPERO has an interesting article in the current number of *La Nature* on the dog in ancient Egypt. It is illustrated by representations of dogs reproduced from Egyptian monuments, and by a mummy of a dog recently opened and sketched by M. Beckmann. In ancient Egypt, as in modern Europe, the dog was regarded both as a friend and as a useful servant. He also received the honours of a god, and there are cemeteries of dogs (corresponding to the cemeteries of cats) where mummies have been found by the thousand. Attempts have been made to identify the various species of dogs represented in wall paintings, but those naturalists who have investigated the subject have not always arrived at the same conclusions. M. Maspero points out that mummies supply more trustworthy materials for study, and urges that men of science should lose no time in examining some of them, as cemeteries of animals are being very rapidly "exploited."

A COMMERCIAL company has for some time been working quarries in the neighbourhood of the well-known glacial grooves at Kelley Island, Ohio; and it was feared that these remarkable relics of the glacial epoch might be wholly destroyed. Fortunately the president of the company understands the interest of the phenomena, and has taken care to prevent the most striking of them from being injured. We learn from the Cleveland *Leader* that some of the grooves have now been rendered safe, the company at its recent annual meeting having decided that the rocks on which they are furrowed should be made over to the president, by whom they will be transferred to a scientific or historical society, "to be preserved in perpetuity for the benefit of science."

MR. C. DAVIES SHERBORN is, we are glad to find, making satisfactory progress with the stupendous task he has undertaken in the production of his "Index Generum et Specierum Animalium." Mr. Sherborn has found it absolutely necessary to accept the year 1758, the date of the tenth edition of Linnæus's "Systema," instead of the twelfth edition (1766), as the starting-point of binomial nomenclature in zoology, and this decision was greatly strengthened by the advice of Prof. Sven Loven, Dr. D. Sharp, and others who had carefully studied the question. This is the only alteration which has been made in the original scheme (see NATURE, vol. xlii. p. 54). During the year, five hundred volumes have been worked through, page by page, and a total of forty thousand species have been recorded, *in duplicate*, involving a use of 80,000 slips. Each species is recorded on a separate slip (5 inches × 2¼), the whole of the *reference*, with the sole exception of the page, being printed with india-rubber type, thus insuring perfect accuracy of date and parts of volumes: as the pages are also checked during work, the chances of misquotation are reduced to a minimum. As the volumes mentioned include the whole of the publications of Linnæus, many of Fabricius, Thunberg, and other voluminous authors of that early period, it is, perhaps, permissible to think that more rapid progress may be made in future years. The dates of publication of the separate parts of a work have been carefully attended to, and much valuable information has been obtained. Some of this has appeared in the *Annals of Natural History* (Pallas's "Icones Insect.," "Nov. Spec. Quad.," and White's "Journal"), while much remains in manuscript until the final completion of detail admits of its publication. As is well known, the authorities of the Natural History Museum have rendered every facility to Mr. Sherborn for the prosecution of his work, and the storage of the manuscripts within the walls of that institution, reducing the risk of loss by fire to a minimum, is a concession highly valued by the author. One set of the slips is arranged in order of genera, and, on application, is available for reference to anyone compiling a monograph of a genus. The manuscript is frequently consulted by those working at the Natural History Museum, even in its present imperfect state, and will, from the very nature of the method of recording, prove of increasing value as it grows to larger proportions.

IN the report of the trustees of the South African Museum for 1890 it is stated that the curator, Mr. R. Trimen, has completed a thorough rearrangement of the fine collection of South African Diurnal Lepidoptera in accordance with the monograph of those insects recently published by him, incorporating many additional species, and replacing imperfect or worn examples by fresher and more characteristic specimens. He has also begun the rearrangement of the more numerous and less known Crepuscular and Nocturnal Lepidoptera. Mr. Trimen has completed for publication two papers—one on the very interesting series of butterflies collected in South-West Tropical Africa by Mr. A. W. Eriksson, and presented by that explorer to the Museum in 1888; and the other on some additions to the list of extra-tropical South African butterflies since the publication of the concluding volume of his work.

AN interesting account of the nest and eggs of the cat-bird (*Ailurœdus viridis*, Latham) is given by Mr. A. J. North in the latest number of the Records of the Australian Museum (vol. i., No. 6). The habitat of the cat-bird is the dense scrubs of the coastal ranges of New South Wales. Although the bird is common, authentic specimens of its nest and eggs seem to have been unknown until lately. For an opportunity of examining such specimens, Mr. North is indebted to Mr. W. J. Grimes, an enthusiastic oologist, who recently secured two nests of this species on the Tweed River. The nest is a beautiful structure, being bowl-shaped, and composed exteriorly of long twigs, entwined around the large broad leaves of *Ptarietia argyrodendron*, and other broad-leaved trees, some of the leaves measuring eleven inches in length by four inches in breadth. The leaves appear to have been picked when green, so beautifully do they fit the rounded form of the nest, one side of which is almost hidden by them. The interior of the nest is lined entirely with fine twigs. The eggs are two in number for a sitting, oval in form, being but slightly compressed at the smaller end, of a uniform creamy white very faintly tinged with green, the shell being comparatively smooth and slightly glossy. Although the cat-bird is usually included in the family of bower-building birds, Mr. North has never known or heard of its constructing a bower.

A CATALOGUE of the Australian birds in the Australian Museum, at Sydney, by Dr. E. P. Ramsay, is being published. Part III., which has just been issued, deals with Psittaci.

AS a substance peculiarly fitted, by reason of its high dispersive power, and transparency for ultra-violet rays, for study of the ultra-violet part of the spectrum, Herr Wolter has recently recommended, in a Hamburg serial, α-monobromnaphtalin.

With a prism of the liquid, he could trace the spectrum beyond N on a fluoresceïn-solution. Besides the above-named properties, the substance has for boiling-point 277° C.; it has no offensive smell like carbon sulphide, and its index of refraction varies much less with temperature than in the case of that liquid.

THE material resources of the southern part of Maryland are still so imperfectly known that a scientific expedition for the investigation of the district was recently organized. The expedition was formed under the joint auspices of the Johns Hopkins University, the Maryland Agricultural College, and the U.S. Geological Survey. An interesting report of the work done has been published in one of the Johns Hopkins University Circulars.

DR. ALFRED TUCKERMAN has compiled an excellent "Bibliography of the Chemical Influence of Light," which has been published as one of the Smithsonian miscellaneous collections. As the compiler had in view only the scientific aspects of the subject, he has omitted nearly all the practical applications, including that of photography. An index to the literature of photography is being prepared under the auspices of the committee for indexing chemical literature, of the American Association for the Advancement of Science.

THE College of Science, Imperial University, Japan, has issued the first part of the fourth volume of its Journal. It opens with a paper on the fœtal membranes of Chelonia, by K. Mitsukuri. After this come the following articles:—On the development of Araneina, by Kamakichi Kishinouye; observations on fresh-water Polyzoa, by A. Oka; on *Diplozoon nipponicum*, n. sp., by Seitaro Goto; a new species of Hymenomycetous Fungus injurious to the mulberry tree, by Nobujiro Tanaka; notes on the irritability of the stigma, by M. Miyoshi; notes on the development of the suprarenal bodies in the mouse, by Masamaro Inaba. Each of the papers is illustrated.

MR. C. C. VEVERS, Leeds, has sent us a copy of the fourth edition, illustrated, of his "Practical Amateur Photography." The volume is described in the preface as "a simple text-book for the beginner, and a handy work of reference for the advanced photographer." Mr. Vevers has also published an illustrated catalogue of photographic apparatus.

THE Manchester Microscopical Society has issued its Transactions and Annual Report, 1890. The volume includes two Presidential addresses by Prof. Milnes Marshall, papers and communications read by the members, and a list of members.

WE have received from Mr. William F. Clay, Edinburgh, a catalogue of scientific books which he offers for sale. The works relate to chemistry and allied sciences.

AS briefly announced in our report of the last meeting of the Paris Academy of Sciences a new compound of iron and carbon monoxide has been obtained by M. Berthelot, analogous to the nickel compound described last year by Messrs. Mond, Lang, and Quincke. In order to obtain it, the iron requires to be in a very finely divided state, and free from admixed oxide. It is most suitably obtained by reducing dried precipitated ferric oxide or oxide obtained by ignition of ferrous oxalate in a current of pure hydrogen. When carbon monoxide is led over metallic iron thus prepared, and the tube containing it gently warmed to about 45° C., the reaction commences, and if the issuing gas, after being washed through water, is ignited at a jet, the flame is observed to be quite different from that of pure carbon monoxide, being brilliantly luminous, almost white, and emitting rays which furnish a definite spectrum. Moreover, if a cold porcelain tile or evaporating basin is depressed upon the flame a deposit of metallic iron more or less admixed with oxide is obtained, indicating the existence in the issuing gas of the vapour of a ferruginous compound. A drop of dilute hydrochloric acid at once dissolves the stain, and the solution affords the ordinary reactions of iron, yielding Prussian blue with potassium ferrocyanide for instance. When the gases are passed through a strictured tube, such as is employed in Marsh's arsenic apparatus, a portion of which is heated to redness, an annular deposit of metallic iron is obtained, containing a slight amount of admixed carbon. M. Berthelot has not yet succeeded in obtaining sufficient of the new compound to condense it to the liquid form, but further experiments with that end in view are in progress. The formation of this volatile compound of iron and carbon monoxide will undoubtedly prove of great interest from a metallurgical point of view, as it may assist in elucidating several of the as yet little understood furnace reactions. M. Berthelot further expresses the opinion that it may help to explain the formation of bubble flaws in manufactured iron, which have so frequently led to such unfortunate results. In addition to the preparation of iron-carbonyl, M. Berthelot describes several new reactions of nickel carbonyl. It will be remembered that this substance is a liquid boiling at 46°, so volatile that, according to M. Berthelot, its vapour tension at 16° is a quarter of an atmosphere. A drop placed upon a glass plate rapidly volatilizes, the portion last to disappear being for a few moments cooled down by the evaporation of the first portion to such an extent as to form beautiful little crystals. When suddenly heated to 70° it detonates, the detonating reaction being expressed by the equation $Ni(CO)_4 = 2CO_2 + 2C + Ni$. When mixed with oxygen, simple agitation of the tube containing it over mercury brings about detonation. When oxygen is permitted to slowly gain access to the liquid oxide, a solid substance is formed, which is green if the oxygen is moist and brownish-yellow if dry. In contact with oil of vitriol the liquid compound appears to be unaffected for a few moments, but suddenly explodes with production of flame. Nitric oxide reacts in a most beautiful manner, either when passed into the liquid or its vapour, bright blue fumes being produced of a complex compound, which eventually subside, forming a blue solid. These blue vapours completely fill the whole vessel, and their formation affords one of the prettiest experiments yet described.

CONTEMPORANEOUSLY with the above work of M. Berthelot, Mr. Mond and his co-workers have also been conducting experiments with the view to the preparation of iron carbonyl, which have been so successful that a brief account of them was laid before the Chemical Society at their last meeting. Further particulars of these experiments will be given as soon as published.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Mr. Albert Job; an American Red Fox (*Canis fulvus*) from North America, presented by Mr. W. Reading; a Two-spotted Paradoxure (*Nandinia binotata*) from West Africa, presented by Mr. E. G. Parkinson; a Sinaitic Ibex (*Capra sinaitic*) from Palestine, presented by Sir James Anderson; two Gaimard's Rat-Kangaroos (*Hypsiprymnus gaimardi*) from Australia, presented by Mr. Walter Howker; a Cuckoo (*Cuculus canorus*), British, presented by Mr. Stacy Marks, R.A., F.Z.S.; two Red-billed Tree Ducks (*Dendrocygna autumnalis*) from America, presented by Mr. Keswick; two White-faced Tree Ducks (*Dendrocygna viduata*) from Brazil, presented by Captain C. A. Findlay, R.N.R.; a Common Viper (*Vipera berus*), British, presented by Mr. J. Sargeant; two White-headed Sea-Eagles (*Haliaëtus leucocephalus*) from North America, deposited; a Burchell's Zebra (*Equus burchelli* ♂), a Derbian Wallaby (*Halmaturus derbianus*), three Common Night Herons (*Nycticorax griseus*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE CAPTURE THEORY OF COMETS.—The last three numbers of the *Bulletin Astronomique* (April–June) have contained papers by M. L. Schulhof, "Sur les Grandes Perturbations des Comètes Périodiques," which place beyond doubt the idea that the periodic comets of our system are captured by the perturbing action of planets. The main object of the research was to develop the relations existing between the elements of the comet's orbit before and after its entrance within the sphere of activity of the disturbing planet. With the criteria obtained, and some results previously formulated by M. Tisserand, it is possible to decide the question as to the identity of two comets of which the time of revolution of one is known, even when the comet is believed to have passed several times within Jupiter's sphere of activity between two apparitions. This result is of the highest importance, for it is only by such means that individual comets can be identified. They cannot be recognized by their appearance, as they possess no peculiar characteristic that can be telescopically observed.

M. Schulhof suggests that, in the light of recent work, periodic comets should not now be classified according to their aphelion distances, but arranged in groups the mean aphelion distance of which approximates to the length of the semi-major axis of one or other of the planets. Such a division has been made for comets having periods between 10 and 10,000 years. From the tabulated results, it appears that four comets have aphelion distances which differ but little from the aphelion distance of Mercury. The Venus group numbers seven, the earth's group ten. Mars possesses four comets, and Jupiter twenty-three. Saturn has a family of nine, Uranus eight, and Neptune five.

WOLF'S PERIODIC COMET (*b* 1891).—The following ephemeris is from one given by Prof. Berberich in *Edinburgh Circular* No. 17. From *Astronomische Nachrichten*, No. 3042, it appears that Dr. Spitalier observed this comet on May 2; that is, before Prof. Barnard. The brightness on the date (May 4) of rediscovery by the latter observer has been taken as unity.

Ephemeris for Berlin Midnight.

1891.	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.	Brightness.
July 6	1 4 34	+ 26 30·3	0·1916	0·2305	3·08
,, 10	1 15 8	27 1·5	0·1800	0·2270	3·30
,, 14	1 25 49	27 28·3	0·1683	0·2237	3·54
,, 18	1 36 35	27 50·5	0·1565	0·2206	3·80
,, 22	1 47 24	28 7·6	0·1446	0·2178	4·06
,, 26	1 58 15	28 19·3	0·1326	0·2152	4·34
,, 30	2 9 7	28 25·2	0·1204	0·2127	4·64
Aug. 3	2 19 57	28 25·1	0·1081	0·2105	4·96
,, 7	2 30 42	28 18·6	0·0957	0·2085	5·31
,, 11	2 41 21	28 5·2	0·0832	0·2068	5·67
,, 15	2 51 51	27 44·7	0·0707	0·2053	6·05
,, 19	3 2 10	27 16·6	0·0581	0·2041	6·44
,, 23	3 12 14	26 40·9	0·0455	0·2032	6·85
,, 27	3 22 0	25 57·3	0·0329	0·2026	7·28
,, 31	3 31 26	25 5·6	0·0204	0·2022	7·72
Sept. 4	3 40 28	24 5·6	0·0080	0·2021	8·18
,, 8	3 49 1	+ 22 57·1	9·9957	0·2024	8·64

The comet is now in Pisces, and in the beginning of September will pass through the Pleiades. M. Bigourdan, of Paris Observatory, observed it on June 12, and remarked that it was "une nébulosité ronde, d'environ 20″ de diamètre, de grandeur 13·3."

YORUBA AND GAZALAND.

AT the meeting of the Royal Geographical Society, on Monday, two papers were read: one by Mr. Alvan Millson, on the Yoruba country, West Africa, and the other by Mr. Denis Doyle, on a journey in Gazaland, in South-East Africa.

The ancient kingdom of Yoruba may be taken as one of the most interesting of the great tribal divisions of West Africa, between the Gold Coast and the Niger.

Landing at Lagos, the only natural harbour on a thousand miles of coast, a narrow entrance with a 15-feet bar leads into the intricate chain of waterways which extends, with few and slight interruptions, for 500 miles from the Volta river to the Benin branch of the Niger Delta. From the east and west, from the Benin river and the waters of the Dahomian frontier, the coast of the gulf is backed by intersecting channels of fresh water flowing steadily from either hand towards the Lagos outlet. In many places these narrow and brimming channels are separated from the onslaught of the Atlantic rollers by no more than five or six level yards of shifting sand; the spray from the ocean drifts over them, and the roar of the surf is heard by the native as he glides over their calm surface in his fragile canoe. These so-called "lagoons of the Bight of Benin" form but a small portion of the littoral river systems of West Africa; for from Cape Palmas to Cape Three Points the long Kroo coast is lined by inland waters for the greater part of 300 miles, and beyond the rocky spurs of the beautiful Gold Coast the Dahomian shores have the same remarkable formation. At right angles to this network of channels numerous rivers flow down from the uplands of the interior, carrying in their rapid streams vast quantities of sand and mud with which they busily build out the land. At first sight it seems strange that so many and such powerful streams, flowing strongly towards the sea should suddenly be turned aside from their courses by so narrow and fragile a barrier of shifting sand. To the influence of the sheltering headlands which jut out towards the south; to the rapid Guinea current which tears away the face of their rocky shores and hurries towards the east a ceaseless stream of sand; to the almost tideless ocean, and the absence of high winds, for the strength and duration of a West African tornado are but slight as compared with the hurricanes of the West Indies or the gales of our stormy coast; and above all to the enormous growths of floating papyrus and water-grass which line the inner banks of the lagoons, and prevent the swollen waters from breaking through into the ocean, are due the formation and continual development of this strange delta system. For these rivers are in most instances choked for many miles by a floating papyrus-sod bound together by wild water-figs and palm-wine palms (*Raphia vinifera*), and when the floods come down from the interior great masses of this floating vegetation are torn away and carried down to the lagoons and onwards towards the sea. Hundreds of acres of these grass islets are annually carried down from each of these rivers, and are driven against the banks of the littoral lagoons, where they lodge and grow, and eventually become anchored in their places by more permanent vegetation. In this manner the lagoon sides are padded for hundreds of yards, and even, in some instances, for two or three miles in depth on either hand, and their banks are protected from the wash of the current and the weight of the accumulated waters. By this means the frail barrier of sea-sand is strengthened, and the inland waters, although they frequently rise to a height of 5 to 6 feet above the sea-level, are effectually prevented from bursting through their banks. Not only are these growths a permanent protection to the land, but by their very nature, floating as they do on the surface of the water, they rise and fall with the floods, and are always ready with their assistance at the right time and place. Were all the rivers which feed the lagoons freed from their natural obstructions, as is the case with the Ogun river near Lagos, the interior to a distance of from thirty to seventy miles would be thrown open to commerce, and the wonderful system of inland navigation which fosters the coast traffic would be still further developed.

Mr. Millson went on to describe a journey from the coast to the interior, the country rising from terrace to terrace. He then spoke at some length of the Yoruba people and country.

About eighty miles from the coast, at Oda Ona Kekere, the dense forest suddenly gives place to open cultivated land, and a densely peopled country. Some three miles to the north of Odo Ona Kekere, from the crest of a rising in the undulating land the great city of Ibadan—the London of Negroland—comes full in view, extending for over six miles from east to west, and for more than three from north to south. Surrounded by its farming villages, 163 in number, Ibadan counts over 200,000 souls, while within the walls of the city itself at least 120,000 people are gathered. Its sea of brown roofs covers an area of nearly 16 square miles, and the ditches and walls of hardened clay which surround it are more than 18 miles in circumference. Its houses are built round courtyards with a single entrance, and form in themselves no mean defence against native inroads. Their walls of thick "adobe" are blank on the outer face, and the thatched roofs are made of a light covering of palm leaves and grass in order to avoid the danger of extensive conflagrations. In the winding rocky streets which intersect these large compounds in every direction, are countless market booths and occasional market places, where the inhabitants can purchase native produce, food, and European luxuries. In the same way, by the sides of the country roads, are built at irregular intervals varying from one to six miles, long low sheds close by some well or running water, where the farm women sit and "make their market." In the farms which extend throughout the country from horizon to horizon as one journeys through it, save where the land is too poor, or the fear of war has desolated the neighbourhood, can be heard the crowing of cocks, the barking of dogs, the shrill laughter of children, and the vociferous clamour of native homestead gossip. For among natives, as among seafaring folk at home, a hundred yards or so is no impediment to polite conversation. From this custom arises the disadvantage that the voices of the people being naturally pitched for distant communication cannot readily be restrained or focussed for nearer ranges of social intercourse. The consequent turmoil and shrill cries are apt at first to unsettle the nerves of an inexperienced traveller, but a few weeks' residence in the country not only accustoms one to their manner of speech, but inures one's system to the sudden shock of their sonorous voices.

Northward from Ibadan, which may be described as the centre of the chief military and commercial power in Yoruba, two days' journey—about 40 miles—through many villages, and a landscape dotted far and near with oil-palms (*Elæis guineensis*) along a road thronged with travellers, brings one to the capital of central Yoruba, Oyo (Awyaw). On leaving Ibadan, Mr. Millson passed, in the course of a morning's march, over 4700 men, women, and children, hurrying into the great city from the farm villages with loads of maize, beans, yams, yam flour, sweet potatoes, fowls, pigs, ducks; or driving cattle, sheep, and goats; or mounted on small native horses which amble quickly along under the combined influence of an Arab ring bit and an armed spur which leaves its traces in deep scores along the flanks of the poor animals. Far and wide the land has for generations, and indeed for centuries, been cultivated by these industrious natives. The hatchet, the fire and the hoe, have removed all traces of the original forest, save indeed where a dark trail of green across the landscape shows where the valley of some narrow watercourse or larger river is hidden among trees. For two or three years at most the land is allowed to lie fallow, while for three or four years double or treble crops are raised with no further cultivation than an occasional scrape with a hoe, and during its fallow time no further care is taken of it than to let a rank growth of reedy grass spring up some 6 or 8 feet in height. Among this grass can be seen the seedlings and young plants of a new forest, which would rapidly take possession were the land to be permanently deserted. In spite of this careless and exhausting method of cultivation the crops maintain an excellent average, and the same plot of ground serves for generations to support its owners.

Mr. Doyle, who accompanies King Gungunhana's two envoys to this country, described his journey from the Mashonaland plateau down through Gazaland to the mouth of the Limpopo. At first the journey was through a broken plateau country, rising to 5000 feet and over, and well adapted for farming operations. After fourteen days' travel, the country suddenly drops from a level of 4000–5000 feet to 860 feet above sea-level. For many miles the altitude was no more than 300 feet, and as it was the rainy season when Mr. Doyle and his companions passed through, they found the country almost entirely a swamp. The actual distance travelled was between 700 and 800 miles, which was traversed in forty-six days.

THE CONDITION OF SPACE.

The question of the condition of inter-planetary space, with special reference to the possibility that it offers a resistance to the passage of the heavenly bodies, has for long occupied the attention of astronomers, but is even yet far from receiving a satisfactory or definite solution. Three hypotheses seem to be more or less in vogue:—

(1) That it is filled with "ether," differing entirely in its properties from ordinary matter, and offering no resistance to the passage of solid or gaseous bodies. Radiant energy is transmitted by the vibratory motion of the ether, and possibly also the force of gravitation is transmitted by a rotatory motion, though, as Laplace points out, the velocity of the gravitation must be at least 7,000,000 times that of light.

(2) That it is filled with an ether more analogous to ordinary matter, which offers resistance, or with a highly rarefied gaseous medium similar in constitution to our atmosphere.

(3) That it is filled with ether, through which innumerable solid bodies of comparatively small size fly singly or in swarms. When they encounter one another, a gas, or a planet, they become luminous, and present the appearance of fireballs, meteorites; shooting-stars, meteors; comets, meteoric swarms; meteoric dust gives rise to the phenomenon of the aurora borealis. This theory has recently been much extended and admirably advocated by Prof. J. Norman Lockyer, in "The Meteoritic Hypothesis."

If the first hypothesis be true, and space offers no resistance to the passage of the planets, Laplace has shown (*Mém. Acad. des Sciences*, 1784) that any change in their orbits will be periodic, or, in other words, that, with only slight variations from time to time, the present condition of the solar system will continue indefinitely.

If the second hypothesis be true, the resistance, however slight it may be, will tend to retard the motion of the planets. In the case of the earth the friction between the outer layers of the atmosphere and the medium will retard the rotation of the earth, and increase the length of the day. There will also be a resistance to the motion of the earth in her orbit, which will tend to decrease the velocity, and therefore to lengthen the year; but, on the other hand, if the tangential velocity be decreased while the attraction of the sun remains the same, the earth will fall towards the sun, the mean distance will decrease, and therefore the time of revolution will be shortened.

If the third hypothesis be true, the rain of meteorites will have no effect on the rotation of the earth, but will tend to lessen the orbital velocity.

Laplace has discussed some consequences of the second hypothesis in "Mécanique Céleste," vii. 6, on secular variations in the movements of the moon and earth which might be produced by the resistance of an etherial medium spread round the sun. He assumes that the density of the medium is a function of the distance from the sun, and that the resistance varies as the square of the velocity. He concludes that the acceleration produced by the resistance of a fluid ether on the mean motion of the moon is, up to "the present time," insensible; and that the acceleration produced by the same ether on the motion of the earth would be less than 1/100 of that caused to the motion of the moon. These results are extended to other planets and to comets in x. 7, where it is shown that the distance at perihelion remains unchanged, and the only alteration in the orbit is a decrease in the length of the major axis and in the eccentricity.

The question is discussed from a mathematical point of view in several text-books (*e.g.* Tait and Steele, "Dynamics of a Particle," pp. 279, 379), but in all cases the mathematics are somewhat difficult, and various assumptions have to be made to render the solution possible.

In the case of the earth, if the resistance of the medium be small, the orbit may be considered to be circular, more especially as it follows from Laplace's results that the error introduced decreases with the time, since the orbit becomes more nearly circular. The following brief abstract of the popular treatment suggested by G. A. Hirn in his "Constitution de l'Espèce Céleste," pp. 104–108, with the substitution of English values, and the extension of the results to the meteoric hypothesis, may be not without interest at the present time.

Many of the data are so uncertain, that the rough approximations by which mathematical difficulties are avoided probably produce no great loss of arithmetical accuracy in the results.

The *vis viva* of the earth at the end of any period is equal to the *vis viva* at the commencement of the period, less the *vis viva* lost owing to the resistance of the medium, and increased by the *vis viva* due to the fall towards the sun. Transposing, and dividing by M/2—

$$v_r^2 = v_o^2 + v_s^2 - v_t^2.$$

Writing S for the attraction of the sun, and resolving along the radius vector A—

$$v_o^2/A_o = S, \quad \therefore v_o^2 = SA_o.$$

After a time t,

$$v_t^2/A = S\frac{A_o^2}{A_t}, \quad \therefore v_t^2 = SA_o^2/A_t.$$

The acceleration towards the sun is expressed by

$$\frac{d^2\mathrm{A}}{d_t^2} + \mathrm{S} \,.\, \frac{\mathrm{A}_o^2}{\mathrm{A}_t^2} = 0;$$

and integrating,

$$v_s^2 = 2\mathrm{SA}_o\left(\frac{\mathrm{A}_o}{\mathrm{A}_t} - 1\right).$$

Substituting and reducing,

$$v_r^2 = v_o^2\left(\frac{\mathrm{A}_o}{\mathrm{A}_t} - 1\right).$$

Hence the *vis viva* lost, owing to the resistance of the medium, is one-half of the *vis viva* gained by falling through $(\mathrm{A}_o - \mathrm{A}_t)$ towards the sun, and the presence of a very slightly resisting medium *increases* the velocity of the earth in its orbit. This increase is easily expressed, since, by Kepler's third law, we may replace $(\mathrm{A}_o/\mathrm{A}_t)^3$ by $(\mathrm{T}_o/\mathrm{T}_t)^2$, where $\mathrm{T}_o\mathrm{T}_t$ are the periodic times at the beginning and end of the period;

$$\therefore\ v_r^2 = v_o^2\left\{\left(\frac{\mathrm{T}_o}{\mathrm{T}_t}\right)^{\frac{2}{3}} - 1\right\}.$$

But the *vis viva* lost owing to the resistance is equal to the work done in forcing the sphere against the resistance of the medium through the distance passed over by the earth during the time. We may assume for simplicity that during the last 2000 years the length of the year has shortened by five seconds; and since the change in the radius vector would be very small, that $\mathrm{A} = 23300a$, where a is the radius of the earth, and hence that the distance through which the earth has passed is $2\pi\ 23300a\ 2000$.

M. Hirn, by theory and experiment, shows considerable reason for believing that the formula of Hutton, for the resistance of a medium in terms of the density δ, gives a result not far from the truth. Hence

$$\cdot 0451 \times (\pi a^2)^{1\cdot 1} \times \delta \times v_o^2 \times 2\pi\ 23300a\ 2000 = \frac{\mathrm{M}v_o^2}{2}\left\{\left(\frac{\mathrm{T}_o}{\mathrm{T}_t}\right)^{\frac{2}{3}} - 1\right\},$$

where

$$\left(\frac{\mathrm{T}_o}{\mathrm{T}_t}\right)^{\frac{2}{3}} - 1 = \left(1 + \frac{5}{31558150}\right)^{\frac{2}{3}} - 1 = \frac{1}{9467445}.$$

$$\therefore\ \frac{1}{\delta} = (\log^{-1} 14{\cdot}32278) \times ga^{\cdot 2}/\Delta,$$

where Δ is the absolute mass of unit volume of the material of the earth.

$$\therefore\ \frac{1}{\delta} = 5{\cdot}64 \times 10^{14} \text{ cubic feet.}$$

M. Hirn further points out that this decrease of five seconds in the length of the year during a period of 2000 years would be accompanied by a change in the longitude of the earth of more than 205″, an amount quite inadmissible since the time of Hipparchus, while the above results have shown that, to produce an acceleration so small as this, the medium must have a rarity such that one pound occupies 564 billions of cubic feet. And the volume occupied by a pound of the gas very nearly varies inversely as the number of seconds gained in the periodic time.

When we pass on to consider the retardation caused by the action of meteorites, we lose the guidance of M. Hirn, but are able to refer for data to Prof. Lockyer's treatise.

About 30 miles, or 158,400 feet per second, may be taken as the average velocity of meteorites (p. 68). Suppose the earth at rest, and struck by a meteorite weighing one pound with this velocity, the *vis viva* of the blow would be $\frac{1}{2g}(158400)^2 = 3{\cdot}98 \times 10^8$ absolute foot-pounds (p. 64).

But the earth is moving in its orbit with a velocity of 18·4 miles, or 97,130 feet per second; hence, of every three meteorites we may presume that two strike the front, and one the back hemisphere. Further, the velocity of the earth is, in the one case, to be added to, and, in the other case, subtracted from, the velocity of the meteorites. Again, we may assume that the earth is struck about equally all over each hemisphere, and that, owing to its attraction, the blows are vertical, and hence that the energy added and subtracted in each hemisphere in the direction of the motion of the earth is one-half of the total *vis viva*, or for three meteorites, each weighing a pound,

$$\frac{1}{2g}\left\{(158400 + 97130)^2 - \tfrac{1}{2}(158400 - 97130)^2\right\}$$

$$= 4{\cdot}58 \times 10^9 \text{ foot-pounds.}$$

Suppose that a meteorite weighing one pound has the specific heat 0·2, which is about double of that of iron; to raise it from −270° C. to 2000° C., 454 units of heat are required, which are equivalent to about $454 \times 44758 = 2 \times 10^7$ absolute foot-pounds of work—a quantity which may be neglected, in comparison with the total *vis viva* of the meteorite.

The weight of meteorites varies from tons to small specimens (p. 19), and hence we must assume an average weight of μ pounds. According to Newcomb, 20,000,000 meteorites a day enter our atmosphere (p. 69). We may again assume that the action has continued for 2000 years, and caused a shortening in the periodic time of five seconds.

The *vis viva* of the impacts,

$$\mu \times 4{\cdot}58 \times 10^9 \times \frac{20000000}{3} \times 365 \times 2000,$$

must be equal to the *vis viva* lost by the earth,

$$\tfrac{1}{2}\mathrm{M}v_o^2\left\{\left(\frac{\mathrm{T}_o}{\mathrm{T}_t}\right)^{\frac{2}{3}} - 1\right\}, \text{ which is } \frac{4\pi a^3 \times 10{\cdot}86 \times (97130)^2}{6 \times 9467445};$$

$$\therefore\ \mu = \frac{1{\cdot}95 \times 10^{33}}{1{\cdot}115 \times 10^{19} \times 2000 \times 9467445}$$

$$= 9240 \text{ pounds, or over 4 tons.}$$

In this case, also, the average mass of the meteorites varies inversely as the shortening of the periodic time. Thus, if the average weight of meteorites is 9 pounds, the shortening would be only 0·005 second—an amount probably inappreciable.

SYDNEY LUPTON.

THE FLOWERS OF THE PYRENEES AND THEIR FERTILIZATION BY INSECTS.[1]

THE observations described in this work were made in the Vallée de Luz (Hautes Pyrénées, France), in August 1889 and June 1890, between 900 and 2200 metres altitude. The author has noticed 1801 visits, brought by 507 different insects to 261 different flowers. In the list of the visits, date and altitude are always noted, and in many cases particulars are given about the special habits of insects in visiting flowers. Many of the mentioned insects were not before seen visiting flowers.

The contrivances by which the flowers are fertilized are described for the following species: *Merendera Bulbocodium*, *Asphodelus albus* (lepidopterophilous, proterogynous), *Hyacinthus amethystinus* (proterandrous, adapted to long-tongued bees), *Iris pyrenaica*, *Antirrhinum sempervirens*, *Linaria origanifolia* (adapted to bees, with special entrance for Lepidoptera or Bombylidæ), *Linaria pyrenaica*, *Horminum pyrenaicum* (gynomonœcious), *Scutellaria alpina* (adapted to long-tongued bees, with special entrance for Lepidoptera), *Teucrium pyrenaicum* (adapted to bees, with entrance for Lepidoptera), *Dianthus monspessulanus* (lepidopterophilous), *Alsine* sp., *Alsine verna*, *Aconitum pyrenaicum* (resembles the *A. lycoctonum*), *A. Anthora*, *Aquilegia pyrenaica*, *Brassica montana* (lepidopterophilous), *Roripa pyrenaica*, *Reseda glauca*, *Geranium cinereum* (proterandrous, gynodiœcious), *Saxifraga longifolia* (proterandrous), *Potentilla alchemilloides*, *Potentilla fragariastrum*.

Some details are given about the construction of the flowers in the following species: *Cirsium eriophorum*, *C. monspessulanum*, *Carduus medius*, *C. carlinoides*, *Centaurea Scabiosa*, *Gnaphalium Leontopodium*, *Angelica pyrenæa*. Almost all those species are illustrated (94 figures), and the explanation of each figure is given in French and in Dutch.

General conclusions:—The relative number of hemitrope Diptera (Syrphidæ, Conopidæ, and Bombylidæ), of allotrope Hymenoptera (all Hymenoptera except the bees), of long-tongued not-social bees and of Coleoptera decreases with increasing altitude. The hemitrope Diptera (all Diptera except those mentioned above) become on the contrary relatively more numerous with increasing altitude; this seems to be also the case with the social long-tongued bees (represented in the Pyrenees by *Bombus* and *Psithyrus*). Müller came to the same conclusions about the influence of altitude upon the same groups of insects in the Alps.

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[1] "De Pyreneeënbloemen en hare bevruchting door insecten," 226 pages, with five plates, a French *résumé*, and the explanation of the plates in French. In *Botanisch Jaarboek*, iii., 1891, published by the Botanical Society Dodonæa, in Ghent, Belgium).

On the other hand, Müller noticed that in the Alps the relative number of Lepidoptera increases, of hemitrope Hymenoptera (short-tongued bees) decreases in the higher parts of the mountains. The influence of altitude upon those two groups of insects is not evident in the Pyrenees.

The Lepidoptera—which in the Alps, according to Müller, are very numerous—are much less numerous in the Pyrenees. All the allotrope insects (Coleoptera, allotrope Diptera, and allotrope Hymenoptera) are relatively more numerous in the Pyrenees than in the Alps. The hemitrope Hymenoptera (short-tongued bees) are somewhat more numerous in the Pyrenees than in the Alps; the hemitrope Diptera (Syrphidæ, Conopidæ, and Bombylidæ) are almost equally represented in both the mountains. The eutrope Hymenoptera (long-tongued bees) seem to be equally numerous in the Pyrenees and in the Alps; in both countries, the humble-bees are predominant, and the not-social long-tongued bees are scarce.

The following table will enable students to compare the flora of the Pyrenees with that of the Alps :—

	Pyrenees.			Alps.	
	Species.	Per cent.		Species.	Per cent.
Pollen flowers (class Po) ...	12	(4·6)	...	14	(3·3)
Fl. with free-exposed honey (class A)	34	(13·0)	...	42	(10·1)
Fl. with partially concealed honey (AB)	45	(17·2)	...	61	(14·6)
Fl. with quite concealed honey (B).....................	37	(14·1)	...	66	(15·3)
Associated flowers with quite concealed honey (B′)	48	(18·4)	...	84	(20·2)
Flowers adapted to bees (Bb)	73	(27·9)	...	110	(26·4)
Flowers adapted to Lepidoptera (Vb)	12	(4·6)	...	39	(9·3)

The allotrope flowers (Po, A, AB) are relatively more numerous, the lepidopterophilous flowers (Vb) are less numerous in the Pyrenees than in the Alps; we have seen that the same differences exist for the corresponding groups of insects.

The hemitrope flowers (B, B′) are a little more numerous in the Alps than in the Pyrenees; the contrary occurs with the hemitrope insects. There is here accordingly no concordance in the geographical distribution between flowers and insects; but the hemitrope insects are not so constant in the choice of their flowers as the allotrope insects and the Lepidoptera; their influence upon the distribution of the corresponding flowers is therefore not so great as that of the two latter groups. The class Bb and the long-tongued bees are nearly equally represented in both the mountains. The parallelism which occurs between the relative abundance of the classes Po, A, AB, Bb, and Vb, and the relative abundance of corresponding insects, agrees very nicely with the theory of flowers.

It may be observed that in the Pyrenees, with reference to the biological floral organization, the Choripetalæ are, on the whole, on a lower level than the Sympetalæ. Only a small number of Monocotyledoneæ could be observed.

University, Ghent. J. MacLeod.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—The Council of the Senate have appointed Mr. E. Hill, of St. John's College, to be a governor of Woodbridge School, under the new scheme.

The Harkness Scholarship in Geology and Palæontology has been awarded to Herbert Kynaston, of King's College.

Mr. A. A. Kanthack has been elected to the John Lucas Walker Studentship in Pathology, vacated by the election of Mr. J. G. Adami to a Fellowship at Jesus College. Mr. Kanthack is at present in India as a member of the Leprosy Commission.

The managers of the John Lucas Walker Fund have made a grant of £60 to Mr. E. H. Hankin, Fellow of St. John's, for the purchase of bacteriological apparatus required for his researches.

I. H. Burkill, of Caius College, has been appointed Assistant Curator of the Herbarium.

Prof. Ewing advertises for a demonstrator in mechanism, who has had a workshop training in mechanical engineering. The salary is £150 a year.

The annual report of the Local Lectures Syndicate, published in the *University Reporter* of June 23, records a large amount of useful work in so-called University extension. The number of courses given in 1890-91 was 135, with an average attendance of 10,947. The average attendance at the classes held after lecture was 4916, the number of weekly papers sent in 2266, and the number of candidates examined for certificates 1547. The following passages refer to a fresh departure of considerable interest, and of far-reaching possibilities in the future :—

"The grant for technical education which has been put at the disposal of the County Councils has led to an extension of the work of the Syndicate, and it seems not improbable that if a grant of this nature is made permanent a considerable demand will be made upon their staff of lecturers. In Devonshire they have provided at the request of the County Council a Lecturer on Chemistry and a Lecturer on Mechanics, in each case with special reference to applications to agriculture. The lectures in chemistry were given at six centres, those in mechanics at five. The average weekly attendance was—at lectures about 40, at classes about 25, at each centre. In all, 64 students presented themselves for examination, of whom 44 passed, 14 obtaining distinction. The audience comprised a number of boys from elementary and secondary schools, and some working men and farmers and schoolmasters, in addition to the usual mixed audience. The lectures were of necessity arranged rather hurriedly, without sufficient time for the local authorities to complete their organization, and they can only be regarded as an experiment. The Syndicate have reason to think that the experiment has been as successful as under the circumstances could be expected. . . .

"Having regard to the probability of a considerable demand for lecturers in connection with the County Councils, the Syndicate have added to their list several new lecturers whose attainments mark them out as suitable for this work. And in order that the lecturers may have practical acquaintance with the applications of their science to the uses of agriculture, the Syndicate have arranged that they should pay visits to farms of various characters and to the experimental farm at Woburn. These visits are paid under the experienced superintendence of Mr. H. Robinson, of Downing College, the assistant to the Professor of Chemistry. Mr. Robinson conducts also a course of laboratory work with the lecturers, with special reference to agricultural investigations. The Syndicate desire to express their grateful sense of the help which Prof. Liveing and Mr. Robinson have so liberally given. The provision of teaching and guidance in Cambridge for the scientific study of subjects connected with agriculture appears to the Syndicate to be so important for the training of students who may become lecturers on their staff, that they will endeavour to secure a continuance of this assistance, and are prepared to devote a portion of their funds to the purpose."

The *Ordo Senioritatis* for the year shows that 6 D.Sc. degrees have been conferred, 19 M.D. degrees, 72 M.B., and 70 B.C. These figures bespeak the steady growth of the faculties of science and medicine, the numbers in medicine being larger than in any previous year.

SOCIETIES AND ACADEMIES.

London.

Royal Society, June 11.—"On Electrical Evaporation." By William Crookes, F.R.S.

It is well known that when a vacuum tube is furnished with internal platinum electrodes, the adjacent glass, especially near the negative pole, speedily becomes blackened, owing to the deposition of metallic platinum. The passage of the induction current greatly stimulates the motion of the residual gaseous molecules; those condensed upon and in the immediate neighbourhood of the negative pole are shot away at an immense speed in almost straight lines, the speed varying with the degree of exhaustion and with the intensity of the induced current. Platinum being used for the negative pole, not only are the gaseous molecules shot away from the electrode, but the passage of the current so affects the normal molecular motions of the metal as to remove some of the molecules from the sphere of attraction of the mass, causing them to fly off with the stream of gaseous molecules proceeding from the negative pole, and to adhere to any object near it. This property was, I believe, first pointed out by Dr. Wright, of Yale College, and some interesting experiments are described by him in the *American Journal of Science and Arts.*[1] The process has been much used for the production of small mirrors for physical apparatus.

This electrical volatilization or evaporation is very similar to ordinary evaporation by the agency of heat. Cohesion in solids varies according to physical and chemical constitution; thus every kind of solid matter requires to be raised to a certain temperature before the molecules lose their fixity of position and are rendered liquid, a result which is reached at widely different temperatures. If we consider a liquid at atmospheric pressure—say, for instance, a basin of water in an open room—at molecular distances the boundary surface between the liquid and the superincumbent gas will not be a plane, but turbulent like a a stormy ocean. The molecules at the surface of the liquid dart to and fro, rebound from their neighbours, and fly off in every direction. Their initial velocity may be either accelerated or retarded, according to the direction of impact. The result of a collision may drive a molecule in such a direction that it remains part and parcel of the liquid; on the other hand, it may be sent upwards without any diminution of speed, and it will then be carried beyond the range of attraction of neighbouring molecules and fly off into and mingle with the superincumbent gas. If a molecule of the liquid has been driven at an angle with a velocity not sufficient to carry it beyond the range of molecular attraction of the liquid, it may still escape, since, in its excursion upwards, a gaseous molecule may strike it in the right direction, and its temporary visit may be converted into permanent residence.

The intrinsic velocity of the molecules is intensified by heat and diminished by cold. If, therefore, we raise the temperature of the water without materially increasing that of the surrounding air, the excursions of the molecules of the liquid are rendered longer and the force of impact greater, and thus the escape of molecules into the upper region of gas is increased, and we say that evaporation is augmented.

If the initial velocities of the liquid molecules can be increased by any other means than by raising the temperature, so that their escape into the gas is rendered more rapid, the result may be called "evaporation" just as well as if heat had been applied.

Hitherto I have spoken of a liquid evaporating into a gas; but the same reasoning applies equally to a solid body. But whilst a solid body like platinum requires an intense heat to enable its upper stratum of molecules to pass beyond the sphere of attraction of the neighbouring molecules, experiment shows that a very moderate amount of negative electrification superadds sufficient energy to enable the upper stratum of metallic molecules to fly beyond the attractive power of the rest of the metal.

If a gaseous medium exists above the liquid or solid, it prevents to some degree the molecules from flying off. Thus both ordinary and electrical evaporation are more rapid in a vacuum than at the ordinary atmospheric pressure.

I have recently made some experiments upon the evaporation of different substances under the electric stress.

Evaporation of Cadmium.—A U-shaped tube was made, having a bulb in each limb. The platinum poles were at the extremities of each limb, and in each bulb was suspended from a small platinum hook a small lump of cadmium, the metal having been cast on to the wire. The wires were each weighed with and without the cadmium. The tube was exhausted, and the lower half of the tube was inclosed in a metal pot containing paraffin wax, the temperature being kept at 230° C. during the continuance of the experiment. A deposit around the negative pole took place almost immediately, and in five minutes the bulb surrounding it was opaque with deposited metal. The positive pole with its surrounding luminosity could be easily seen the whole time. In thirty minutes the experiment was stopped, and after all was cold the tube was opened and the wires weighed again. The results were as follows:—

	Positive pole.	Negative pole.
Original weight of cadmium	9·34 grs.	9·38 grs.
Weight after experiment	9·25 ,,	1·86 ,,
Cadmium volatilized in 30 mins. ...	0·09 ,,	7·52 ,,

Finding that cadmium volatilized so readily under the action of the induction current, a large quantity, about 350 grs., of the pure metal was sealed up in a tube, and the end of the tube containing the metal was heated to a little above the melting-point; the molten metal being made the negative pole, in a few hours the whole quantity had volatilized and condensed in a thick layer on the far end of the tube, near, but not touching, the positive pole.

Volatilization of Silver.—Silver was the next metal experimented upon. The apparatus was similar to that used for the cadmium experiments. Small lumps of pure silver were cast on the ends of platinum wires, and suspended to the inner ends of platinum terminals passing through the glass bulb. The platinum wires were protected by glass, so that only the silver balls were exposed. The whole apparatus was inclosed in a metal box lined with mica, and the temperature was kept as high as the glass would allow without softening. The apparatus was exhausted to a dark space of 3 mm., and the current was kept on for 1½ hours. The weights of silver, before and after the experiment, were as follows:—

	Positive pole.	Negative pole.
Original weight of silver ...	18·14 grs.	24·63 grs.
Weight after the experiment ...	18·13 ,,	24·44 ,,
Silver volatilized in 1½ hours ...	0·01 ,,	0·19 ,,

In this tube it was not easy to observe the spectrum of the negative pole, owing to the rapid manner in which the deposit obscured the glass. A special tube was therefore devised, of the following character:—A silver rod was attached to the platinum pole at one end of the tube, and the aluminium positive pole was at the side. The end of the tube opposite the silver pole was rounded, and the spectroscope was arranged to observe the light of the volatilizing silver "end on." In this way the deposit of silver offered no obstruction to the light, as none was deposited except on the sides of the tube surrounding the silver. At a vacuum giving a dark space of about 3 mm. from the silver, a greenish-white glow was seen to surround the metal. This glow gave a very brilliant spectrum. The spark from silver poles in air was brought into the same field of view as the vacuum glow, by means of a right-angled prism attached to the spectroscope, and the two spectra were compared. The two strong green lines of silver were visible in each spectrum; the measurements taken of their wave-lengths were 3344 and 3675, numbers which are so close to Thalén's numbers as to leave no doubt that they are the silver lines. At a pressure giving a dark space of 2 mm. the spectrum was very bright, and consisted chiefly of the two green lines and the red and green hydrogen lines. The intercalation of a Leyden jar into the circuit does not materially increase the brilliancy of the lines, but it brings out the well-known air lines. At this pressure not much silver flies off from the pole. At a higher vacuum the luminosity round the silver pole gets less and the green lines vanish. At an exhaustion of about one-millionth of an atmosphere the luminosity is feeble, the silver pole has exactly the appearance of being red-hot, and the volatilization of the metal proceeds rapidly.[1]

If, for the negative electrode, instead of a pure metal such as cadmium or silver, an alloy was used, the different components might be shot off to different distances, and in this way make an electrical separation—a sort of fractional distillation. A negative terminal was formed of clean brass, and submitted to the electrical discharge *in vacuo*; the deposit obtained was of the colour of brass throughout, and on treating the deposit chemically I could detect no separation of its component metals, copper and zinc.

A remarkable alloy of gold and aluminium, of a rich purple colour, has been kindly sent me by Prof. Roberts-Austen. Gold being very volatile in the vacuum tube, and aluminium almost fixed, this alloy was likely to give different results from those yielded by brass, where both constituents fly off with almost equal readiness. The Au-Al alloy had been cast in a clay tube, in the form of a rod 2 cm. long and about 2 mm. in diameter.

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[1] Third Series, vol. xii. p. 49, January 1877; and vol. xiv. p. 169, September 1877.

[1] Like the action producing volatilization, the "red heat" is confined to the superficial layers of molecules only. The metal instantly assumes, or loses, the appearance of red heat the moment the current is turned on or off, showing that, if the appearance is really due to a rise of temperature, it does not penetrate much below the surface. The extra activity of the metallic molecules necessary to volatilize them is, in these experiments, confined to the surface only, or the whole mass would evaporate at once, as when a metallic wire is deflagrated by the discharge of a powerful Leyden jar. When this extra activity is produced by artificial heat one of the effects is the emission of red light; so it is not unreasonable to imagine that when the extra activity is produced by electricity the emission of red light should also accompany the separation of molecules from the mass. In comparison with electricity, heat is a wasteful agent for promoting volatilization, as the whole mass must be raised to the requisite temperature to produce a surface action merely; whereas the action of electrification does not appear to penetrate much below the surface.

It was sealed in a vacuum tube as the negative pole, an aluminium pole being at the other side. Part of the alloy, where it joined the platinum wire passing through the glass, was closely surrounded with a narrow glass tube. A clean glass plate was supported about 3 mm. from the rod of alloy. After good exhaustion the induction current was passed, the alloy being kept negative. Volatilization was very slight, but at the end of half an hour a faint purple deposit was seen both on the glass plate and on the walls of the tube. On removing the rod from the apparatus it was seen that the portion which had been covered by the small glass tube retained its original purple appearance, while the part that had been exposed to electrical action had changed to the dull white colour of aluminium. Examined under the microscope, the whitened surface of the Austen alloy was seen to be pitted irregularly, with no trace of crystalline appearance.

This experiment shows that, from an alloy of gold and aluminium, the gold is the first to volatilize under electrical influence, the aluminium being left behind. The purple colour of the deposit on glass is probably due to finely-divided metallic gold. The first deposit from a negative pole of pure gold is pink; this changes to purple as the thickness increases. The purple then turns to green, which gets darker and darker until the metallic lustre of polished gold appears.

If we take several liquids of different boiling-points, put them under the same pressure, and apply the same amount of heat to each, the quantity passing from the liquid to the gaseous state will differ widely in each case.

It was interesting to try a parallel experiment with metals, to find their comparative volatility under the same conditions of temperature, pressure, and electrical influence. It was necessary to fix upon one metal as a standard of comparison, and for this purpose I selected gold, its electrical volatility being great, and it being easy to prepare in a pure state.

An apparatus was made that was practically a vacuum tube with four negative poles at one end and one positive pole at the other. By a revolving commutator I was able to make electrical connection with each of the four negative poles in succession for exactly the same length of time (about six seconds); by this means the variations in the strength of the current, the experiment lasting some hours, affected each metal alike.

The exposed surface of the various metals used as negative poles was kept uniform by taking them in the form of wires that had all been drawn through the same standard hole in the drawplate, and cutting them by gauge to a uniform length; the actual size used was 0·8 mm. in diameter and 20 mm. long.

The comparison metal, gold, had to be used in each experiment; the apparatus thus enabled me to compare three different metals each time. The length of time that the current was kept on the revolving commutator in each experiment was eight hours, making two hours of electrification for each of the four negative electrodes; the pressure was such as to give a dark space of 6 mm.

The fusible metals, tin, cadmium, and lead, when put into the apparatus in the form of wires, very quickly melted. To avoid this difficulty a special form of pole was devised. Some small circular porcelain basins were made, 9 mm. diameter; through a small hole in the bottom a short length of iron wire, 0·8 mm. in diameter, was passed, projecting downwards about 5 mm.; the basin was then filled to the brim with the metal to be tested, and was fitted into the apparatus exactly in the same way as the wires; the internal diameter of the basins at the brim was 7 mm., and the negative metal filed flat was thus formed of a circular disk 7 mm. diameter. The standard gold pole being treated in the same way, the numbers obtained for the fusible metals can be compared with gold, and take their place in the table.

The following table of the comparative volatilities was in this way obtained, taking gold as = 100:—

Palladium	108·00	Platinum	44·00
Gold	100·00	Copper	40·24
Silver	82·68	Cadmium	31·99
Lead	75·04	Nickel	10·99
Tin	56·96	Iridium	10·49
Brass	51·58	Iron	5·50

In this experiment equal surfaces of each metal were exposed to the current. By dividing the numbers so obtained by the specific gravity of the metal, the following order is found:—

Palladium	9·00	Copper	2·52
Silver	7·88	Platinum	2·02
Tin	7·76	Nickel	1·29
Lead	6·61	Iron	0·71
Gold	5·18	Iridium	0·47
Cadmium	3·72		

Aluminium and magnesium appear to be practically non-volatile under these circumstances.

The order of metals in the table shows at once that the electrical volatility in the solid state does not correspond with the order of melting-points, of atomic weights, or of any other well-known constant. The experiment with some of the typical metals was repeated, and the numbers obtained did not vary materially from those given above, showing that the order is not likely to be far wrong.

It is seen in the above table that the electrical volatility of silver is high, while that of cadmium is low. In the two earlier experiments, where cadmium and silver were taken, the cadmium negative electrode in 30 minutes lost 7·52 grs., whilst the silver negative electrode in 1½ hours only lost 0·19 gr. This apparent discrepancy is easily explained by the fact (already noted in the case of cadmium) that the maximum evaporation effect, due to electrical disturbance, takes place when the metal is at or near the point of liquefaction. If it were possible to form a negative pole *in vacuo* of molten silver, then the quantity volatilized in a given time would be probably more than that of cadmium.

Gold having proved to be readily volatile under the electric current, an experiment was tried with a view to producing a larger quantity of the volatilized metal. A tube was made having at one end a negative pole composed of a weighed brush of fine wires of pure gold, and an aluminium pole at the other end.

The tube was exhausted and the current from the induction coil put on, making the gold brush negative; the resistance of the tube was found to increase considerably as the walls became coated with metal, so much so that, to enable the current to pass through, air had to be let in after a while, depressing the gauge ½ mm.

The weight of the brush before experiment was 35·4940 grs. The induction current was kept on the tube for 14½ hours; at the end of this time the tube was opened and the brush removed. It now weighed 32·5613, showing a loss of 2·9327 grs. When heated below redness the deposited film of gold was easily removed from the walls of the tube in the form of very brilliant foil.

After having been subjected to electrical volatilization, the appearance of the residual piece of gold under the microscope, using a ¼-inch object-glass, was very like that of electrolytically deposited metal, pitted all over with minute hollows.

This experiment on the volatilization of gold having produced good coherent films of that metal, a similar experiment was tried, using a brush of platinum as a negative electrode. On referring to the table it will be seen that the electrical volatility of platinum is much lower than that of gold, but it was thought that by taking longer time a sufficient quantity might be volatilized to enable it to be removed from the tube.

The vacuum tube was exhausted to such a point as to give a dark space of 6 mm., and it was found, as in the case of gold, that as a coating of metal was deposited upon the glass the resistance rapidly increased, but in a much more marked degree, the residual gas in the tube apparently becoming absorbed as the deposition proceeded. It was necessary to let a little air into the tube about every 30 minutes, to reduce the vacuum. This appears to show that the platinum was being deposited in a porous spongy form, with great power of occluding the residual gas.

Heating the tube when it had become this way non-conducting liberated sufficient gas to depress the gauge of the pump 1 mm., and to reduce the vacuum so as to give a dark space of about 3 mm. This gas was not re-absorbed on cooling, but on passing the current for ten minutes the tube again refused to conduct, owing to absorption. The tube was again heated, with another liberation of gas, but much less than before, and this time the whole was re-absorbed on cooling.

The current was kept on this tube for 25 hours; it was then opened, but I could not remove the deposited metal except in small pieces, as it was brittle and porous. Weighing the brush that had formed, the negative pole gave the following results:—

	Grains.
Weight of platinum before experiment	10·1940
,, ,, after experiment	8·1570
Loss by volatilization in 25 hours	2·0370

Another experiment was made similar to that with gold and platinum, but using silver as the negative pole, the pure metal being formed into a brush of fine wires. Less gas was occluded during the progress of this experiment than in the case of platinum. The silver behaved the same as gold, the metal deposited freely, and the vacuum was easily kept at a dark space of 6 mm. by the very occasional admission of a trace of air. In 20 hours nearly 3 grs. of silver were volatilized. The deposit of silver was detached without difficulty from the glass in the form of bright foil.

Chemical Society, June 4.—Mr. W. Crookes, F.R.S., Vice-President, in the chair.—The following papers were read:—The molecular refraction and dispersion of various substances in solution, by Dr. J. H. Gladstone, F.R.S. The paper is a continuation of that laid before the Society in March last, and deals with solid and gaseous substances that have been dissolved in water and other liquids for examination. The results are given in several tables. In the case of organic compounds, the theoretical and experimental numbers are frequently in close agreement. Hydrogen chloride, bromide, and iodide give figures for the molecular refraction and dispersion much higher than the sum of the hydrogen and halogen as determined from the paraffin compounds, and the values rise as the dilution becomes greater. Selenious and selenic acids afford optical values much less than what would be expected from the known values of their constituents. Metaphosphoric acid does the same. The data relating to solutions of salts and alkalies will afford material for a revision of the refraction equivalents of the different metals, and of the electro-negative elements with which they are combined. Ammonia, in contrast with the hydrides of chlorine, bromine, and iodine, appears to be uniform in its optical properties, whatever the strength of the solution. The refraction equivalents of cerium, didymium, and lanthanum were found about 12·4, 16·4, and 15·5 respectively. The molecular refraction for ClO_3 in its salts dissolved in water comes out at about 18·3, that for BrO_3 at 24·9, and for IO_3 at 33·8.—The nature of solutions as elucidated by a study of the densities, heat of dissolution, and freezing-points of solutions of calcium chloride, by S. U. Pickering. The curves representing these properties were examined in the same way as those for sulphuric acid, and similar conclusions are drawn—namely, that changes of curvature, which occur at certain points which are the same whatever property is examined represent the existence of hydrates in solution. The simplest hydrates indicated consist of $CaCl_2$ with 6, 7, and $8H_2O$; more complex hydrates also exist, as in the case of sulphuric acid.—Note on a recent criticism by Mr. Sydney Lupton of the conclusions drawn from a study of various properties of sulphuric acid solutions, by S. U. Pickering. Mr. Lupton (*Phil. Mag.*, xxxi. 418) applies a single parabolic equation to a portion of one of the author's sulphuric acid density curves, where a change of curvature was supposed to exist, and shows that it represents the results accurately if the experimental error is of a certain magnitude. This magnitude is between 1000 and 10,000 per cent. greater than the ascertained magnitude, and the equation represents all errors of like signs as grouped together. Such a representation cannot disprove the existence of the particular change of curvature under examination, still less that of the 101 others examined by the author. The hydrate on which Mr. Lupton considers that his investigation throws "very grave suspicion" happens to be the one which the author has isolated in the crystalline condition. In the discussion which followed, Prof. Ramsay doubted the validity of Mr. Pickering's methods of differentiating his curves. His own experience was that it was impossible to obtain results nearer than 2 or 3 per cent. to the truth. Dr. Armstrong said that he was prepared to believe in the existence of hydrates in solution, but could not imagine that the 102 breaks in the sulphuric acid curves, for example, could be interpreted as evidence of as many distinct hydrates. He was inclined to think that the breaks might be due to change both in the complex water molecules and the sulphuric acid. He was inclined to believe that the hydrate, to which Mr. Lupton's conclusions related, did not begin to form in solution until the temperature sank to within a few degrees of its point of fusion. Dr. Morley said that a break in the curve should indicate that some new hydrate had just begun to form, but need not show what that hydrate was. Thus, a liquid of the composition $CaCl_28H_2O$ might be expected to contain, besides the hydrate $CaCl_28H_2O$, also higher and lower hydrates, such as $CaCl_29H_2O$ and $CaCl_27H_2O$. Prof. Rücker said that, in reality, Mr. Pickering's results were obtained, not by calculation, but by a method of observation and experiment applied to curves, which themselves represented the results of other experiments. It was admitted that the curves had to be specially drawn, and the scale of the co-ordinates carefully chosen, if the results were to be satisfactory, and probably the conclusions arrived at depended in a large measure on the details of this preliminary adjustment. In the case of the more striking changes in direction and curvature which were clearly visible in the original curve, the various differential curves did not add much to the information it supplied. He thought that the evidence afforded by these secondary curves of changes of curvature, not otherwise detected, was of the most untrustworthy character. Mr. Pickering said that Mr. Lupton's equation represented the rate of change of the densities as a straight line, while the figure which the actually observed rate of change formed was as different from a straight line as possible. The figures here referred to were the first differential figures (rate of change) deduced directly from the determinations themselves; the question of the accuracy attainable in differentiating a graph, raised by Prof. Ramsay, did not apply. He thought that Prof. Armstrong was somewhat rash in holding that a particular hydrate did not exist in solution at moderately high temperatures, because he had recognized it at low temperatures only, especially as he (the speaker) had been led to search for it, and finally to isolate it from results obtained at high temperatures. The multiplicity and complexity of the hydrates indicated must endanger the acceptance of his conclusions amongst chemists; and he was perfectly ready to accept any other explanation of the changes with weak solutions.—Ethylic $\alpha\alpha'$-dimethyl-$\alpha\alpha'$-diacetylpimelate and its decomposition-products, by Dr. F. S. Kipping, and J. E. Mackenzie. This paper contains an account of the preparation and properties of the following compounds: ethylic $\alpha\alpha'$-dimethyl-$\alpha\alpha'$-diacetylpimelate, $\alpha\alpha'$-dimethyl-$\alpha\alpha'$-diacetylpentane, $\alpha\alpha'$-dimethyl-α-acetylcaproic acid, $\alpha\alpha'$-dimethylpimelic acid, and ethylic-$\alpha\alpha'$-dimethylpimelate.—Volatile platinum compounds, by W. Pullinger. The author has studied the volatile compounds of platinum with chlorine and carbon monoxide described by Schützenberger. He describes their behaviour when heated in various gases; as they do not completely volatilize, a determination of the vapour-density was not possible. He describes a non-volatile compound of the formula $PtCl_6.C_2O_2$, and has also prepared the compound $PtBr_2CO$. Directions are given for the preparation of platinic bromide and iodide, from which it appears that spongy platinum readily dissolves in hot solutions of bromine in hydrobromic acid or of iodine in hydriodic acid.

Mineralogical Society, June 16.—R. H. Scott, F.R.S., President, in the chair.—The following papers were read:—On the occurrence of sapphire in Scotland, by Prof. M. Forster-Heddle.—On the optical properties of gyrolite, by Prof. M. Forster-Heddle.—On Fresnel's wave-surface, by L. Fletcher, F.R.S.

Linnean Society, June 18.—Prof. Stewart, President, in the chair.—Mr. W. H. Beeby exhibited specimens of *Hieracium protractum* and other plants collected in Shetland.—Mr. Stuart Samuel exhibited a dwarf specimen of *Acer palmatum*, and made some remarks on the dwarf trees artificially produced by the Japanese.—Mr. R. V. Sherring showed some cases of dried Bananas, and described a new method of preservation adopted in Jamaica to save waste of small parcels of fruit which would be otherwise unsaleable.—Mr. A. W. Bennett exhibited and made remarks upon a specimen of *Selaginella lepidophylla*, which was found to possess remarkable vitality, and upon proper treatment to resume its normal appearance after having been gathered some months.—Dr. R. A. Prior exhibited samples of the Spiked Star of Bethlehem (*Ornithogalum pyrenaicum*), and stated that, although described in British floras as a rare plant, it is so abundant on the hill pastures around Bath that it is brought to the market there in large quantities under the name of French asparagus, and sold for a penny a bunch.—Mr. R. A. Rolfe showed two hybrid *Odontoglossums* with the parent plants—namely, *O. Wilckeanum* (produced from *O. crispum* and *O. luteopurpureum*) and *O. excellens* (produced from *O. pescatorei* and *O. triumphans*). These had first appeared as natural hybrids out of imported plants, and the parentage was subsequently ascertained under cultivation.—On behalf of Sir George Macpherson Grant, Mr. J. E. Harting exhibited some curiously abnormal horns of the Roe Deer (the result of disease), which had been taken from an animal found dead near Forres, N.B. For the purpose of comparison he exhibited some normal heads of the Roe from other parts of Scotland and Germany, and made some remarks on the causes of variation in the size and form of the antlers to which Roe Deer were peculiarly liable.—A paper was then read by Mr. Spencer Moore on the true nature of *Callus*, and in continuation of former remarks on the same subject (Linn. Soc. Journ., Bot., vol. xxvii., Nos. 187–188). He showed that the outer sieve-plates of the fig are obliterated by a substance giving all the dye reactions of *Callus*, which does not peptonize and will not yield proteid reactions. Many of the inner sieve-plates he found to be stopped up with a proteid *Callus* resembling in every way the substance of *Ballia* stoppers, and the proteid *Callus* of the vegetable marrow. It appeared that true *Callus* would dissolve in a solution of gum-arabic, but whether by agency of a ferment or of an acid he had not yet determined.—A second paper by Mr. Spencer Moore dealt with the alleged existence of protein in the walls of vegetable cells, and the microscopical detection of glucosides therein.

PARIS.

Academy of Sciences, June 22.—M. Duchartre in the chair.—Method for the determination of the equatorial co-ordinates of the centres of the plates which are to form the photographic map of the heavens, by M. Lœwy.—On a generalization of equations relating to the theory of the functions of a complex variable, by M. Émile Picard.—On the determination of the mechanical equivalent of heat, by M. Marcel Deprez. At the meeting of June 8, M. Miculesco described an apparatus he had employed for determining the mechanical equivalent of heat. It is now remarked that the same method was employed by Hirn in the experiments made by him in 1860, and in latter years by M. d'Arsonval.—On the formation of the leaves of *Æsculus* and of *Pavia*, and on the order of appearance of their first vessels, by M. A. Trécul.—On the apparent and real glycolytic fermentation in the blood, and on a rapid and exact method of estimation of glycogen in the blood, by MM. R. Lépine and Barral.—On a telephone receiver of reduced weight and dimensions, by M. E. Mercadier.—Observations of the new asteroid discovered at Nice Observatory on June 11, by M. Charlois. Observations for position were made on June 11 and 12.—Observations of the same asteroid made at Algiers Observatory with the telescope of 0·5 metre aperture, by MM. Rambaud and Sy. Observations for position were made on June 12 and 13.—Extraordinary luminous phenomena observed on the sun, by M. E. L. Trouvelot.—On the determination of spiral surfaces according to their linear element, by M. L. Raffy.—On certain systems of spherical co-ordinates, and on the corresponding triple orthogonal systems, by M. A. Petot.—On the damping of Hertz vibrations, by M. V. Bjerknes.—Transmission of light across disturbed media, by M. A. Hurion.—On the electrolysis of barium chloride, pure or mixed with sodium chloride, by M. C. Limb. With moderate currents the author fails to obtain metallic barium; with the pure salt an infusible body of high resistance is deposited; with the mixed salts chlorine is disengaged at the anode, and, from the results of analyses given, it would appear that among the products of the electrolysis some subchloride must be formed.—The calculation of the temperature of ebullition of any liquid whatever, under all pressures, by M. G. Hinrichs.—Action of heat on solutions of chromium salts: green salts of chromium, by M. A. Recoura.—The constitution of the green chromium salts is elucidated by means of the results of experiments following thermochemical methods.—Researches on osmium, osmiamic acid, and osmiamates, by M. A. Joly. Taking the revised atomic weight of osmium, the analyses of Fritzsche and Struve, as well as those of the author, point to $KNOsO_3$, and not to $K_2N_2Os_2O_5$, as the formula denoting the composition of potassium osmiamate. Relations may be traced between osmiamic acid and the nitroso-compounds of ruthenium, $RuNOCl_3$ and $RuNO(OH)_3$. $O{=}OsNO{-}OH$ may be viewed as the first anhydride of the hypothetical $OsNO(OH)_3$.—On the alkaline zirconates, by M. L. Ouvrard.—On the bromo-iodides of silicium, by M. A. Besson.—On the cyanogen compounds of magnesium, by M. Raoul Varet.—On the action of nitric acid of different degrees of concentration upon iron at various temperatures, by MM. Henry Gautier and Georges Charpy. The writers conclude from their experimental results that "*iron is always attacked by nitric acid, whatever its concentration.*" The action may proceed in two ways—(1) rapid, and accompanied by the disengagement of gas; (2) slow, and without evolution of gas. The latter corresponds to what is known as the passive state of iron.—Action of sodium benzylate upon camphocarbonic ether, by M. J. Minguin.—Comparative influences of the sulphates of iron and calcium on the preservation of nitrogen in naked soils and on nitrification, by M. P. Pichard.—On the value of animal *débris* as nitrogenous dressing, by MM. A. Muntz and A. C. Girard.—On the development of blastodermic leaves in Crustaceæ Isopodæ (*Porcellio scaber*), by M. Louis Roule.—On the disengagement of oxygen by plants at low temperatures, by M. H. Jumelle. It appears that in plants capable of resisting excessive humidity or cold the decomposition of carbon dioxide may continue at very low temperatures, even when respiration has ceased. Conifers such as the juniper-tree, and a lichen (*Evernia prunastri*), in light can assimilate the carbon in the air in an atmosphere having a temperature as low as -30° or -40° C.—The parasitic fungi of Acridians, by MM. J. Kunckel d'Herculais and C. Langlois.—On the supposed post-Secondary granites of Ariège, by M. A. Lacroix.—On the age of a porphyritic granite from the Western Pyrenees, by M. Joseph Roussel.—Experimental researches on muscular exertions, by M. Charles Henry.—Diseases of the bones of chimpanzees, gorillas, and orang-outangs, by M. Etienne Rollet.

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THURSDAY, JULY 9, 1891.

THE UNITED STATES ENTOMOLOGICAL COMMISSION.

Fifth Report of the United States Entomological Commission on Insects Injurious to Forest and Shade Trees. By Alpheus S. Packard, M.D., Ph.D.

VERY valuable Reports have been presented by the United States Entomological Commission from time to time. Among these may be mentioned that upon "The Rocky Mountain Locust," prepared by Prof. Riley in 1878, which is a most exhaustive record of the habits of this terrible pest, and of methods of prevention and remedies against its attacks. Later on, an equally valuable and instructive Report was submitted with regard to the cotton worm (*Aletia argillacea*), very destructive to the cotton plant, whose crop it has reduced in some seasons from 30 to 75 per cent. in the principal cotton-producing States. Both these elaborate works, as might be expected from their authors, Prof. Riley and Dr. Packard, who practically constituted this Entomological Commission, are full of interesting experiments, ingenious contrivances, and subtle devices, to circumvent the insect hordes advancing with the insistance of invading armies.

This Report upon "Insects Injurious to Forest and Shade Trees" is perhaps not so exciting or painfully interesting, as the harm caused to trees is not so directly felt as that occasioned to various food crops and other crops of the field by locusts and caterpillars innumerable, and the name of the insects described therein is legion, and their individual mischief is comparatively small.

As Dr. Packard says, "a volume could be written on the insects living on any single kind of tree, and hereafter it may be expected that the insect population of the oak, elm, poplar, pine, and other trees will be treated of monographically." Kaltenbach, in "Die Pflanzenfeinde, aus der Klasse der Insekten," gives accounts of 537 European species of insects injurious to the oak, 107 to the elm, and 396 to the willow. Perris, a French observer, has recorded no less than 100 species of insects found upon the maritime pine.

The attacks of insects upon forest trees and upon shade trees, or trees planted for shade and ornamentation in parks, streets, and other public places, are becoming far more numerous and serious, just as in the case of all cultivated crops under the sun. In the United States these attacks are creating intense interest, as the forests are of the highest commercial importance, and have been extensively decreased by clearing, by wanton and accidental fires, and other causes. This Report, then, is opportune, and must be of great service, as it demonstrates the sources of the injuries, and suggests means of preventing them or of diminishing them.

The French, German, Austrian, and Italian Departments of Agriculture are giving much attention to this subject, for it is found that the forest trees of these countries are becoming more liable to harm from insects. In Great Britain some kinds of trees, notably of the pine tribe, have suffered much damage from insects hitherto unknown, or, at least, not reckoned as injurious. There are, without doubt, many others unsuspected in British woods and forests, slowly but surely working great mischief.

Dr. Packard shows that trees are attacked in every part and in every conceivable manner by insects. Their roots, leaves, bark, fruit, and twigs are all more or less subject to their visitations. The most curious of those which affect the roots is the "seventeen year" Cicada, whose larvæ remain for over sixteen years attached to the rootlets of the oak, other forest trees, and fruit trees, as the pear and apple. According to Prof. Riley, these larvæ are found at a great depth, sometimes as much as 10 feet below the surface. The female, resembling a locust, deposits long slender eggs in an unbroken line upon the terminal twigs of oak and other trees in May and June. Sometimes the twigs are so "badly stung" by this oviposition that the trees are seriously injured. The length of wood perforated on each branch sometimes varies from one to two and a half feet, averaging probably eighteen inches, and appearing to be the work of one female. From the eggs the larvæ hatch out in six weeks and drop to the ground, in which they live, sucking the roots of the trees for nearly seventeen years, the pupa state lasting but a few days.

A formidable enemy of the "live-oak" (*Quercus virens*) is an enormous beetle, *Mallodon melanopus*, Linn., whose larva, three inches long and an inch in thickness, bores into the roots upon which it lives. As a result of the work of this insect in South Georgia and Florida, "vast tracts, which might otherwise have become forests, enriching the ground with annual deposits of leaves, are reduced to comparatively barren scrub, in which the scattered oak-bushes barely suffice to cover the surface of the sand." The eggs are laid by the beetle in the foot, or collar, of the tree, just below the surface of the ground. It is not known how long the larvæ live, but their life must extend over several years, "since the roots occupied by them grow to a large size, while they show an abnormal development, and become a tangle of vegetable knots. In fact, the entire root in its growth accommodates itself to the requirements of the borer within." The effect on the tree is to kill the original stem, which becomes replaced by a cluster of insignificant and straggling suckers, forming, perhaps, a clump of brushwood.

Among the tree-borers, other than beetles, the oak "carpenter worm," the caterpillar of *Prionoxystus robiniæ*, Peck, is the largest and most destructive. It is larger and far more abundant than the European caterpillar of *Cossus ligniperda*, or goat-moth, belonging to the same family of Cossidæ, but it sinks its tunnels deep in towards the heart of the tree, not confining its mischief to the limbs and large branches like the goat-moth caterpillar. Fitch says of this:—"Of all the wood-boring insects in our land, this is by far the most pernicious, wounding the trees most cruelly. The stateliest oaks in our forests are ruined, probably in every instance where one of these borers obtains a lodgment in their trunks." Another species of Cossus, known as *Cossus centerensis*, bores into poplars. Its appearance and habits also resemble those of the goat-moth, well known in this country.

There are numbers of boring beetles, of the families Buprestidæ, Cerambycidæ, and Scolytidæ, whose larvæ make burrows, passages, and galleries in trees, mainly just under the bark. Of these, the elm-tree borer, *Saperda tridentata*, is prominent, often killing elm-trees by wholesale, both in forests and in public parks. The larvæ bore in the inner bark, making irregular furrows and tunnels upon the surface of the wood, which "is, as it were, tattooed with sinuous grooves, and the tree completely girdled by them in some places." In the State of Illinois attention was attracted to the gradual decay and death of white elms (*Ulmus americanus*) in rows in some towns. The leaves fell off in the summer, and some of the branches died. Finally, the tree perished altogether. On peeling off the bark, half-grown larvæ of *Saperda tridentata* appeared in considerable numbers, and the manner in which the bark had been mined by the Saperdas gave sufficient evidence of the cause of the death of the tree. Prof. Forbes, State Entomologist of Illinois, says: "From the present appearance of the elms throughout the towns of Central Illinois, it seems extremely likely that this pest will totally exterminate this tree, unless it be promptly arrested by general action." It is recommended that all affected trees should be removed and destroyed in autumn and winter, before the beetles have a chance to emerge from the trunks. This beetle is not quite an inch long; its larva is rather more than an inch in length, having a large flat head.

Fir trees, especially the white pine (*Pinus strobus*), the yellow pine (*Pinus mitis*), and *Pinus rigida*, are much injured by the pine borer or "sawyer," *Monohammus confusor*. "I have seen," writes Dr. Packard, "hundreds, perhaps nearly a thousand, dead firs, whose trunks were riddled with the holes of these borers." Dr. Packard cites a correspondent of the *North-Western Lumberman* who reported that "extensive and valuable forests of yellow pine in the Southern States are destroyed by a worm commonly called here a 'sawyer,' or flat head." White pine trees are also much beset by the "wood engraver" bark beetle (*Xyleborus xylographus*, Fitch), so called because it makes beautifully regular and artistic furrows on the surface of the wood under the bark. It is the most common, and probably the most pernicious, of all the insects that infest the forests of white pine in New York State, and of yellow pine in the States south of New York.

A weevil, the white pine weevil (*Pissodes strobi*), frequently spoils the finest white pines in parts of America by placing numerous eggs in the bark of the topmost shoots of fir trees; the larvæ from these make mines in the wood and pith, causing the shoots to wither and die, thereby occasioning a fork, or crook, at this point. This is a very small insect, not three-quarters of an inch long, and the larvæ are not half an inch in length.

There is a mighty army of caterpillars of various moths described in this Report, which devour the foliage of trees of all kinds in American forests and gardens. Several species of Clisiocampa and Gasteropacha, of the Bombycidæ, assail oak, willows, ash, chestnut, apple, and pear trees. These are termed "tent" caterpillars, as they live in webs of a tent-like form, as the *Clisiocampa Neustria*, or lackey moth, in Europe. But the most voracious of caterpillars are the "fall web worms" of the moth *Hyphantria cunea*, Drury. For instance, in 1886, the city of Washington, as well as its vicinity, was entirely overrun by them. All vegetation, except that not agreeable to their tastes, suffered greatly. Fine rows of shade trees, which grace the streets and avenues, were leafless in midsummer, and covered with hairy worms. The pavements were strewn with moultings of the caterpillars and their webs, which were blown about unpleasantly by the wind.

Because they are hairy they have comparatively few enemies, among birds at all events. The "English sparrow," fast becoming as great a nuisance in the United States as the rabbit in Australasia, will not look at them, and has driven away by its pugnacity many birds that would eat them. Fortunately there are insect enemies which prey upon them, as the *Mantis carolina*, or "rear horse," an extraordinary insect of the same family as the "praying" mantis, and the "wheel bug" (*Prionidus cristatus*). Several parasitic insects also greatly check the spread of this moth. One fly, *Telenomus bifidus*, Riley, lays its egg within the tiny egg of the moth, in which all the transformations of the fly take place, and its food and lodging are found. In due time, having cleared out the egg, the fly emerges.

Mr. Bates, in his graphic account of tropical insects, has pictured many that are made to closely resemble their surroundings, for their preservation and other purposes. In his well-known paper on mimicry, he alludes to the insects known as Phasmidæ, or "spectre" insects, as especially typical of this adaptation to circumstances, preserved and augmented, as Darwin says, "through ordinary selection for the sake of protection." Mr. Wallace brings forward the Phasmidæ as striking instances of mimicry, remarking that "it is often the females alone that so strikingly resemble leaves, while the males show only a rude approximation."

Species of this family of Phasmidæ are mischievous to trees in America, principally the oak and the hickory. The chief of these is the *Diapheromera femorata*, Say., popularly called "walking-stick," "walking-leaves," "stick-bug," "spectre," "prairie alligator," "devil's horse." This insect, especially the female, is so like the twigs of trees in colour and appearance, that it is difficult to discover it. It has a habit, too, of stretching out the front legs and feelers, greatly enhancing this resemblance. While the vegetation is green the "walking-sticks" are green; when the foliage changes in the autumn they also change colour; and when the trees are bare of leaves they closely resemble the twigs on which they rest. The eggs are dropped upon the ground from whatever height the females may be, "and, during the latter part of autumn, where the insects are common, one hears a constant pattering, not unlike drops of rain, that results from the abundant dropping of these eggs, which in places lie so thick among and under the dead leaves that they may be scraped up in great quantities." Prof. Riley adds, with regard to these singular creatures and their wonderful resemblance to the oak vegetation upon which they occur, "one cannot help noticing still further resemblances. They are born with the bursting of the buds in the spring; they drop their eggs as the trees drop their seeds, and they commence to fall and perish with the leaves, the later ones persisting, like the last leaves, till the frost cuts them off."

There is not space enough to do more than allude to the sawflies, another class of insects fearfully injurious to trees of divers kinds. Many of these Hymenoptera, as in Great Britain and other European countries, mainly of the genus *Nematus*, clear off the leaves of forest and fruit trees. Others attack firs, notably some species of Lophyrus and Lyda, as the *Lophyrus abietis*, *Lophyrus pinetum*, and *Lophyrus pini-rigidæ*, and some of the Lyda. Cameron, in his monograph of the British phytophagous Hymenoptera, states that there are fifteen species of Lophyrus in North America, and that the species of Lyda are common there.

Lophyrus abietis and *Lophyrus abbotii* appear to do the same harm in America to firs as the *Lophyrus pini* in Scottish fir plantations, whose larvæ not only eat the leaves but the bark of the young shoots, frequently occasioning great losses.

An instructive account is given in this work of the effect of temperature upon insects. It is the fashion in Great Britain to say that insects are killed by hard frosts. But they are not killed in countries—as America, for example—whose winters are far more severe. Dr. Packard, quoting Judeich and Naitsche's "Lehrbuch der Mittel-Europäischen Forstinsektenkunde," observes that "the influence of even very great cold on the normal hybernating stages of our insects is not very great. In the summer of 1854 the 'nun' moth had very generally laid its eggs in Eastern Prussia uncovered on the bark, and these did not freeze in the hard winter of 1854–55. According to the observations of Regener, openly exposed caterpillars of the pine silk-worm endured 10° F. The pupa froze at 21° F., the moth at 19° F. According to Duclaux, the eggs of the silk-worm endure well, remaining two months in a temperature of 17° F. Great fluctuations of temperature during the winter produce an abnormal interruption of the winter's rest or hibernation, and thus cause the death of many insects." It will be noticed that in all these cases the insects were unprotected, whereas there is generally some kind of protection during the winter for insects in all stages, provided by their instinct.

Not the least useful part of the Report is that treating of remedies for insect attacks, and machines and engines for applying them. Arsenical poisons, known as Paris Green and London Purple, are strongly recommended for spraying or syringing trees infested with the larvæ of beetles and sawflies, or the caterpillars of moths. These have been recently introduced into England, being advocated by the Board of Agriculture, but have not been extensively adopted yet, owing to the natural prejudice against the use of poisons. In America they are employed most extensively and with the greatest benefit. By means of these the potato beetle (*Doryphora decemlineata*) was circumvented, and the cotton and boll worms checked, and the onslaughts of many other insects materially lessened. For Aphides, Scale insects, and other insects which suck the sap of leaves, "emulsions" or washes of soft soap, or "jelly soaps," made directly from fish oil and concentrated lye, or whale-oil soap, are prescribed. Also kerosine, naphtha, and petroleum, applied in a fine spray, or mixed with soap and soap jelly, forming "emulsions." These remedies act by contact, being applied principally to insects which do not eat the leaves, as well as by making the surroundings unpleasant and unbearable. Powdered substances, as pyrethrum, hellebore, and sulphur, are not much employed for forest work, but cases frequently arise warranting their use in a limited way. Hellebore, as gooseberry growers in Kent and Cambridgeshire well know, is of especial value against all sawfly larvæ. Sulphur is valuable against the red spider (*Tetranychus telarius*), and is used alone or in connection with emulsions of kerosene.

Numerous machines are in vogue for putting on washes and powders, from the small "knapsack" machine carried on the back, to huge tanks on wheels, fitted with powerful hand-pumps and long lengths of hose, through which liquids are forced to great heights; for very high trees, tall ladders are used, which are set near the trees, upon which men mount, and direct the hose into the topmost branches. For smaller trees and shrubs, a barrel fixed on wheels, having a good force-pump with hose, is adopted. Pumps are also fitted into tanks of all shapes and sizes, and moved from place to place by hand or horse-power. To distribute the liquids there are endless nozzles or jets contrived with much ingenuity to send forth fine mists, or sprays, or continuous volumes. It will suffice to say that the best of these is the cyclone, or Riley nozzle, which is just being introduced into Great Britain.

Foresters, and all concerned in the management of woods and forests, public parks, and gardens, would do well to consult this work for information as to the various insect enemies of trees, and the best means of dealing with them. It is quite impossible in a review to give anything more than a general idea of its scope and nature.

PHYSICAL RELIGION.

Physical Religion. The Gifford Lectures delivered before the University of Glasgow in 1890. By F. Max Müller. (London: Longmans, 1891.)

THE present volume, which embodies the author's second course of Gifford Lectures, with notes and appendices, is devoted to the consideration of "Physical Religion," that is the religion which finds its object the Infinite in or behind the phenomena of Nature. The author's previous writings have made it clear that for the simplest and most abundant manifestation of this form of religion we must go to the Veda, so his first task in the lectures before us is to tell once more the familiar story of the discovery, the character, and the age of the Veda. To this survey four lectures are devoted, and, in conclusion, the author—not without duly considering all that in recent years has been urged to the contrary—reaffirms his conviction that the hymns of the Rig Veda cannot have been collected later than 1000 B.C.

In the sixth lecture the author deals with the evolution of the idea of God. It is often supposed—even by philosophers of repute—to be a sufficient account of the earliest form of religion to say that men worshipped stones and other fetishes as their gods. But, as the professor well remarks—

> "Does it never strike these theorizers that the whole secret of the origin of religion lies in that predicate, *their gods?* Where did the human mind find that concept and that name? That is the problem which has to be solved; everything else is mere child's play."

And he exhibits the process by which Agni (the Vedic god of fire), from being originally nothing but "the mover," came to be called *deva;* and it is this word *deva* which when examined yields the clue to the development, and teaches us a lesson of the highest importance:—

> "Guided by language we can see as clearly as possible how, in the case of *deva*, the idea of God grew out of the idea of light, of active light, of an awakening, shining, illuminating, and warming light. We are apt to despise the decayed seed when the majestic oak stands before our eyes, and it may cause a certain dismay in the hearts of some philosophers that the voice of God should first have spoken to man from out the fire. Still, as there is no break between *deva*, bright, as applied to Agni, the fire, and many other powers of nature, and the *Deus optimus maximus* of the Romans—nay, as the God whom the Greeks ignorantly worshipped was the same God whom St. Paul declared unto them—we must learn the lesson, and a most valuable lesson it will turn out to be, that the idea of God is the result of an unbroken historical evolution, call it a development, an unveiling, or a purification, but not of a sudden revelation."

The two following lectures are devoted to the detailed following out of the biography of Agni, who appears in a variety of characters as the sun, the fire on the hearth, lightning, the messenger between gods and men, and priest. Finally, divested of his material character altogether, he is raised to a sublimer level as creator, ruler, and judge. The value of this inquiry, into the details of which we have no space to enter, lies in the fact that it involves the refutation of two objections which are frequently urged—with or without knowledge—against natural religion by the professors of so-called supernatural religion. The first is that natural religion, though it may lead men to a conception of "gods," is powerless to suggest to them the conception of God. This is directly contradicted by the history of Agni, whom we can watch, as it were, passing through many stages of growth until he becomes in the end "a supreme god, *the* Supreme God, till his very name is thrown away, or is recognized as but one out of many names by which ancient seers in their helpless language called that which is, the One and All." Driven from this position, however, the orthodox objector usually takes up another, and contends that the supreme God of natural religion lacks some if not all of the lofty attributes which he is enabled to know and to predicate of his own God by supernatural revelation. But Prof. Max Müller's answer to this objection is equally decided:—

> "Trusting to the fragments that have been preserved to us in the Veda, to the remains of the most childish as well as the most exalted thoughts, we may say that natural religion, or the natural faculties of man under the dominion of the natural impressions of the world around us, can lead, nay, has led man step by step to the highest conception of deity, a conception that can hardly be surpassed by any of those well-known definitions of deity which so-called supernatural religions have hitherto claimed as their exclusive property."

In the ninth lecture the Professor leaves for a while the field of his special studies to glance at the history of religious ideas among other peoples than the Âryas of the Veda. And it is noteworthy that he fully recognizes the possibility that Jehovah himself may originally have been a god of fire. But we must protest against the way in which he alludes to Abraham, the legendary founder of Hebrew monotheism, as if his historical character had never been questioned. It is, of course, perfectly open to any one to believe that Abraham was a real individual, who received a "revelation," whatever that word may be defined to mean (see p. 221); but at the same time, in a course of lectures addressed to an academic audience, it should surely have been mentioned that this is an hypothesis, which Renan, for instance, among Semitic scholars, does not even take the trouble to discuss.

In the lecture on the mythological development of Agni, we would call attention to the importance assigned to *riddles* as a cause of the growth and preservation of mythology. To take a simple example:—

> "After the Âryas in India had once arrived at the conception that fire was apt to consume the fire-sticks, or that Agni had eaten his father and mother, they seem to have amused themselves by asking such questions as, Who eats his own parents? The answers given would then enter upon many details, more or less far fetched, and the question would continue to be asked between young and old people."

And we think that this is a far more natural explanation of the origin and popularity of such stories than the hypothesis, which has no external evidence to support it, that the Âryas were simply ascribing to Agni the atrocities which they practised themselves.

Finally we come to the question, What can a study of natural religion teach us? "Why," answers Prof. Max Müller, "it teaches us that religion is natural, is real, is inevitable, is universal," and he proceeds to exhibit in detail one or two of the more important implications of this great lesson. With regard to miracles, for instance:—

> "Is it not clear that in the eyes of those who believe in the omnipresence of the Moral Governor of the world, miracles, in the ordinary sense of the word, have become impossible, and that to them either every event is miraculous or no event can claim that name. Before the great miracle of the manifestation of God in nature, all other miracles vanish. There is but one eternal miracle, the revelation of the Infinite in the finite."

The Professor then shows by a series of examples that the tendency to ascribe a miraculous birth to the founders of religions is natural and widespread, and asks by what right people claim a different character for the legends of the birth of Jesus than for the similar legends told of Buddha and Mohammed. The honesty and candour with which the question is stated are specially welcome at the present time, when it is becoming the fashion with ecclesiastical amateurs in Biblical criticism to blow hot and cold, as it were, with the same infallible mouth—that is, to reject the miracles of the Old Testament, but retain those of the New. For instance, in a recent manifesto, highly recommended as providing a temporary shelter for the destitution of the semi-reasonable, there is, on the one hand, some tall talk about the imaginative performances of "a dramatizing Jew" in the Old Testament, while, on the other hand, we are gravely informed that "the Church can insist upon the truth" of all that is recorded in the New Testament. That this cheap substitute for criticism will eventually be discredited, even in England, we have no doubt whatever. Meanwhile we cordially recommend the present volume not only for the interest of its subject-matter, but as an example of the masterly application of the only method which in these inquiries can lead to sure results.

THE KARWENDEL ALPS.

Das Karwendelgebirge. Von A. Rothpletz. Separat-Abdruck aus der *Zeitschrift des Deutschen und Oesterreichischen Alpenvereins.* With Map. (München, 1888.)

THE Karwendel Alps are a mountain mass lying to the north of the valley of the Inn, between Innsbruck and Jenbach, and bounded on the east by the Achensee, on the north and west by the upper valley of the Isar, and on the south roughly by a line drawn along the Hinterauthal (the highest part of the valley of that river) to Schwaz, in the Innthal. This region has been explored and mapped by Herr Rothpletz, with the assistance of other workers, and it is described as consisting of three roughly parallel ranges. Though their peaks do not attain to a very great elevation, the higher summits ranging from 6500 feet to rather over 8200 feet, their grand cliffs of cream-coloured limestone and their pine-clad slopes afford very beautiful scenery.

In this part of the Alps the mountain masses are wholly composed of sedimentary deposits which range from the Trias to the Neocomian. The oldest are the *Werfener Schichten*, a mass of sandy shales and sandstones, often containing numerous flakes of biotite, indicative, in all probability, of the denudation of the crystalline masses which form the floor of the Mesozoic rocks in the Alpine region. They correspond in age roughly with the upper part of the Bunter in Germany and England. Then comes the remainder of the Trias, including the Muschelkalk, followed by the representatives of the Rhætic, the Lias, and other Jurassic deposits, and a part of the Neocomian, a marine series from top to bottom. Neither the last nor the Jurassic system attains to a great thickness, but both the Rhætic and the Trias are represented by great masses of rock. In the one, the *Haupt-dolomit* occasionally attains to a thickness of 500 metres; in the other, one member, the *Myophorienschichten*, is said to be equally important. Careful descriptions of each subdivision, with lists of the more characteristic fossils, are given in the memoir. Neither Cretaceous nor Tertiary strata occur to bridge over the interval between the Neocomian and the superficial Glacial or post-Glacial deposits.

The physical history of these ranges is made the subject of an elaborate discussion. Herr Rothpletz is of opinion that, at some epoch after the Neocomian and before the commencement of the folding process by which the existing Alpine ranges were upraised, the region was affected by movements which produced a system of faults. In consequence of these, a zone of upheaval was bordered on either side by one of depression. These caused important modifications in the great east and west folds, to which the Eastern Alps are due; the rocks in the two troughs were crushed together; the upheaved tracts were upthrust. A folding plate represents an ideal section of the region after the "pre-Alpine" movements, side by side with one which shows its present state.

There can be no doubt that, in explaining the physical structure of the Alps, we have to take account of much more than the later Tertiary foldings to which the formation of the mountain-chain is due, such as the old irregularities of the pre-Mesozoic land-surface; and any important system of faults could not fail to produce very marked effects. Also, it seems indubitable that there were interruptions to the downward movement in parts of the Alpine area during the later Mesozoic and the earlier Tertiary times, which may, very probably, have caused faults such as are described by Herr Rothpletz. These, it may be noticed, appear to run obliquely to the general trend of the main folds.

Herr Rothpletz, in conclusion, expresses an opinion adverse to those geologists who consider that glaciers have played an important part in the erosion of valleys, and calls especial attention to the Soiernsee, a small lake lying in a fold of the *Plattenkalk*, which, in his opinion, indicates that "the movement of flexure acted in this case with greater rapidity than the erosive action of streams or glacier."

The geological map is on a scale of 1 : 50,000; the separate memoir, of octavo size, contains 76 pages, with 9 plates and 29 smaller illustrations. It also includes a full list of works bearing on the district. So far as we can judge, it is an elaborate and valuable contribution to the knowledge of a region but little known to English travellers, who, however, occasionally pass very near to it along the margin of the beautiful Achensee.

T. G. B.

OUR BOOK SHELF.

Graphical Statics. Two Treatises on the Graphical Calculus and Reciprocal Figures in Graphical Statics. By Luigi Cremona. Translated by Thomas Hudson Beare, Professor of Engineering and Applied Mechanics, Heriot-Watt College, Edinburgh. (Oxford: Clarendon Press, 1890.)

TREATISES on this and allied subjects of the Graphical Calculus are not uncommon in our language; but, although nowadays indispensable for engineering purposes, the subject does not flourish in our theoretical courses of instruction.

The theorems of Graphics once stated—that is, drawn out carefully on the drawing-board—are obvious, or at least do not lend themselves to verbal written demonstration, so that for purposes of competitive examination, the controlling influence of modern education, the subject of Graphical Statics and Calculation is useless.

Geometrical drawing is not taught in our public schools and Universities; and the student in a technical college only requires the bare minimum of Graphics, sufficient to enable him to pass on to practical developments; so that we fear the elegant abstract theorems on the use of signs in Geometry, as applied to lines and areas, graphical multiplication, division, involution and evolution, solution of equations, centroids, rectification and graphical analysis generally, will receive but slight attention.

There is a note of defiance in the Author's Preface to the English edition of "Reciprocal Figures in Graphical Statics" (the second treatise): "At a time when it was the general opinion that problems in engineering could be solved by mathematical analysis only, Culmann's genius suddenly created Graphical Statics, and revealed how many applications graphical methods and the theories of modern (projective) geometry possessed," &c.

The preface to "Geometry of Position," by R. H. Graham, must be consulted for the counterblast in favour of Maxwell's claim to the honour of priority.

A. G. G.

The History of Commerce in Europe. By H. de B. Gibbins. With Maps. (London: Macmillan and Co., 1891.)

THE chief defect of this little book is that the author does not bring into sufficient prominence the geographical element in commercial history. What are the geographical conditions which have favoured the growth of particular industries in special localities? And in what ways have such conditions affected the interchange of commodities between one part of the world and another? Mr. Gibbins has not, of course, neglected these questions, but he scarcely seems to have realized that they are of vital importance for the scientific presentation of his subject. On the other hand, his appreciation of the action of historical causes in the development of commerce is excellent; and for a general view of commercial progress his manual will be of much service to students. After an introductory chapter he considers "ancient commerce," by which he means the commerce of the Phœnicians, the Carthaginians, and the Greek colonies. He then deals with the ancient Greek States and Rome as trading communities. Next comes "mediæval commerce," in connection with which he has much that is interesting to say about the Italian cities, the Hansa towns, mediæval trade routes and fairs, the manufacturing centres of Europe, and other topics. Under "modern commerce" he treats of the commercial empires in the East, the commercial empires in the West, English commerce from the sixteenth to the eighteenth century, European commerce in the seventeenth and eighteenth centuries, the industrial revolution in England and the Continental wars (1793), modern English commerce, and the development of commerce in France, Germany, Holland, Russia, and the other European States. The maps are very good, and add considerably to the value of the text. We may also note that the volume includes a useful series of questions on the various chapters, and two appendices, in one of which there is a list of British produce and manufactures in 1840 and 1889, while the other consists of a table showing the present colonial empires of European Powers.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Albert University.

THE remarks of Mr. Thiselton Dyer upon the draft charter of the "Albert University" have my fullest concurrence. I have never desired to see such a University as is sketched in that charter set up in London by the side of the existing University. The charter and the general scheme of its proposals never obtained the sanction of the professoriate of University College whilst I was a member of that body; and many of us were as active as circumstances allowed us to be, in opposing its federal principles and bureaucratic tendency. That University and King's Colleges should be united in some way to form a University is one proposition: that the University should take the particular form excogitated by Sir George Young is another. It is well that it should be generally known that the elaborate (and to my mind mischievous) constitution sketched in the draft charter of the Albert University is the product of the devotion and ingenuity of Sir George Young, an active member of the Council of University College.

I was not aware, when I wrote in NATURE some weeks ago on this subject, that the Lord President of the Privy Council had determined to set aside the recommendations of the late Royal Commission, and to hurry through a formal inquiry into the draft charter propounded by the Councils of University and King's Colleges.

So long as the matter was in the hands of the Commission, this charter, put forward by the Councils of the two Colleges, was merely one of many suggestions as to the proper form which a new or reconstituted University of London should take. It was notorious that the Councils' support of Sir George Young's scheme did not represent the attitude either of the Professors of the two Colleges or of those throughout the country who have special knowledge of Universities and of the best methods of academical organization.

The Royal Commission of 1888 was appointed to inquire "whether any and what kind of new University or powers is or are required for the advancement of higher education in London." The Commission took a large amount of evidence from interested parties—practically none from persons outside the London institutions concerned—and recommended that the University of London should be invited to meet the needs set forth in such documents as the draft charter of the Albert University, by some modifications of its constitution and procedure. In the event of a failure on the part of the University to do this, the Commissioners recommended *that the matter should be referred back to them.*

My support of the claim of University and King's Colleges to be incorporated as some kind of University has always depended on the assumption that no Commission or other serious authority could possibly accede blindly, and without full consultation of the best authorities in the land, to the scheme embodied in the Albert University draft charter. The Commissioners took, it seems to me, the only rational view of that charter—namely, that it might serve as a suggestion to the University in Burlington Gardens for a reform which would meet, at any rate, some of the objections raised to the existing constitution of the latter body.

Lord Cranbrook, however, seems anxious to hurry on the shelving if not the solution of the University of London question. Instead of referring the matter back to the Commissioners, he takes the matter out of their hands. The Commissioners have *never reported in answer to the question set before them.* No one knows whether they think any, and, if so, what kind of new University is required in London.

Having failed to settle the question for the time being by such a reform of the University in Burlington Gardens as Mr. Dyer advocates, the Commissioners ought—according to their own recommendation—to have been allowed to proceed further. "It is now ascertained," they would have said, "that the existing University of London will not reform itself in the way we have suggested: what sort of University shall we now recommend, if any?" They might have suggested the coercion of the Convocation of Burlington Gardens by an Act of Parliament; or they might have—after inquiring from authorities in Oxford, Cambridge, Dublin, Edinburgh, and wherever else some understanding of the nature and objects of Universities happens by chance to dwell—recommended the formation of a professorial University in London similar to those of Scotland and of Germany.

I confess that it has always been my hope, though not my expectation, that they would take the latter course. I am sure that if they had proceeded to take the evidence of experts in University matters, and had not attached undue importance to the proposals of competing corporations, they would have found the balance of unprejudiced opinion to be in favour of a "professorial" rather than a "federal" University. The difficulty they would have had to contend with would have been that some of their own body, and nearly every witness whom they lately examined, are very far from having a clear idea as to what are the possible forms of University organization, what the merits and the demerits respectively of the "federal" and the "professorial" scheme as now in practice in Europe. This is obvious enough from the printed "Minutes of Evidence taken before the Royal Commissioners appointed to &c.," which can be purchased of Messrs. Eyre and Spottiswoode for about half-a-crown.

But whatever else the late Royal Commission might have done, I cannot believe that they would have proposed to set up so extraordinary and useless a piece of complicated machinery as the Albert University (of the draft charter) by the side of Burlington Gardens. The draft charter, having failed to reform the existing University of London, ought, one would have thought, to have been torn up.

I quite agree with Mr. Dyer that it is little short of monstrous for the Government to set up in London *two* such organizations as Burlington Gardens and the federal Albert; there is the strongest reason for insisting that there shall be only one of them, whether Convocation likes it or not.

Meanwhile, we are no nearer than we were seven years ago to the formation in London of a Senatus Academicus which shall retain in the metropolis—in contact with its statesmen, lawyers, physicians, authors, and the intelligent men and women of wealth and leisure—the strongest and best of our scholars, historians, physicists, and biologists. Is it well that the President of the Royal Society of London should have to travel from Glasgow to the meetings of that body? that its senior Secretary should spend his life in Cambridge? and that there is absolutely no professorship in the metropolitan area which can, by virtue of its dignity or its pecuniary value, entice men from the seclusion of provincial Universities? The draft charter of the Albert University does not even attempt to supply such a want. It actually makes the London professor more a creature of competition and the servant of red-tape officialism than he is at this moment.

E. RAY LANKESTER.

Mr. THISELTON DYER has done good service in pointing out the nature of the proposed Albert University, which, unfortunately, seems not unlikely to be the result of the discussions that have been going on for the last six or eight years with respect to a "Teaching University for London." Should the charter petitioned for by the Councils of University and King's Colleges be granted, it will not constitute a teaching University in any real sense, but, as Mr. Thiselton Dyer says, an institution very similar to what the present University of London was as constituted by the original charter of 1837. There are, of course, differences of organization and machinery, such as the institution of Assemblies of Faculties and Boards of Studies (which the existing University might institute next week, if it saw fit), but there is little or nothing that can be looked upon as a difference of principle. The nearest approach to this are the provisions: (1) that the Colleges whose students are to be eligible as candidates for degrees shall have a certain amount of representation on the governing body of the University; (2) that the claim of additional Colleges to enter the University shall be decided by the governing body of the University, subject to appeal to the Queen in Council (instead of, as in the charter of 1837, being decided on directly by the Crown); (3) that "the University may appoint lecturers independently of a College or medical school to give instruction in any subject, whether it be or be not included in a Faculty."

With the exception of this last provision, slipped in at the end of Section V., "*University Degrees and Certificates*," as though modestly shunning the notice that a separate heading might call to it, there is no allusion from beginning to end of the draft charter to any teaching to be done by or through the University as such. If it comes into existence, it will be a mere examining University over again. Such a scheme can go no appreciable way towards remedying the existing defects of University organization in London. It is not easy to see what public advantages are likely to result from it. Seeing that it is put forward as representing the views of University College, London, it does not seem irrelevant to the present stage of the discussion to say that the scheme of the Albert University has never been submitted to a general meeting of the Governors of the College.

University College, London. G. CAREY FOSTER.

The Draper Catalogue.

ON p. 133 of the current volume of NATURE (June 11) Mr. Espin gives a comparison of the Draper Catalogue of Stellar Spectra with the catalogues of Vogel and Dunér. Vol. xxvi. of the *Harvard Annals*, of which the first part will be distributed in a few days, discusses at length the deviations from Vogel and also from the similar catalogue of Konkoly. A second examination was made on photographic plates having a long exposure of those stars which appeared discordant. Since spectra of the first type pass by insensible degrees into the second, and these in turn into the third, no two observers would agree on the exact points of distinction. Moreover, different characteristics would distinguish the photographic and visual portions of the spectra (*H. C. Annals*, xxvi. pp. 177, 189). Some discrepancies, as in the case of the three fourth-type stars which are erroneously entered in the Draper Catalogue, are due to errors of identification (xxvi. p. 192). The photographic spectra of faint third-type stars are always indistinguishable from those of the second type (xxvi. p. 178). See also remarks following Table II. of vol. xxvii. The bright lines cited by Mr. Espin are probably portions of the spectra contained between dark bands or lines (xxvii. p. 3). Spectra are difficult to classify when measured as faint as 6·5; not when the final magnitude is brighter than 6·5, as might be inferred from Mr. Espin's reference (xxvii., preface).

EDWARD C. PICKERING.

Cambridge, U.S., June 22.

The Cuckoo.

I DO not know if the hibernating of swallows and other summer visitors is still a debated question or not, but the following account of a cuckoo may be of interest to some of your readers.

In the month of August a young cuckoo was taken from its nest and kept in the house, where it lived and throve—until one day in November, when it escaped and could not be found. But in the following March, during the usual spring cleaning, this very bird was discovered on a shelf in the back kitchen, hidden away behind some old pots and pans, still alive, and asleep, with all its feathers off, and clothed only in down, the feathers lying in a heap round the body. The rude awakening which the cuckoo received was fatal to its existence, for it survived only for a few hours.

E. W. P.

Colour-Associations with Numerals, &c.

THE following record of experiments extending over a period of nearly ten years, under exceptionally good conditions, appears to me to be worthy of attention. A preliminary note on the subject was printed in *Science*, vol. vi. No. 137, 1885, p. 242, part of which is reproduced below.

In 1880, while I was in Washington, I read Mr. F. Galton's note on "Visualized Numerals," in NATURE, vol. xxi. p. 252.

After I came to Wisconsin—probably late in 1881, or early in 1882—I mentioned my own entire inability to visualize numerals or anything else of the kind to a member of the University faculty, Prof. Owen. I was interested to learn that, when a boy, he had always conceived the vowel sounds as having colour, and that he still retained some traces of this early habit.

I spoke of this subject in my house shortly after; and my daughter Mildred, then about seven years old, said she also had colours for the days of the week, as follows: Monday, *blue;* Tuesday, *pink;* Wednesday, *brown* or *grey;* Thursday, *brown* or *grey;* Friday, *white;* Saturday, *pure white;* Sunday, *black.* It was said laughingly, and at the time it passed to my mind as a joke—that she wished in sport to assume the idiosyncrasies of elder persons. A few days after, I questioned her on these colours, and she gave the same replies. It was again spoken of as a kind of a joke and a question of memory, but I wrote the colours down in my memorandum-book for 1882. A year later I produced this, and again questioned her—this time seriously—and found her answers the same as at first. Again, on August 5, 1885, her replies were the same. The tenacity of a child's memory is very remarkable; but I was convinced this was not a case of memory and imagination, but a true phenomenon of the kind referred to. I therefore went farther, and asked her if there were any other phenomena of the same sort (she was now ten and a half years old). I found that each of the letters of the alphabet had a colour to her, as follows:—

A, *white;* B, *blue;* C, *yellow, cream colour;* D, *dark blue;* E, *red;* F, *black;* G, *green;* H, *white;* I, *black;* J, *grey, brown;* K, *grey;* L, *dark blue;* M, N, *brown,* not much colour; O, *yellow;* P, *green;* Q (?); R, *brown;* S, *yellow;* T, *green;* U, *yellow;* V, *white;* W, *brown;* X, Y, not much colour; Z, *greenish.*

The prevalence of yellow and green, and the scarcity of reds and pinks, are noteworthy. I found that she knew these colours instantly, and when I asked for them in any order. What is more remarkable, she could instantly name the brown letters in a group, the black ones, &c. Apparently she did not require to pass the alphabet in review to decide this. The numbers also had colours to her, as follows:—

1, *black;* 2, *cream colour;* 3, *light blue;* 4, *brown;* 5, *white;* 6, *crimson, pink;* 7, *greenish;* 8, *white;* 9, *greenish* (?); 10, *brown;* 11, *black;* 12, *cream colour;* 13, *blue;* 14, *brown;* 15, *white;* that is, 11 had the same colour as 1, 12 as 2, 13 as 3, &c.

These colours were also named instantly, and in any order, and in groups.

Case of Miss Mildred Holden.

Age	= 7	= 8	= 10½	= 13	= 14½	= 16½
Year	1882	1883	August 1885	December 1887	June 1889	June 1891
Monday	Blue	Blue	Blue	Blue	Blue	Blue
Tuesday	Pink	Pink	Pink	Pink	Pink	Pink
Wednesday	Brown *or* grey	Brown *or* grey	Brown *or* grey	Brownish	Brownish	Brownish-grey—more brown than grey
Thursday	Brown *or* grey	Brown *or* grey	Brown *or* grey	Brownish	Brownish	
Friday	White	White	White	Whitish	White	White
Saturday	Pure white	White	White	Cream; light yellow	Cream colour	Cream colour
Sunday	Black	Black	Black	Black	Black	Black
A	—	—	White	White	White	White
B	—	—	Blue	Blue	Blue	Blue
C	—	—	Yellow; cream	Cream colour	Cream	Cream
D	—	—	Dark blue	Blue	Blue	Blue
E	—	—	Red	Red	Red	Light red
F	—	—	Black	Brown	Brown	Brown
G	—	—	Green	Green	Green	Green
H	—	—	White	White	White	White
I	—	—	Black	Black	Black	Black
J	—	—	Grey; brown	Brown	Brown	Brown
K	—	—	Grey	Grey	Grey (?)	Grey
L	—	—	Dark blue	Blue	Blue	Blue
M	—	—	Brown	Brown	Brown	Brown
N	—	—	Brown	Brown	Brown	Brown
O	—	—	Yellow	Cream colour	Cream (?)	Cream
P	—	—	Green	Green	Green	Green
Q	—	—	?	Purple	Purple	Purple
R	—	—	Brown	Brown	Brown	Brown
S	—	—	Yellow	Yellow	Cream	Yellow
T	—	—	Green	Green	Green	Green
U	—	—	Yellow	Cream colour	Cream	Cream
V	—	—	White	White	—	White, I think, not sure
W	—	—	Brown	Brown	Brown	Brown
X	—	—	Not much colour	Red	Red	Red
Y	—	—		Cream colour	Cream	Cream
Z	—	—	Greenish	Green	Green	Green
1	—	—	Black	Black	Black	Black
2	—	—	Cream	Cream	Cream	Cream
3	—	—	Light Blue	Blue	Blue	Blue
4	—	—	Brown	Brown	Brown	Brown
5	—	—	White	White	White	White
6	—	—	Crimson; pink	Pinkish	Pink	Pink
7	—	—	Greenish	Green	Green	Green
8	—	—	White	Cream colour	White	Cream
9	—	—	Greenish	Blue	Bluish-green?[1]	Dark blue
10	—	—	Brown	Brown	Black?	Black *or* brown

[1] If anything.

Note.—The column for June 1891 was sent to me in a letter, as written in the table, except that *Wednesday* and *Thursday* are described as "brownish-grey, with little dots," and *Friday* as "white, with dots." The letter says:—"Is this right? I write this out without giving much thought to it—writing as fast as I can write. I am not quite definite in my mind as to the colours of 9, 10, G, T, K, O, Q, S, V; but the others have never changed. The days of the week I *never* think of without thinking of their corresponding colours. They have always remained the same. I don't quite remember if I have ever told you about the dots before, but they have always been there, and are like minute pencil marks showing through the colour. *Tuesday* is slightly dotted."

The table gives the results of the earlier experiments together with others which have been subsequently obtained. The later experiments have been made under circumstances which are peculiarly favourable—usually by correspondence during my daughter's absence at school.

The table undoubtedly represents vivid and permanent associations of colour with numerals, letters, &c. If we collect the various signs which correspond to a given colour, it appears, on the whole and in a general way, that the colour is associated with the *sound* rather than with the *form* of a letter. For example, G, P, T, Z are *green;* A, H, eight, are *white;* V, Friday, five, are *white;* C, S, Saturday, are yellow, &c. There are numerous exceptions to this, however, and it is by no means proved that there is a real law here. I simply make the suggestion on account of its bearing on the question whether or no we can think without words. It is clear that many experiments, such as are exhibited in the table, must be made before the time will arrive for definite conclusions to be drawn. Perhaps this brief note may induce others to print the results of similar investigations.

EDWARD S. HOLDEN.

Mount Hamilton, June.

Erratic Barometric Depression of May 23-29, and Hailstorm of May 24.

In connection with the very interesting letter of the Rev. Clement Ley (on p. 150), descriptive of the barometric depression which passed over these isles towards the end of last month, the following extract from a letter of mine published in the local press, with a view of obtaining further information, but without success, may be of interest. At the time when the centre of the depression lay over the mouth of the Thames, as mentioned by Mr. Ley, this neighbourhood was being visited by a thunderstorm of great severity and lengthy duration, and at 6 p.m. the rain gave place to hail, and "In the short space of twenty minutes the ground and roofs of houses were covered with a compact layer of frozen rain-drops, which at the end of half an hour (6.30 p.m.), when the storm had abated and given place again to rain, I found to have an average depth of 0·75 inch, though the stones were then reduced to about half their original size. . . . But few of the hailstones, which were nearly all ovarious in form, were smaller than 0·375 by 0·250 inch, and three which I picked up at random at 6.10 p.m. when the storm was at its height, measured respectively 0·065 by 0·051 inch, 0·437 by 0·562 inch, and a spherical one had a diameter of 0·5 inch." Such large hailstones are, I believe, rarely met with in storms near London. This one seems to have been confined to a comparatively small area, the hail falling in its greatest severity at Leyton, and not extending much beyond Walthamstow, Stratford, West Ham, and here. B. J. Hopkins.

Forest Gate, E., June 22.

"An Alphabet of Motions."

I have lately found the following extract in Arthur Young's "Travels in France, in 1787," which I fancy is not generally known. It occurs in Betham Edwards's late edition (Bell and Sons), at p. 96.

"In the evening to Mons. Lomond. . . In electricity he has made a remarkable discovery. You write two or three words on a paper; he takes it with him into a room and turns a machine inclosed in a cylindrical case, at the top of which is an electrometer, a small fine pith ball; a wire connects with a similar cylinder and electrometer in a distant apartment; and his wife, by remarking the corresponding motions of the ball, writes down the words they indicate, from which it appears he has formed an alphabet of motions. As the length of the wire makes no difference in the effect, a correspondence might be carried on at any distance."

J. S. Dismorr.

Stewart House, Wrotham Road, Gravesend, June 24.

On a Cycle in Weather Changes.

It is known that Prof. Brueckner, of Berne, in a work on "Klimaschwankungen," published a short time ago, offers a large amount of evidence for the view that our globe is subject to a weather-cycle of about 35 years, a series of cold and wet years, or warm and dry ones, recurring at about that interval. Has it been noticed in this connection that Bacon, in one of his essays (No. lviii. "Of Vicissitude of Things"), makes reference to such a cycle? The passage is as follows:—"There is a toy which I have heard, and I would not have it given over, but waited upon a little. They say it is observed in the Low Countries (I know not in what part) that every five-and-thirty years the same kind and suit of weathers comes again; as great frosts, great wet, great droughts, warm winters, summers with little heat, and the like, and they call it the *prime*. It is a thing I do the rather mention, because, computing backwards, I have found the same concurrence." A. B. M.

THE FORECAST OF THE INDIAN MONSOON RAINS.

AFTER an interval of twelve more or less prosperous years, following on the memorable Madras famine of 1876–77, and the drought and fearful mortality of North-Western India in 1877–78, India seems once more to have entered on one of those prolonged series of adverse seasons which put a severe strain on the protective powers of its Government, and, despite all human precaution, bring suffering, disease, and premature death to thousands of its industrious peasants, and to even larger numbers of the impoverished outcasts who form the lowest fringe of its teeming population, fighting the precarious battle of their life at all times on the verge of destitution. The drought in Ganjam in the autumn of 1889 has been followed by the failure of the late autumnal rains over the central districts of the Carnatic towards the close of last year, and the too familiar machinery of relief works for the able-bodied, and doles of food to the helpless indigent, has been in active operation for several months past in the districts around Madras. Another monsoon, another season of those periodical rains on which depends the fate of millions, is now due and overdue, and there comes from India an ominous note of warning that there is reason to fear that more than one great province of the empire, or certain portions of them, may again this year lie parched and barren, their young crops withering and shrivelled under the dry west wind, while, month after month, men scan with ever-growing anxiety the pale dust-obscured sky and scattered ball-shaped clouds that never mass themselves to rain-clouds, but mock their hopes with the promise of showers that never fall to moisten the sun-baked soil.

And this warning, alas! is no mere guesswork of credulous and speculative minds, such as in these latitudes certain of our would-be weather prophets love to put forth at hazard, to furnish the topic of a day's gossip to the million, or haply to win for themselves a summer day's reputation with the uninstructed, in the event of a successful issue. Certainty, indeed, there is not and cannot be till science shall have extended its domain far beyond its present limits; but, in India, the stately march of the seasons is but little obstructed by the vicissitudes of fugitive cyclones and anticyclones, that originate we know not how, and disappear by some concurrence of causes equally beyond our ken. In the tropics, and in the realm of the monsoons, all weather phenomena are more massive and slower in progress, and each great change of seasons is heralded by signs which, if we can as yet but vaguely interpret them, are at least recognizable as such, and, with a certain allowance for possible error, must be accepted as timely monitors of what is likely to follow. These it is that, whether rightly or wrongly deciphered, furnish the basis for the present warning. To those who, like the present writer, have followed for many months past, not without anxious interest, the telegraphic and other reports periodically transmitted from India, it comes as no surprise, but as a confirmation of misgivings long entertained though only now backed by the warranty of full official evidence. The events of the next three months may yet belie the present indications, and that they may do so is still our fervent hope; but it would be folly to ignore them, and to shut our eyes to the probabilities that they seem to portend.

For the last eight years it has been one of the duties of the Indian Meteorological Department, some time early in June, to prepare, for the information of Government and the public, a forecast of the probable character of the summer monsoon, based on the reports of the snowfall on the Himalaya and the western mountains, and on the indications afforded by the weather of the previous winter and spring. The possibility of framing such a forecast was in a measure foreseen by the Famine Commissioners appointed by the Home Government after the disastrous famines of 1876 and 1877, of which Commission General R. Strachey, the true founder of the Meteorological Department of India, was the scientific member; and it is in no small degree due to the weighty advocacy of this Commission that the Department owes its present extension and importance. Mr. Eliot's forecast for the coming season is now before us. It sets forth at length the general and special grounds on which he bases his conclusions; and these, though duly guarded by the reminder of their essentially empirical character, and of the unavoidable imperfection of our information regarding certain important data, are expressed in terms that leave unhappily no doubt of the adverse character of the outlook.

Attention was first directed to the apparent connection of the Himalayan snowfall with the prevalence of dry land winds in India, in the year 1877, and about the same time the late Prof. S. A. Hill and Mr. Douglas Archibald showed that, as a general rule, an unusual cold weather rainfall in Northern India was followed by a deficient rainfall in the ensuing summer monsoon. In a paper published in the Proceedings of the Royal Society in 1884, these two classes of facts were shown to be merely different phases of the same phenomenon, and a summary was given of all the evidence on the subject that had been accumulated up to that date. Since then, there has been but one year of heavy Himalayan snowfall, viz. 1885, and in that year the rains were greatly delayed on the Bombay coast, and were very deficient in North-Western India in June, July, and September, commencing late, and terminating early. During the past winter and spring the snowfall on the North-Western Himalaya and the mountains of Afghanistan and Baluchistan has been excessive—indeed, as Mr. Eliot states, unprecedented during the last twenty-five years—and from the reports received from the civil officers and observatories in the mountain districts, he estimates that an average fall of 40 feet, if not considerably more, must have fallen over all the higher ranges, from Murree eastward to Garhwal, if not to Central Nepal. That it was the same on the less accessible range of the Hindu Kush we have reason to believe from the casual reports that were received during the last winter, and we know that in Southern Europe and even in Northern Africa, snow fell down to the sea-level, and was such as has hardly been experienced certainly during the greater part of the present century. The phenomenon has therefore been one of widespread incidence, and indicates some remarkable and rare condition of those higher strata of the atmosphere which, we have now reason to believe, are the seat of the more important changes that regulate the vicissitudes of the weather of the globe.

Concurrently with this exceptional extension of the snowfall to low latitudes of the temperate zone, the Indian registers afford evidence of certain abnormal features, which are such as have been noticed on former occasions of unusual snowfall on the North-West Himalaya, and the bearing of which on the weakness of the summer monsoon is more clearly traceable. In fact, they tend to link the two phenomena together, whether we regard them as the common effects of some more remote agency, or as displaying the different steps of a physical sequence of cause and effect. The most important of these are: the unusual rainfall over the whole of Northern India in the past winter and spring, amounting to from two to three times the average in the Punjab, where it was heaviest; a prevailing low temperature in Northern and especially North-Western India, together with a corresponding excess of temperature in Assam, Burma, and Southern India; and finally, a persistent excess of atmospheric pressure in the former region and a deficiency in the latter. These anomalous features have characterized more or less all the months of the present year, especially March and May. As estimated by European standards, the anomalies of this last element may indeed appear small. For instance, the mean excess at Peshawar in May was 0·052 inch, at Mooltan 0·041, and at Quetta 0·049 inch, while the deficiency at Calicut was 0·040 inch, and at Sibsagar 0·031 inch. Taken together, they constitute an anomalous gradient from north-west to south and east of something under a tenth of a barometric inch in distances of 1300 and 1500 miles. But in India such differences are relatively large, and, as former experience has abundantly shown, very significant. As temporary phenomena they might indeed be of little importance; but, lasting as they have done through nearly half a year, they point to an anomalous state of the atmosphere which is evidently persistent, and is distinctly adverse to the northern incursion of the summer monsoon. Taking the general mean of all parts of the empire, the atmospheric pressure has been above the average in every month of the present year. With respect to the winds, Mr. Eliot remarks:—"South-easterly winds have been unusually prevalent in Bengal and Behar during the months of April and May, and north-westerly and northerly winds on the west coast of India as far south as Cochin. The unusual prevalence of north-westerly winds on the Bombay coast in the month of May was one of the features of the weather in 1876, 1883, and 1885, in which years the monsoon was greatly retarded on that coast."

Finally, after reviewing the chief characteristics of other years in which the Himalayan snowfall has been heavier than usual, Mr. Eliot draws the following conclusions with respect to the probable character of the monsoon rains of the present year in the different provinces of India:—

"(1) Snowfall conditions on the Western Himalayas, &c., and the pressure conditions in India are very unfavourable to the establishment of a strong and early monsoon on the Bombay coast. It is very probable that it will not be established in full strength on the Bombay coast before the third or fourth week in June, and it is probable that it will be below its average strength, and may be withdrawn from Upper India earlier than usual in September.

"(2) The snowfall conditions in the Eastern Himalayas, and the pressure conditions in North-Eastern India and Burma, are favourable to the advance of a moderately strong or strong monsoon in the Bay of Bengal earlier than usual, and to its establishment in Burma and Bengal before or about its normal period," and Burma, Bengal, and Assam are expected to receive an average or more than an average rainfall; Behar and the eastern districts of the North-West Provinces about the usual amount. In Southern India it is thought probable that the rains may be retarded, but that Malabar and Southern India generally are likely to receive favourable rain during the monsoon.

On the other hand, it is pointed out that "conditions are very unfavourable for Rajputana, and also to some extent in Guzerat, the southern districts of the Punjab, and the western districts of the North-West Provinces. It is probable the rainfall will be more or less deficient over the whole of that area, and possible that the deficiency may be large and serious." In Northern Bombay and Berar it is thought that "the rainfall is more likely to be slightly deficient than up to its normal amount," and that in the Central Provinces it will be "fairly normal."

From this abstract it will be seen that the region in which drought is chiefly to be anticipated is the western provinces of Northern India, comprising Rajputana, Guzerat, the southern districts of the Punjab, and the western districts of the North-West Provinces; provinces the average rainfall of which does not exceed between 20 and 30 inches, and which time after time have been the seat of disastrous famines. Now there is one consideration relevant to this subject of which no mention is made in Mr. Eliot's report, and which, notwithstanding that its bearing is purely empirical, cannot, we think, be entirely disregarded when dealing with the question of probabilities. This is the fact, first pointed out by the Famine Commissioners, that between 1782 and 1877, on no less than five occasions, a drought in Southern India was followed by a drought in Northern or rather North-Western India in the succeeding year. It does not seem possible, in the present state of our knowledge, even to suggest any physical explanation of this remarkable sequence, but it has been repeated too often to allow of our regarding it as purely fortuitous, and unfortunately it only tends to strengthen the probability of the adverse conditions inferred by Mr. Eliot from the existing state of things.

It must be confessed, then, that, according to our present means of judgment, the present outlook is by no means hopeful. The mere fact of a retardation of the monsoon rains would not in itself afford cause for serious anxiety. According to the latest report from Madras, indeed, this part of Mr. Eliot's forecast seems to have been justified by the event, for on June 26 the Governor of Madras telegraphs that the south-west monsoon rains have not set in properly in the interior, and are very light even on the Malabar coast,[1] whereas the date at which they are usually expected is the end of May or the beginning of June. The really critical months in North-Western India are August and September. If the rainfall is then abundant and continuous up to the end of the third week in September, with a final shower or two at the end of the month, all may yet be well; but if the rainfall of these months is light and partial, and if it ceases prematurely, the crops form no ear, and they perish and dry up in the warm dry west winds that speedily follow. And it is these crops that furnish the food staples of the agricultural classes of India. H. F. B.

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[1] We have taken the liberty of altering the punctuation of this telegram to bring it into accordance with sense and fact.

PHYSICAL SCIENCE FOR ARTISTS.[1]

II.

We next come to the absorption of light. I do not know whether you have had any opportunity yet in your laboratory course of observing the spectral phenomena produced when white light, or say solar light, is absorbed by different substances. The white light is broken up by the dispersion of the prisms into a rainbow band; while it is possible, by one means or another, one substance or another, to filch out of this coloured band some of the constituent colours, now at one end, now at another, sometimes in different parts at once; and when this has been done, the light which finally reaches the eye may be of any colour, as is evidenced by the different colours you see in a stained glass window. This is what happens also by the absorption of our atmosphere, due in all probability in great part to the contained aqueous vapour. The sun is white in the middle of the day and red at sunset. The blue part of the light, which, when all the colours reach us, looks white, has been taken away, and practically nothing but red is left; only certain parts of the spectrum are left. It is easy, after two or three hours' experiments with the absorption of light by different media, to grasp the laws which govern sunset colours precisely, as it is easy in the anatomical school to study the facts relating to the human form, particular muscles and the like. A diligent student will thus have the world of colour at his feet. This can, however, only be done by one interested in physical science, and I think it should be done by anyone who wishes to deal with landscapes or seascapes, anything, in fact, which has to do with the natural world. The results obtained in this way of course come to us pictorially, chiefly in the colour of sky and water and in the colours of clouds, and they are mixed up in pictures by the knowledge, or want of knowledge, of the artist who paints these various reflecting surfaces. The reflecting surface, whether water or cloud, or what not, must not only be true in colour, but perfectly formed, in order to give an absolutely perfect and pleasant picture.

Here I think it is that the need of physical science is greatest, and I do not know, in fact, that there ought not to be some kind of an examination in a College like this which shall insure that anyone who is going to take up the study of art is not colour-blind. This is done in the case of sailors and engine-drivers, and I think it should be done in the case of artists. There are pictures which have apparently been painted by colour-blind people; and of course it should be a subject of great regret that so much skill has been wasted in consequence of such a malformation as this.

It may be, of course, that in some cases, where the thing may be charitably supposed to arise from a physical defect, it is the result of mere ignorance, or want of observing power; but if that be so, then my point is proved, because it is clear that a good scientific training will cause these objectionable, impossible, colours to be gradually eliminated from our exhibitions. On the other hand, when we look at a gallery of pictures, one is so frequently rewarded by the exquisite truth of some of them, that one could very well look over the defects of others, if all thoughts of the possible progress of art achievement were banished from one's mind.

Some of you may perhaps have read Mr. Ruskin's chapter on clouds. The scientific basis of the various cloud forms, however, you will not find there. Now when we consider that in land- and sea-scapes the sky, and especially the clouds, are among the most important reflectors of light, whether white or coloured, the form of the cloud is absolutely of very high importance. If the light is reflected by an absolutely impossible cloud, your delight at the colour, which may be true, is absolutely checked by the treatment of the anatomy of the cloud. Here, again, we touch a distinct branch of physical science. An acquaintance with the various forms of condensation assumed by aqueous vapour under the various conditions of the atmosphere would certainly keep one right where one would be very apt to go wrong. I referred, also, to the reflection of sunlight, whether white or coloured by absorption, by water. Here, I think, is a region where physical science is also helpful. There can be no question that the grandest display of colour in the natural world is a sunrise or sunset, either at sea, or where there is a water surface to bring in a second series of reflection phenomena. As a rule, perhaps, if the water be somewhat land-locked, or at all events not broken up by strong wind, the effect is finer, and this perhaps is one of the reasons, but only one, why the sunsets seen off the west coast of Scotland are so remarkable.

This, however, does not always hold. I have seen a sunrise in the Mediterranean when passing the Straits of Gibraltar twenty years ago, which was so magnificent, that not only is it still present in my mind's eye, but all the sailors who were swabbing the deck at the time ceased work and simply gazed at it entranced. It promised to be a cloudy sunrise, but suddenly the cloud pall melted into mackerel sky, and the sun at rising payed out different colours on the high and low patches; the sea was choppy, and every facet of every wave, and every facet of a facet, being turned to different parts of the sky; these picked up and reflected to the eye different colours, so that every wave looked like a casket of gems.

The red or yellow colours on the clouds depend simply upon the thickness of the atmosphere which the sunlight has traversed to reach them; the colour depends absolutely upon the light received from the sun, and it has nothing to do with the apparent angular distance from the sun in your picture; but while all this change is going on in the clouds the sky itself will be zoned above the horizon from the red to the blue overhead, and in addition to that, you will get the greater luminosity nearer the sun's place. But further than that the sky will not go, because it cannot. At the same height above the horizon you must have absolutely the same sky colour. Now that is a very obvious conclusion. You will always note the greatest possible distinction between the colour of the pure sky and of the clouds. A favourite *sky* colour in sunsets is green. I have seen no green *clouds* except in pictures.

I have noted a few of those pictures this year, which, in my opinion, and I only give it for what it is worth, are remarkable for their truth, or for the absence of it, in different degrees. The numbers are those of the Royal Academy Catalogue, unless otherwise stated:—

Clouds.—Good colour, 351.
Good form, 288, 600.
Good colour and form, 238.
Water.—Good colour, 630, 1029.
Good surface, 682, 759, 1013. New Gallery, 102, 120.

With great deference I must, until convinced to the contrary, hold that much of the colour in the following pictures is *impossible*—543, 1028, 176, 192, 515; it is bad in 203, 498, 586, 602, 1044, 1071.

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[1] Continued from p. 178.

The cloud forms in 498, 536, and 966 are unlike anything I have seen in any quarter of the world.

But cloud is not the only thing we have to deal with. There is a still finer form of aqueous vapour which shows itself as *atmosphere;* its function is to soften distant outlines, to gradually assimilate colours, laying, so to speak, its own upon them, and then, again, to soften even this. So that distant vistas of hills and vales first become blue in prevailing tone, but the most distant ones lose this, and fade to a more neutral tint.

These things this year are admirably rendered in 1130. 293 offends by the impossible hardness of the hill on the right of the picture.

To most of you the terms selective *absorption* and selective *reflection* of colour are familiar; of the latter an admirable study is to be found in 1062. For reflection badly managed, study 145 in the New Gallery. The artist seems to be under the impression that some birds have a special capacity for reflecting colour.

Of special studies of various natural objects, I think the following in the German Exhibition are worth examination: a glacier (287); cloudy moonlight (433); careful study of light-grading (but sun should be more luminous in the latter) (52, 343).

It is not a little singular that we should find such a close association between bad cloud forms and bad colour. It was a true instinct which led Mr. Ruskin to treat of these matters in his "Modern Painters," but why did he not go further into the real basis—the real grit of it all, instead of confining himself to the mere fringe of these great subjects? It was, I expect, because the possible connection between science and art was less recognized then even than it is now. But is it too late? No one could touch the questions still with more sympathy than Mr. Ruskin.

But to come back to the pictures. Almost, if not quite as good as 600, is No. 50 in the German Exhibition We find in 630 a careful study of colour. The most wonderful colour which can be got on nearly still water is that you sometimes see at sunrise or sunset with a good green or yellowish sky near the horizon, and a perfectly blue sky overhead. In that case every unit of the surface (every undulation) will reflect to your eye a certain amount of horizon-light and a certain amount of blue sky, and the total result will be a sea of molten steel. Another point in this connection is this: if your surface is even, you can get a reflection of this kind from several surfaces besides water. I was in Egypt last winter, and I saw a wonderful sunset, looking out from the little quay at Ismailia. The sand of the desert lay beyond and round the water in the foreground, which was more or less bluish; the lake, in fact, is bounded by sandbanks of no great elevation, the canal coming in at one end and running out at the other.

In the day-time in full sunshine the sand is yellow, as yellow as it can be, and at sunset it is grey-white. There is nothing very remarkable in the sky, but the intense blue in the sky overhead. There is no aqueous vapour to absorb, and therefore there is no colour. But wait for the afterglow! when you get sunlight, reflected from the clouds or sky, which reaches your eye after two transmissions through the lower air; then you can get colour, and you do get it. What you see is the most exquisite violet halo, and the colours with which we are familiar here more or less; but the striking thing is the intense violet halo in the sky, and the warming up of colour till the sunset place is reached. Well, now, what is the effect of that upon the landscape? Everything is turned green, for the simple reason that the only constituent common to the colour which reaches, and is reflected most readily by, the sand, is the tint of green: the sands change, as if by magic, into a wonderful chlorophyll green. Now, I venture to think that the artist who endeavours to work out problems of this kind will be more likely to paint a beautiful picture than the one who copies nature merely, and this brings me into strict harmony with the Academy motto. It seems to me that physical science may in this way, if associated with the study of art, give us new possibilities in the art future that will transcend anything that we know of now, and the time will certainly come, ultimately, when the highest art will result from the study of natural science and the science of the human form.

Seeing that already artists spend years in the study of only one part of the field of observation, they must surely in time come to the conclusion that it would be better to annex other branches.

It would not be right if I concluded what I have to say without calling attention to the important remarks made by Mr. Briton Rivière, on science in relation to painting, at the Edinburgh Art Congress:—

"Whatever may have been done in other lines of human energy during the Victorian age, there can be no question that its most remarkable achievements, both theoretical and practical, have been those of science. . . . The art of the painter has not escaped its influence. On one side, and a very important one—that of realism—the side which furnishes the language—*i.e.* the signs and symbols which express the idea of the artist—there is a wide front open to the influence of science; and on that side art has not been slow or unwilling to follow the advice of science, or ungrateful for the valuable help it has afforded. According to my theory, this supremacy of science would have influenced art under any circumstances, but it has been able to do so through the very method and language of art itself.

"Will this influence help or retard the influence of art? My answer is, it may do either, according to the manner in which it is received and used by the artist. If the painter resolutely holds the belief that painting is a language, and a work of art the expression of an idea, and uses science, and all that it has discovered and teaches, to enable him better to understand his signs and symbols, viz. the material facts of nature, so that by means of them he may express himself correctly, just as a writer has behind him the philologist to busy himself about the derivation and meaning of words, and the grammarian to show him how to place these words so as to produce the meaning he requires—if, I say, the painter so receives and uses the knowledge and appliances of science, then I think the cause of art will be much advanced by science, and works produced under its influence will be stronger and richer than they could possibly have been without it. On the other hand, if the painter allows this scientific knowledge of the material or realistic part of his work to obscure the purely artistic or ideal part of it, to obscure instead of to intensify the *idea;* and if, carried away by the material wonders of the 'thing' which science has unfolded, he forgets the 'thought' altogether, then assuredly, however true he may have shown himself to be to the cause of science, that of art will suffer at his hands—indeed, may be lost altogether. For I feel sure that most of my brother artists will agree with me that it is possible for a picture to be scientifically true and have no art at all in it; and, on the other hand, to contain several scientific blunders and yet be a great work of art."

It will be seen, then, that I have ventured to-day to preach no new doctrine to you; even my gloss on the Academy motto is endorsed by Mr. Briton Rivière.

But I can go further than this, and quote Prof. Helmholtz in support of the gloss. You should all read his admirable lecture "On the Relation of Optics to Painting."[1] In it he remarks, "The artist cannot transcribe Nature: he must translate her;" and he adds, "This translation may give us an impression, in the highest degree distinct and forcible, not merely of the objects themselves, but even of the greatly altered intensities of light under which we view them. . . . Thus the imitation of Nature in the picture is at the same time an ennobling of the impression on the senses."

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[1] "Popular Scientific Lectures," Helmholtz, 2nd Series, p. 135. (Longmans. 1881.)

Let me congratulate you on the fact that here, at all events, the importance of physical science in its relation to art is not forgotten. J. NORMAN LOCKYER.

LUMINOUS CLOUDS.

IN an article contributed to NATURE on November 20, 1890 (vol. xliii. p. 59), Herr O. Jesse (of the Royal Observatory of Berlin) gave an account of his observations of luminous clouds. He has recently submitted to the Prussian Royal Academy of Sciences a record of later work; and, as the subject is one of considerable interest, it may be worth while to translate his paper.[1]

With regard to the results, already briefly noted, obtained in the summer of 1890, I have now to report more precisely, that with the help of the grant made by the Academy of Sciences we were able, during the period when the phenomenon appeared, to secure a collection of photographs which afford rich material for study.

On this as on previous occasions the clouds were visible only between the end of May and the beginning of August. They appeared for the first time, in 1890, on May 26; for the last time—and then there was only a feeble trace of them—at the beginning of August. The phenomenon, therefore, was seen within nearly four weeks of the summer solstice—before and after it—but chiefly after it.

Since my last report, I have received confirmation of the statement that the time when the phenomenon appears in the southern hemisphere has a corresponding relation to the summer solstice there. Unfortunately, however, more precise facts with regard to place, &c., in the southern hemisphere, are still lacking.

During the period between May 26 and July 24, 1890, we obtained altogether 180 photographs of luminous clouds at Steglitz, Rathenow, and Nauen, and at the Observatory of Urania, Berlin. Of these photographs, 75 are suitable for the determination of height, inasmuch as they were secured at the same time in at least two different places. Thirty of the photographs may be used for the determination of the speed and direction of the movements of the clouds, because their representations of the clouds were taken at proper intervals at one and the same place. The remaining photographs are adapted for investigations relating to the dimensions of the clouds and their structure.

The phenomenon was again less bright than it had been in the preceding year. Only when the atmosphere was exceptionally transparent was there an approach to the former brilliancy. The aggregations of these masses of particles are obviously becoming thinner, as may also be perceived from the more distinct appearance of certain relations of structure, like the ridge and rib formations (wave formations) mentioned in my last report. Formerly these were concealed by superposition and apparent interference of a greater richness of analogous strata; now the characteristic lines of the configurations consisting of these ridge and rib formations present themselves more simply and in greater isolation.

It has now been proved more successfully than before that the ridges or longitudinal strips lie parallel to, while the ribs or cross strips are almost at right angles to, the direction of the movement of the entire cloud. Further, we made on different days several series of measurements of the distances of the ribs (wave-crests) from one another with the following groups of results:—

				Kilom.
Mean value of the distances of	9	wave-crests	...	8·3
,, ,, ,,	10	,,	...	9·9
,, ,, ,,	10	,,	...	8·4
		Average	...	8·9

Especially striking, last summer, was the difference between the clearness with which the clouds appeared in the morning hours, and that with which they appeared at the corresponding times before midnight.

With regard to the height of the luminous clouds in the summer of 1890 the measurements, so far as they were definitely calculated, gave the mean value of 82 kilom., agreeing almost exactly with the value of nearly 83 kilom., deduced from my photographs of 1889.

The persistence from year to year—now for the first time shown with sufficient accuracy—of the distance, and therefore of the position of the level surface of the phenomenon, would alone deserve to be recorded as a scientific fact of great importance.

As for the speed and directions of the movements, it was again found that the chief component of the movement was directed from east to west, and amounted to nearly 100 metres in the second, while the speed of the revolution of the zone of the earth above which the clouds were placed is about 240 metres in the second from west to east.

There was also a smaller and variable component in the direction of the meridian. This was directed from north to south at the times at which we have hitherto obtained tolerably secure determinations of movement.

The points of view from which the phenomenon of luminous clouds, on the ground of the observations hitherto made, is to be regarded, are already numerous. There is still, however, a wide field for research in connection with the questions, What are the forces which make the phenomenon appear chiefly in the morning hours? and, What is the nature of those forces which cause the movement of the clouds to be mainly from the north-east, and drive them from the northern to the southern hemisphere and back again? Then the question as to the height of the phenomenon in different latitudes is probably of great importance for the constitution of our atmosphere; and not less interesting is the question relating to the material of which the luminous clouds are composed. Unfortunately the interest taken by the scientific world in this remarkable phenomenon is in general so small that during the short time the phenomenon will probably present itself we can scarcely expect to obtain for these questions answers that shall be to any considerable extent satisfactory.

WILHELM EDUARD WEBER.

THE venerable physicist, Wilhelm Eduard Weber, whose death on June 23 we shortly announced last week, was born at Wittenberg on October 24, 1804, the second of three sons of Michael Weber, Professor of Positive Divinity at Wittenberg. He studied at the University of Halle, where Schweigger was then Professor of Physics; he took his Doctor's degree in 1826, became Privatdocent in the following year, and Professor-Extraordinary of Physics in 1828. In 1831 he was called to Göttingen to succeed Johann Tobias Mayer in the Chair of Physics, and remained there till 1837. Among other results of the death in this year of King William IV., there came about serious changes in the University of Göttingen. Queen Victoria being excluded from the throne of Hanover, by the operation of the Salic law, her uncle, Ernest Augustus, Duke of Cumberland, became King of Hanover. This prince held high views as to the powers of hereditary rulers. In his view the narrow liberties enjoyed by his subjects, under the Constitution reluctantly granted by William IV. in 1833, were excessive and intolerable. He suspended the Constitution, and thereby called forth vigorous protests from Dahlmann and other Professors of the Hanoverian University. As a punishment, seven of them—Dahlmann, Weber, the two Grimms (Jacob and Wilhelm), Albrecht, Gervinus, and Ewald—were ejected from their chairs, and Gervinus, Dahlmann, and Jacob Grimm were even expelled from the country. From this time Weber lived for some years in retirement, but in 1843 he accepted the Professorship of Physics in Leipzig (in succession to Fechner), and in 1849 he returned to his former position in the University of Göttingen. He was in Göttingen at the time of his death.

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[1] "Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin," 1891. xxvi. Sitzung der physikalisch-mathematischen Classe, vom 28 Mai. "Untersuchungen über die sogenannten leuchtenden Wolken," von O. Jesse, Steglitz.

Wilhelm Weber's eldest brother, Ernst Heinrich, was the celebrated Professor of Anatomy and Physiology at Leipzig. He was born at Wittenberg in 1795, and died at Leipzig in 1878, having been elected a Foreign Member of the Royal Society of London in 1862. The youngest of the three brothers, Eduard Friedrich, was also highly distinguished as an anatomist, and held office for many years in the University of Leipzig.

Weber's first contribution to science at once took rank as a scientific classic, a position it is likely to keep for many years to come. This was "Die Wellenlehre auf Experimente gegründet," a volume of 574 pages, and 18 copper plates, nearly all engraved by the authors, published in 1825 by the brothers Ernst and Wilhelm Weber, and embodying the results of numberless original experiments and observations. One of the most striking results of these investigations was the discovery that, when a regular series of waves follow each other along the surface of water, the particles at the surface describe vertical circles whose plane is parallel to the direction of propagation of the waves, and those lower down ellipses of which the vertical axis becomes smaller and smaller with increasing depth. As to the composition of this work, the authors say that it grew up as the result of such constant and intimate communication between them with regard to all parts, that it is impossible to assign to either of them the separate authorship of any distinct portions.

For several years Weber continued to occupy himself mainly with questions of acoustics, on which he published various papers of importance. In 1833 he published, in conjunction with his brother, Eduard Friedrich, a memorable investigation into the mechanism of walking "Mechanik der menschlichen Gehwerkzeuge").

But it is chiefly by his magnetic and electrical researches that Weber's place in the history of science is marked. These are contained for the most part in the "Resultate aus den Beobachtungen des magnetischen Vereins," published by Gauss and Weber from 1837 to 1843, and in Weber's "Elektrodynamische Maassbestimmungen" (published in collected form in 1864, though the first paper dates from 1846). In this series of papers Weber showed for the first time how methods of absolute measurement, analogous to those which Gauss had very shortly before shown to be applicable to magnetic measurements, could be extended into the region of electricity. Before this time Ampère's splendid discoveries as to the laws of the mutual forces between magnets and conductors traversed by electric currents, or between two such conductors, had been made known, and G. S. Ohm had established once for all the relations between electrical resistance, electromotive force, and strength of current; but, nevertheless, there was as yet no settled system for the measurement and statement of electrical quantities themselves. Until Weber's time electrical measurements were merely comparisons between magnitudes of the same kind: the resistance of one conductor could be compared with that of a particular piece of wire, the electromotive force of one battery could be compared with that of another; but that the value of an electrical quantity could be stated without reference to any quantity of the same kind, and in terms not involving any physical constants but the units of length, time, and mass, was as yet an entirely new conception. Weber, however, not only showed that such a system of measurements was theoretically possible, but in a series of most masterly experimental investigations he showed how it could be practically carried out. Our countryman Sir William Thomson was one of the very first men of science to recognize the fundamental character and far-reaching importance of Weber's work; and owing mainly to his clear-sighted advocacy of the absolute system of measurement, this system was from the first adopted as the basis for the operations of the British Association Committee on Electrical Standards, appointed originally in 1862. This system has now become so familiar to electricians, and is taken so much as a matter of course, that it requires some mental effort to recall the state of science when it did not exist, and to appreciate the intellectual greatness of the man to whom it is due. If we consider method and point of view, rather than acquired results, it is not too much to say that the idea of absolute measurements, underlying as it does the conception of the conservation of energy, constitutes the most characteristic difference between modern physics and the physics of the early part of our century. And to no one man is so large a share in this great step due as to Wilhelm Eduard Weber.

Weber was a Corresponding Member of the Institute of France. He was elected a Foreign Member of the Royal Society in 1850. G. C. F.

A SOUVENIR OF FARADAY.

The following letter, written by an old friend of Faraday's and of mine, long since dead, may interest your readers, now that we are celebrating the centenary of Faraday's birth. It came in reply to one in which I asked Mr. Ward's assistance in preparing an obituary notice of Faraday for the *Chemical News*.

William Crookes.

Cornwall, August 30, 1867.

Dear Crookes,—I should be proud indeed to be the spokesman of the chemical world in doing honour to Faraday's illustrious name on the occasion of his accession to immortality.

But I should not dare to meddle with the laurels on so august a brow, without many days and nights of earnest research and meditation, to fit me for summing up, without omission, the splendid list of his imperishable labours.

Only in this reverential spirit of earnest solicitude to do aright, which is, if I mistake not, the philosophical counterpart of prayer—of the religious feeling—could so solemn a duty be fitly undertaken.

Only with the aid of other minds, kindred with Faraday's in genius, and filled with the light of his manifold discoveries, could any one man's mind become an adequate mirror to reflect the gigantic Shadow that has just passed to its place in futurity.

For the present it is my fate to fulfil much humbler duties—which, having undertaken, I have no right to set aside. For duty must still be done, even when such appeals as yours set the wings of the caged lark trembling, and point him upwards to his barred out home.

I must remain, therefore, a unit among the millions whose hearts do silent homage to the illustrious dead; and can but watch from afar the starry coronation of which you invite me to be minister.

So best, perhaps. For, after all, the name and fame of

Faraday transcend all pomp of celebration, all burning words of praise. For whose the pen to weave so bright a glory as that electric fire which glows, through all the ages, round his brow, who first drew lightning from the lodestone, as Franklin drew it from the sky?

In the moment of separation that little spark breaks forth—instantaneous yet eternal. It is but one vivid point of the radiance that encircles his name, yet of itself it is glory enough.

From that spark a new branch of science has sprung, and under its creator's name, were it mine to carve his epitaph, these three should be the chosen words:—

FULMEN ERIPUIT FERRO!

Ever yours faithfully,
F. O. WARD.

NOTES.

We print elsewhere an account of the fourth annual meeting of the National Association for the Promotion of Technical and Secondary Education. After the meeting an important conference was held, and it is now hoped that all the influences which are tending towards the establishment of a proper system of technical instruction in England may soon be thoroughly organized. Next week we shall have something to say about the work of the conference and about the Association's report.

The *conversazione* given by the President of the Institution of Electrical Engineers, Prof. Crookes, F.R.S., and Mrs. Crookes, on Monday evening, was brilliantly successful. It was held in the galleries of the Royal Institute of Painters in Water Colours, Piccadilly. There were about 800 guests, among whom were many eminent men of science.

On Tuesday evening the Fellows of the Royal Meteorological Society and their friends dined together at the Holborn Restaurant, to celebrate the entrance of the Society upon its new premises in Great George Street, Westminster. Mr. B. Latham, the President, occupied the chair. Mr. A. R. Binnie (Engineer to the London County Council) proposed "The Royal Meteorological Society," and Mr. G. J. Symons responded. Mr. Latham, replying to the toast of "The President," referred to the enormous amount of records in the possession of the Society. All they now wanted was a few more members. However, they had gone on increasing, and were now in a prosperous state, as they had been able to collect from the members of the Society a considerable sum of money, which had been funded, and the interest on which would meet the expenses of the new establishment. The Society now possessed one of the finest meteorological libraries in the world, and one which would be of enormous value to future generations.

We are glad to note that the Marine Biological Association have now only three unoccupied tables. Many investigators are taking advantage of the facilities offered them at Plymouth.

The Exhibition Committee of the Photographic Society of Great Britain announce that the annual exhibition of that Society will be held at the Gallery of the Royal Society of Painters in Water Colours, Pall Mall East, from Monday, September 28, until Thursday, November 12 next. The exhibition will be open daily (Sundays excepted) from 10 a.m. to 5 p.m., and on Monday, Wednesday, Thursday, and Saturday evenings from 7 p.m. to 10 p.m. Medals will be awarded for artistic, scientific, and technical excellence of photographs, for lantern transparencies, and for apparatus.

The Pacific Postal Telegraph Company had lately a gathering of some 500 guests at the opening of a new telegraph office in San Francisco. After shortly describing the various instruments, Mr. Storrer, the superintendent, said he was often asked how long it took to telegraph to different places and get a reply. He would therefore now send a telegram to Portland, New York, Washington, Seattle, Tacoma, Canso (Nova Scotia), and London, inquiring about the weather. The first reply came from Portland in 3 minutes, "Weather fine"; the next from New York in 3 minutes 10 seconds, "Misty and warm"; Washington in minutes 11 seconds, "Misty and warm"; Seattle in 3 minutes 21 seconds, "Misty and calm"; Tacoma in 3 minutes 28 seconds, "Misty, cool, and calm"; Canso, Nova Scotia, in 4 minutes 20 seconds, "Cold and misty"; while the answer "Misty and cold" came from London in 6 minutes 22 seconds.

The Governors of the Royal Holloway College have appointed Miss M. W. Robertson to the Resident Lectureship in Natural Science. Miss Robertson, who is now a lecturer on the staff of Alexandra College, Dublin, has taken the degrees of B.A. and M.A., with high honours in chemistry and physics, at the Royal University of Ireland, and has also gained the University Studentship in Experimental Science.

The Education Department has issued a memorandum, by Mr. J. G. Fitch, on the working of the free school system in America, France, and Belgium.

The death of M. Rodolphe Kœppelin, a distinguished chemist, is announced. He was born at Colmar in 1810, and from 1828 to 1859 held the Chair of Physics and Natural History at the College of his native town. For many years he was intimately connected with the Agricultural Society of the Upper Rhine, and, as a chemist, he was able to render great services to the agriculturists of his department. After the Franco-German war, M. Kœppelin quitted Alsace, and settled in Paris, where he was regarded as one of the most eminent members of the Alsatian colony.

In another part of the paper we print a report, by Herr O. Jesse, of his observations of luminous clouds in the summer of 1890. We learn from Herr Jesse that on the night of June 25–26 last the luminous clouds were again very visible at Steglitz and Nauen, and that they were photographed eight times simultaneously at these two places. Writing to us from Sunderland on July 1, Mr. T. W. Backhouse says there was a fine display of the luminous clouds during the previous night, their motion being, "as usual, from a north-easterly direction." Mr. D. J. Rowan informs us that on the same night, from 11.30 p.m. to 12.30 a.m., the clouds, as seen at Kingstown, co. Dublin, "appeared well-developed on a polar arc of 30° and at a mean altitude of 5°." They had been faintly visible at Kingstown on June 3, 7, and 9. It is astonishing that no observer seems yet to have had energy and intelligence enough to take spectroscopic photographs of these striking phenomena.

According to a telegram from Melbourne, dated July 4, the Swedish-Australian Antarctic Committee of the Victorian branch of the Royal Geographical Society, which was formed to raise subscriptions in order to take advantage of Baron Nordenskiöld's offer to equip an expedition to the Antarctic regions, announces that a sum of £3000 only is required to complete arrangements, and that there is every prospect of the expedition starting in about fifteen months' time. It is expected that the expedition, in addition to its geographical and other scientific discoveries, will be the means of opening up extensive whale and other fisheries in the Antarctic seas.

We learn from the *Botanical Gazette* that Lieut. R. E. Peary, of the U.S. Navy, proposes to reach the North Pole on foot through Greenland, starting from Whale Sound, and expecting to be absent from 1½ to 2½ years. He states that the region about Whale Sound is rich in Arctic plants, Kane having brought over 106 species of Phanerogams and 42 of Cryptogams, several of which were new, but that very little has been done in its investigation since that time.

THE danger of using arsenical preparations for the poisoning of plants is illustrated by the fact that Dr. B. L. Robinson, assistant in the Gray Herbarium, Cambridge, U.S.A., has been compelled to resign his position owing to ill-health resulting from this cause. It is stated that the poisoning of plants has now been entirely abandoned in the herbarium; the tightness of the cases, and constant handling of the sheets being relied on to preserve the specimens.

MR. SPENSER LE MARCHANT MOORE has been appointed botanist to the Matto Grosso Gold and Explorations Concessions Expedition, which is about to depart for Brazil.

A NEW botanical journal has just been started, devoted to the diseases of plants, *Zeitschrift für Pflanzenkrankheiten*, edited by Dr. Sorauer, and published at Stuttgart.

DR. JOHN MURRAY contributes to the *Journal of Botany* for July a very interesting account of the Clyde sea-area, its physical characters, and the chief features of its natural history. This sea-area is a natural system of deep-sea basins or lochs in the west of Scotland, communicating southward with the Irish Channel by a single opening between the Mull of Cantyre and the shores of Wigtown and Ayr. It has a water surface of about 12,000 square miles; its greatest depth is 107, and its mean depth about 29 fathoms. There is a great variety in the pelagic fauna and flora in the surface and intermediate layers of water, the abundance and the species of organisms varying in the different layers according to the seasons, and even in different years. There is likewise a great variety in the bottom-living fauna and flora, which varies according to the nature and depth of the bottom in the different parts of the area. In some of the deeper lochs a few animals are met with which do not usually occur in more open situations around our coasts till a depth of 200 or 300 fathoms is reached. Some of these forms are limited to one loch on the west coast; for instance, *Conchæcia elegans*, which is abundant in Loch Etive. This form has never been taken in any of the lochs of the Clyde sea-area, although *Euchæta norvegica*, with which it is associated in Loch Etive, occurs abundantly in Upper Loch Fyne and Loch Goil. *Nyctiphanes norvegica* and *Boreophausia Raschii*, which are abundant in the upper lochs of the Clyde sea-area, do not, on the other hand, occur in Loch Etive.

THE French Minister of Public Works has addressed a circular letter to civil engineers, asking them to use their influence to protect prehistoric monuments from the injury often done by ignorant proprietors. It seems that little respect is shown for such monuments in some parts of France. *La Nature* speaks of a proprietor who sold "a magnificent dolmen," which was to be transformed into "a tomb in a cemetery."

IN his report, for 1890, to the trustees of the Peabody Museum of American Archæology and Ethnology, Prof. F. W. Putnam, the Curator, records that in no former year had the friends of the Institution been so generous in giving aid. Gifts for current expenses were received which, in the sum total, exceeded the regular income from the funds; and Mrs. Mary Copley Thaw, of Pittsburg, added no less than 30,000 dollars to the amount held in trust, this sum being set apart as an endowment for a fellowship.

AN apparatus has been recently constructed by M. Ducretet, for getting quickly in the laboratory a fall of temperature 70° to 80° C. below zero, by means of the expansion of liquid carbonic acid. The inner of two concentric vessels contains, in alcohol, a serpentine metallic tube communicating through a tube with two stopcocks, with the carbonic acid reservoir outside, and opening below into the annular space round the inner vessel, in which are some pieces of sponge impregnated with alcohol. This two-walled vessel with coil is inclosed in a box. One stopcock being opened wide, the other slightly, the carbonic acid passes through the coil as snow, and turns to gas, with strong cooling effect, and any of it not vaporized in the coil is dissolved in the alcohol of the sponge. The gas escapes through a tube passing through the outer box. The instrument, called a *cryogen*, is represented in *Cosmos* of June 27.

EXPERIMENTS have lately been made by Herr Regel (*Bot. Centralb.*) with reference to the influence of external factors on the smell of plants. In the front rank appears the direct and indirect influence of light on the formation of etheric oils and their evaporation. In the case of strongly fragrant flowers (as *Reseda*) heat and light intensify the fragrance, which in darkness is lessened without quite disappearing. When the whole plant was darkened, those buds only which were before pretty well developed yielded fragrant flowers; the others were scentless. If, however, only the flowers were darkened, all were fragrant. Other plants open their flowers and smell only by night (as *Nicotiana longiflora* and *Nycterinia copensis*). When these plants were kept continuously in the dark, they, in course of time, lost their scent, as they lost their starch. On being brought into light again, both starch and fragrance returned. Besides light, respiration has a decided influence on the fragrance. Nycterinia, inclosed in a bell jar with oxygen, behaved normally, but with hydrogen the flowers did not open, and had no fragrance. In general, the opening of flowers coincides with their fragrance, but there is no necessary connection between these phenomena.

A NEW antiseptic, said to have certain advantages over those hitherto in use, has been brought before the French Academy of Medicine by Prof. Berlioz, of Grenoble: extreme solubility, harmlessness, efficacy, and rapidity of action are claimed for it. It is called *microcidine*, and is a compound of naphtol and soda, is neither poisonous nor irritant, is twenty times as active as boric acid, and much more soluble than thymol, carbolic acid, &c. Microcidine has the form of a greyish-white powder. In a solution of 3 grammes per litre it is very slightly coloured, but it does not stain either the hands or bandages. For family use it is said to be of great service.

MOST Russian geologists are now of opinion that the boulder-clay which covers the whole of Middle Russia is nothing but the bottom moraine of the ice-cap which, during the Glacial epoch, extended from Scandinavia and Finland to the latitude of Kieff and Poltava. A couple of years ago, Prof. Pavloff, while working in connection with the Geological Survey in Nijni Novgorod, indicated some traces of an inter-glacial milder period among the glacial deposits covering the province. Like indications have been noticed in Poltava and Tchernigoff. New data to confirm this view are now given by N. Krischtafovitch in the *Bulletin* of the Moscow Naturalists (1890, No. 4). After a careful exploration of the Quaternary deposits at Troitzkoye—a village on the Moskva River, seven miles to the west of Moscow, the diluvial deposits of which have very often been mentioned since Prof. Rouillier's and Murchison's times—the Russian geologist came to the conclusion that these deposits are indicative of an inter-glacial period, during which Middle Russia had a flora and fauna much like those which exist now, but with the addition of the Mammoth. The layers described by M. Krischtafovitch as inter-glacial are of lacustrine origin; they are covered with undoubtedly glacial deposits, and they are deposited over glacial sands containing boulders of northern origin. Further research, however, is wanted. It is certain that, both during the first invasion of the ice-cap and its ultimate retreat, its outer limits must have been subject to very great oscillations. We know that, in Greenland, parts of valleys which for hundreds of years were covered with vegetation, are sometimes invaded again with ice, and that lacustrine deposits must arise in this way between purely morainic deposits. The same must have taken place in the ice-cap of Russia; and the oscillations of the glaciers on the outer border of a large ice-cap are on a much greater scale than the oscillations of isolated glaciers in Alpine regions. When the ice-cap began to invade Middle Russia, its advance was undoubtedly accompanied by many oscillations; regions invaded by ice must have been set free of ice for a succession of years, and they became the seats of lakes. The same oscillations must have taken place during the retreat of the ice-cap. The existence of a warmer inter-glacial period, therefore, though not improbable in itself, can be proved only by means of a very wide exploration of the boulder-clay, and such an exploration has not yet been made.

THE system of meteorological observations in Alsace-Lorraine has now been centralized, a meteorological service for the Reichsland having been established. The control of the new service has been intrusted to the geographical seminary in connection with the Strassburg University, and has been definitely undertaken by Dr. H. Hergesell, who desires to organize the service in accordance with the best modern ideas. A meteorological record will be issued as a part of the German meteorological *Jahrbuch*.

A REMARKABLE series of three hailstorms which passed over Graz on August 21 last year, about 5, 6, and 7 p.m. respectively, has been carefully studied by Prof. Prohaska (*Met. Zeits.*) Stones from 1·6 to 2·4 inches in diameter fell in the town, forming a compact ice-mass, in some places about 3 feet thick, and a white cloud of vapour formed over the ice. It is noteworthy that all three storms took a nearly quite straight path over mountain, valley, and plain; no influence of mountains on the direction was perceptible. The advancing strips of hail were 10 to 14 km. in width; the first went 173 km. east-south-east; the second and third almost exactly east; one 110 km., the other 201 km. The 70 km. stretch of country from Stiwoll over Graz to the Hungarian border lay in the path of all three, so the ice deposited by the first offered no hindrance to the others. Mountains seem to have affected the velocity, if not the direction, of the storms; they were passed more slowly than plains or undulating ground (35 km. an hour against 49 km.). A violent wind came out from the hail column, a west or north-west wind in front, north on the south side. But further out, in front especially, there was a well-marked air-current towards the hailstorm; and this was especially strong on the lee side of a mountain. Whirling movements were not observed, and there was but little thunder and lightning. The falls of temperature were very pronounced: *e.g.* in the first storm from 26° C. to 5°. The barometer went down before each hailstorm, then suddenly rose.

AT the meeting of the Linnean Society of New South Wales on May 27, Mr. Henry Deane stated that in April, while travelling by night through the Big Scrub in the Richmond River District, his interest was aroused by the remarkable effect produced by luminous insects which abounded by the roadside. Specimens were secured and sent off in the hope that they would arrive in time to be exhibited at the previous month's meeting, but they came a day too late, and in the meanwhile had died. From their general resemblance to the larvæ of *Ceroplatus mastersi*, Sk., which are also phosphorescent, Mr. Fletcher, who had seen the specimens forwarded, was of the opinion that these were very probably also dipterous larvæ. Mr. David made some remarks on certain luminous organisms which he had observed in old coal-mine workings in Illawarra, the identification of which it was hoped would not long be postponed.

MESSRS. CASSELL AND CO. have issued Part 33 of the "New Popular Educator," which is to be completed in forty-eight parts. The present number includes, besides the illustrations in the text, a coloured representation of insectivorous plants.

THE first volume of Messrs. Whittaker's new "Library of Popular Science" will be an elementary introduction to astronomy, by Mr. G. F. Chambers. The volume will be ready in the course of a few weeks, and will shortly be followed by others.

AN interesting report, by Mr. Campbell, of the British Consular Service in China, has been issued by the Foreign Office. It is the record of a journey of over 1300 miles in districts in Northern Corea, many of which have never before been visited by Europeans. Mr. Campbell started from Seoul, the capital, and crossed the peninsula to the treaty port of Won-san (Gensan), and thence pursued his way along the east coast around Broughton Bay, whence he turned north-eastward, crossing the Yalu River to Păik-tu-San, known to Europeans as the Long White Mountain, and already visited by Messrs. James, Fulford, and Younghusband. The return journey was partly over the same ground, but on arriving at Won-san Mr. Campbell recrossed the peninsula, and so made his way to Seoul. Besides the ordinary record of this journey Mr. Campbell gives a great amount of information on various subjects connected with Corea. The chief amongst these is a most interesting section on the prevalence of Buddhism in the peninsula, and one on the agriculture and productions. He gives a good deal of information in regard to the geography of Northern Corea, and also of the gold production of the country. That Corea contains gold-bearing strata has long been known through the export of gold-dust from the ports, but from Mr. Campbell's report it appears that gold-fields do exist in considerable numbers, and that some of them are worked with the imperfect native methods. There seems no doubt that, if circumstances were favourable to the proper scientific working of the Corean gold-fields, the country would be one of the principal producers of the precious metal in the world. Education in the country seems to be at a very low ebb, and is confined to a knowledge of Chinese. All energy and enterprise is crushed out by an all-pervading tyrannical officialism, and poverty and squalor are universal.

THE new reports of the Inspectors of Sea Fisheries are interesting chiefly for the observations of Mr. Fryer on the oyster fisheries. He mentions the appearance of a curious disease in the neighbourhood of the Thames estuary, in the course of which the shells become so rotten that they will not bear the pressure necessary to open them. The oysters themselves were in good condition, but their round shells, which were muddy, were completely tunnelled in all directions, while the flat valves, which were clean, were uninjured. This points to the conclusion that the ravages were caused by some enemy working from below. The borings were not, Mr. Fryer says, those of either *Cliona* or whelk-tingle, and it seemed probable that they were the work of a minute Annelid which was present in abundance in the interstices of the shells, and in the adherent mud. In a further example sent to him in June no worms were present, although the oyster-shells were similarly undermined; but their place was taken by larvæ closely resembling, if not identical with, those of the worm *Polydora ciliata*. A means of guarding against its ravages, suggested by Mr. Fryer, is the use of an apparatus recently invented by M. Bouchon Brandély, and employed in some of the French oyster *parcs* for the purpose of facilitating the growth of oysters. This consists of a series of shallow flat baskets or trays of wire-netting on an iron frame, about 4 inches deep and 2 feet square, placed in tiers, and held together by two iron bands, the number depending on the depth of water in each case. These are either fixed to the soil, or suspended from rafts or other floating bodies, by which means depths of water otherwise inaccessible can be utilized. The other advantages claimed for the apparatus are economy of space in "planting" oysters, and of labour in collecting them, protection of the oysters from five-fingers, and from contact with unsuitable soil, and their exposure on all sides to the free circulation of the water, resulting in more rapid and regular growth, and a greater tendency to depth of shell than under the most favourable of ordinary circumstances. In the case of beds infested with the boring worm referred to, the trays in question would in all probability afford a ready means of placing the oysters beyond the reach of these marauders. The convenience of such appliances, especially in cases where French oysters are laid down temporarily on English beds, to be afterwards transferred to other grounds, *e.g.* during the winter, would probably be found to be very great.

AT a meeting of the Chemical Society held on June 18, a paper was read by Ludwig Mond and F. Quincke, on a volatile compound of iron and carbonic oxide. The authors describe experiments from which they conclude that iron forms a volatile compound with carbonic oxide of the formula $Fe(CO)_4$, corresponding to that of nickel. Very finely divided iron—obtained by reducing iron oxalate by hydrogen at a temperature but little exceeding 400°, and allowing it to cool to 80° in hydrogen—when heated in an atmosphere of carbonic oxide gave a gas which burnt with a yellow flame; and on passing the gas through a heated tube a mirror of iron was formed at between 200° and 380°, while at higher temperatures black flakes of iron and carbon were deposited. Only about 2 grams of iron, however, were volatilized after six weeks' treatment of 12 grams of the metal; it was necessary every five or six hours to interrupt the operation, and to re-heat the iron to 400° in hydrogen during about twenty minutes. When passing carbonic oxide at the rate of about 2½ litres per hour, not more than 0·01 gram of iron was volatilized, corresponding to less than 2 c.c. of the compound $Fe(CO)_4$ in a litre of gas. The authors have effected an analysis of the compound by passing the mixture of gases into mineral oil, boiling between 250° and 300°, and heating the solution so obtained to 180°; iron free from carbon is then deposited and carbonic oxide gas is evolved. Five analyses are quoted, the results of which give a ratio of Fe : CO, varying only from 1 : 4·03 to 1 : 4·264. Dr. Armstrong said that the authors' discovery was extremely interesting on account of the explanation which it might be held to afford of the permeability of iron by carbonic oxide at high temperatures, as well as to the production of steel by the cementation process, to which Graham had drawn special attention. Just as platinum was permeable by hydrogen and silver by oxygen at high temperatures, so iron was permeable by carbonic oxide; it might be supposed, in each case, because a dissociable compound of the metal with the gas was formed. Prof. Thorpe drew attention to the value of the experiments in connection with the production of steel by the cementation process, and stated that he had recently observed that platinum had the property of causing the separation of carbon from carbonic oxide. Mr. Mond said they had refrained from discussing the application of their discovery in the directions indicated, as the compound was only obtained at low temperatures. Dr. Armstrong said this might well be the case; but as Mr. Mond and Dr. Quincke had established the all-important fact that iron had a specific affinity for carbonic oxide, the argument he had used would apply, although the compound might not be sufficiently stable at high temperatures to exist alone.

THE additions to the Zoological Society's Gardens during the past week include a Chimpanzee (*Anthropopithecus troglodytes* ♂) from West Africa, presented by Major Al. McDonnell Moore; a Duyker Bok (*Cephalophus mergens* ♂) from South Africa, presented by Mr. A. Barsdorf; five West Indian Agoutis (*Dasyprocta antillensis*) from Jamaica, presented by the Board of Governors of the Institute of Jamaica; a Spotted Cavy (*Cœlogenys paca*) from Guiana, presented by Mr. R. Kirk; two Slow Loris (*Nycticebus tardigradus*), a Javan Fish-Owl (*Ketupa javanensis*) from Java, presented by Mr. R. Dixon; an Orange-cheeked Waxbill (*Estrelda melpoda*), a Zebra Waxbill (*Estrelda subflava*) from West Africa, a Nutmeg Finch (*Munia punctularia*) from India, presented by Mrs. Harris; a Chattering Lory (*Lorius garrulus*) from Moluccas, presented by Miss Alice Dundas; a Common Viper (*Vipera berus*), British, presented by Mr. W. H. B. Pain; four Grey Parrots (*Psittacus erithacus*) from East Africa, deposited; a Thar (*Capra jemlaica*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

LUMINOUS OUTBURST OBSERVED ON THE SUN.—*Comptes rendus* for June 22 contains the information that on June 17, at 10h. 16m. Paris mean time, M. Trouvelot observed a luminous outburst on the sun, apparently of the same character as that witnessed by Carrington and Hodgson in 1859 (*Monthly Notices R.A.S.*, vol. xx. pp. 13–16). A luminous spot subtending an angle of 3° appeared near the western limb of the sun (position-angle 281°). It had not the characteristic white colour of faculæ, but was yellowish, and strikingly resembled the light emitted by incandescent lamps shortly before they reach their maximum brilliancy. M. Trouvelot's first impression was that an opening at the eye-piece allowed a ubiquitous sunbeam to fall upon the screen upon which the sun's image was being projected, but an examination proved that the phenomenon was truly solar. In fact, shortly after the time of the first observation, a similar brilliant object subtending an angle of about 5° or 6° appeared slightly to the north of the first, its position-angle being about 289°. By means of spectroscopic observations it was found that the first object consisted of a central eruption from which a species of incandescent volcanic bombs were thrown to heights of 2′ or 3′ above the chromosphere, where they rested as if suspended, and appeared as dazzling globes on the red background on which they were projected. A few minutes later the sparkling balls were replaced by numerous brilliant filaments or jets, which at 10h. 24m. were shot out to a height of 5′ 24″. In spite of the vivid light of this prominence only a few lines in the spectrum were seen to be reversed. In addition to the lines C, D_3, F, and G, which were all extremely bright, the line at λ 6676·8, the *b* group, and a line about λ 4394·8, were seen bright. The sodium lines, D_1 and D_2, showed no indication of reversal. Considerable displacements of the C line towards both ends of the spectrum were observed. On the following day at 9h. 30m. the eruption was still very apparent, but diminishing in activity, and at 2h. 45m. all signs of an eruptive prominence had disappeared. The striking character of the outburst led M. Trouvelot to suggest that it might be accompanied by a simultaneous terrestrial magnetic perturbation. This was not the case, however, for after examining the records obtained at Kew Observatory, Mr. Whipple writes that there was not the slightest magnetic disturbance on the dates when the eruption was observed.

LORD HARTINGTON ON TECHNICAL EDUCATION.

THE fourth annual meeting of the National Association for the Promotion of Technical and Secondary Education took place on Friday last at 14 Dean's Yard, Westminster. Lord Hartington, President of the Association, occupied the chair. He said:—

In opening the proceedings it will be, fortunately, unnecessary for me to trouble you with more than a very few brief observations. It has not been considered necessary to make any attempts to obtain a very large attendance to-day, or to meet in any place where we could have a meeting on the scale of others which we have had on this subject in previous years, not but that we have arrived at a very important epoch in the development of the objects for which this Association was founded four or five years ago. It may, perhaps, be desirable for me, in the first place, to call your attention and the attention of the public to the special objects for which this Association has been founded, as I think there is in some quarters some misapprehension as to the practical nature of the objects which we have in view. As is stated in the report, its object has not been to interfere with the teaching of trades in workshops, or with the industrial and commercial training in the manufactory and in the warehouse. It desires, first of all, to develop increased general dexterity of hand and eye among the young, which may be especially useful to those who have to earn their own livelihood, and at the same time improve rather than hinder their general education; secondly, to bring about more widespread and thorough knowledge of those principles of art and science which underlie much of the industrial work of the nation; and, thirdly, to encourage better secondary instruction generally, which will include more effective teaching of foreign languages and science, for those who have to guide our commercial relations abroad and to develop our interests at home. Now, those are the objects to carry out which this Association was founded. At the time when it was first originated, these objects were very little recognized in any quarter. They were not recognized as in any degree the duties of the State, except to a very limited extent, so far as the operations of the Science and Art Department were concerned. But, useful and valuable as has been the teaching carried on under the guidance and direction of the Science and Art Department up to a very short time ago, I think it may be said that scarcely any attempt had been made to give to that teaching a practical application, or to apply its instruction to the advancement and improvement of the industries of the country. Well, the absence of any State recognition was not to any large extent supplied at that time by private efforts. It is quite true that a few manufacturers in different parts of the country had set the very useful example of establishing, in connection with their works, some technical and scientific teaching. There were also a few institutions, such as the well-known Polytechnic Institution here, others in the City and in various other parts of the country, which were making attempts to give instruction with the objects which I have just enumerated, but those efforts were rather of a philanthropic than of a practical character, and they had not four or five years ago attained a very large or extensive development. Well, we may look back now at those years as years of very great and very satisfactory progress. I will not say all that has been done has been done in consequence of the exertions of this Association. Certainly these objects have been greatly advanced since the foundation of the Association, and, we flatter ourselves, to a certain extent in consequence of the efforts of the Association. But whether the progress that has been made has been in consequence of, or independent of, any exertion of ours, it is equally a matter of congratulation that progress has been made. In the first place, those objects to which I have already referred have been recognized by Parliament as proper objects to receive assistance, by means of public funds, in the shape of the application to them of the rates. By the Technical Instruction Act, which was passed in 1889, mainly at the instance of some active Parliamentary representatives of this Association, that principle was for the first time admitted; but a very much greater step was taken in the next year, 1890, when, under the Local Taxation Act, a sum very nearly approaching £750,000 for England and Wales was placed at the disposal of local authorities, mainly for the objects which this Association has in view. It is quite true that the application of that sum was to a large extent optional. It would have been in the power of local authorities in whose hands it was placed to apply it in aid of the rates or to other purposes, but the efforts of the Association were directed, as I think I shall be able to show you, with very great success, in order to secure the appropriation of these large funds to the purposes of practical technical instruction. You will recollect that in the winter of last year—I think in December—an important conference was held under the direction of the Executive Committee of this Association at the rooms of the Society of Arts, in which members representing County Councils in various parts of the country entered into conference and discussion with the Executive Committee of this Association. Information was given as to what had already been done by certain County Councils which had taken the lead, and suggestions were made as to the manner in which other Councils could most usefully follow their steps and devote these sums to the purposes for which we believe they were intended by Parliament. The results which have already been accomplished are recorded in the report of this Association, which will be immediately circulated. Of County Councils in England, excluding Monmouthshire, 37 have already decided to give the whole of this grant for the purposes of technical instruction; 8 have decided to give a part of this grant for the same purposes, and 2 only have decided to apply the whole of it in aid of the rates. In Wales and Monmouthshire 11 County Councils have given the whole to education, and 2 have given a part to the same purpose. Of the county boroughs in England, 33 have devoted the whole of the funds to educational purposes, and 3 have devoted a part to the same objects. In Wales 2 county boroughs have devoted the whole of the fund to education, and none to any other purposes. With regard to 23 county boroughs, either we have not sufficient information, or they have not yet arrived at a conclusion upon the subject. Well, that appears to us to be an extremely encouraging result so far as it has gone. The exertions of the Executive Committee have not, however, been entirely confined to securing this appropriation of the funds placed at the disposal of the Councils by Parliament. The same gentlemen who have taken the lead in the matter from the beginning—I refer chiefly to my friend Sir Henry Roscoe, Mr. Acland, Mr. Hobhouse, and others—have obtained from Parliament additional legislation considerably extending and developing the principle which for the first time received the assent of Parliament in 1889. I think it is hardly necessary that I should give further information as to the effect of the amending Act of this session. I prefer to leave the gentlemen I have named to give that explanation. But I desire, however, to point out that the work of this Association, which has been so successfully begun, has not by any means yet ended. The application of these grants in the various localities is, of course, a work of great variety and of the utmost importance. Fortunately, I think, the State has not undertaken, except under very wide conditions, to exercise any supervision over the application of these funds. In a country possessing industries of so extremely varied a character as ours, it would have been almost impossible, and I think certainly would have been most undesirable, that any cut-and dried system should be adopted by which one identical, application of public money to purposes of technical instruction should be adopted all over the country. The application of these funds must vary very greatly in agricultural districts, and in agricultural districts themselves as between arable and dairy or cheesemaking districts. It must vary in those districts which are chiefly devoted to cotton and woollen industries, and those which are chiefly employed in the coal-mining, metal, or chemical trades; and in almost every different county of England a different application of those resources would have been required. I think very wide discretion has been very wisely left by Parliament to the local authorities themselves, which are in this instance County Councils or county borough councils. And these Councils have again adopted the wise course of appointing committees to prepare schemes for the approval of the Councils for the application of these grants. The work was, of course, very new to a great many who had to take it up, and this Association has been able, we think, to give valuable assistance to them, both by affording information and giving advice, and, above all, by providing the means of communication between those who are interesting themselves in this work in various parts of the country, to enable them to know what other authorities were doing, what difficulties were found, what means had been found of surmounting those difficulties, and of generally taking counsel and acting together in co-operation. Now, the subject of agricultural education, which up to a very short time ago had been almost entirely neglected, has been by many County Councils vigorously taken up. Courses of instruction in elementary science applying to agricultural pursuits have been instituted, and also instruction of a still more practical character, in the shape of travelling dairies and other instruction of the same kind, has been given in many places. I am glad to say that the two great Universities of Oxford and Cambridge have also turned their attention to this important subject, and both of them are preparing to take steps by which the teachers who will be so much required in order to give effect to the desire of the County Councils to improve the agricultural education of their districts will be provided. I am glad to say also that the important subject of the technical education of girls as well as boys is receiving almost universal attention from County Councils. Suggestions have been made by this Association, which have in most cases received attention, to provide not only for the instruction of the boys, but also of the girls, in such subjects as cookery, laundry-work, and dairy management. In all these matters the Association has been able to give some assistance, and we believe that there remains a great deal still in which they will be able to afford the same nature and description of assistance. I need not say, ladies and gentlemen, that for a very considerable time the work which is likely to be thrown upon this Association will be work which cannot be conducted without considerable financial resources. The income of the Association is not a very large one. We have made an appeal to many of those who throughout the country have interested themselves in this work in connection with County Councils, and we have received very liberal assistance. I think, however, the time has come when we may hope that the efforts which have been made will be to a certain extent, still more than they have hitherto been, supplemented by the assistance of gentlemen connected with the great manufacturing, mining, and commercial industries of the country, who are likely, I think, to derive at least as much benefit from the operations of this Association, and from the development which it has aided in giving technical instruction throughout the country, as the agricultural industry has already received. Ladies and gentlemen, I must apologize to you for the imperfect character of these observations, which I have been obliged to condense as much as possible, as my time, and I dare say yours, is extremely limited. I only hope that any omissions which I have made will be supplied by my friends who are on each side of me.

Sir H. Roscoe, M.P., presented the report of the work of the Association during the past year. He said that there was no doubt that during the year a very great expansion of the work of the Association had been seen under both the Acts of Parliament to which reference had been made by the Chairman. The spread of technical education throughout the country had been most remarkable. From what had already been said by Lord Hartington, it would be concluded that practically the whole of England had devoted the whole of the money to technical instruction. The effects of this could scarcely be over-estimated. The only two places where the money had been devoted to the relief of the rates were, he regretted to say, London and Middlesex. But it should be borne in mind that what had been already accomplished was nothing to what remained to be done. The County Councils were as yet only breaking the ground. Their efforts were merely tentative. They had, as it were, to work out their own salvation in this matter of education, and there was certain to be at no great distance of time an Intermediate Education Act for England. Referring to the Act of 1891, he said that it was important and valuable because it enabled a County Council to go out of its own district if it thought necessary to promote technical education. Under that Act, for instance, the three Ridings of Yorkshire had been able to vote money to assist the Yorkshire College in its scheme for the improvement of agricultural education. Many of the County Councils had already appointed organizing secretaries, and it was on these that the main part of the work would fall. To them they had to look for the special organization of each particular district, and the importance of their work could scarcely be overrated. Then in the county boroughs the work was being got into shape. In Sheffield a sum of £8495 had been appropriated towards assisting institutions giving technical and secondary education. In the same way in Manchester £10,200 had been devoted to a like purpose. Agricultural education was making rapid progress, and already in Yorkshire, Durham, and Wales there was the nucleus of high class agricultural colleges. After referring to the necessity of some part of the money being devoted to the technical instruction of girls, he concluded by expressing the hope that the Association would be placed in a position by an increase of its resources to carry on actively a work that was daily becoming more important and more costly.

On the motion of Mr. H. Hobhouse, M.P., seconded by Lord Thring, the report was unanimously adopted.

Lord Hartington at this point left the chair, which was taken by Sir Bernhard Samuelson.

Lord Monteagle moved the reappointment of the vice-president, executive committee, and officers of the Association, the name of Mr. Bryce, M.P., being substituted for that of the late Earl Granville. Dr. Gladstone seconded, and Mr. Snape supported, the motion, which was carried unanimously.

Mr. Bryce, M.P., proposed the following resolution :—

"That this Association heartily congratulates the County Councils of England and Wales on the great progress they have made during the past year in the promotion of education in their districts, and earnestly trusts that they will continue to work until the country is provided with an organized system of secondary and technical education."

Miss Hadland seconded the resolution, which was agreed to.

Sir John Lubbock, M.P., proposed, and Mr. Rathbone, M.P., seconded, a vote of thanks to the Chairman, and this having been heartily accorded was acknowledged by Sir Bernhard Samuelson.

The proceedings then terminated.

SCIENTIFIC SERIALS.

IN the *Journal of Botany* for May, Prof. R. J. Harvey-Gibson has an interesting article, illustrated, on the histology of *Polysiphonia fastigiata*. In the June number, Mr. A. W. Bennett contributes a short paper on sexuality among the Conjugatæ. These numbers also contain continuations of Mr. E. G. Baker's synopsis of the genera and species of Malveæ, and of the Rev. H. G. Jameson's useful key to the genera and species of British mosses.

THE papers in the *Botanical Gazette* for April and May are concerned almost exclusively with American botany. Mr. D. M. Mottier has an interesting note on the apical growth of Hepaticæ, which bears such a striking resemblance to that of the prothallium of ferns.

THE number of the *Nuovo Giornale Botanico Italiano* for April is chiefly occupied by papers of special interest to Italian botanists, and by the Bulletin of the Italian Botanical Society. Among the articles coming under the latter head is one by Sig. Baccarini on the secretory system of the Papilionaceæ, and one by Sig. Pichi containing an account of experiments on the parasitism of *Peronospora* on the vine.

THE *Botanical Magazine* of Tokyo still contains occasional articles in the English language. Those in the numbers most recently received, for March and April, relate to the native plants of Japan.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 11.—"A Study of the Planté Lead—Sulphuric Acid—Lead Peroxide Cell, from a Chemical Standpoint. Part I." By G. H. Robertson. Communicated by Prof. Armstrong, F.R.S.

The investigation, the results of which are recorded in this paper, was instituted about a year ago at the Central Institution, at Dr. Armstrong's suggestion, as McLeod's observations on the electrolysis of sulphuric acid solutions led to the supposition that the changes occurring in the acid were probably less simple than was commonly supposed. This supposition was verified.

The first section of the paper deals with the nature of the lead salt formed during discharge. Experiments made on various samples of red lead of different percentage composition showed that, as with nitric, so with sulphuric acid, it behaved like a mixture of peroxide and monoxide, the sulphate formed always corresponding to the monoxide originally present.

As analysis alone can give no proof of the existence of a definite homogeneous sulphate corresponding to red lead; evidence must be obtained that the product differs in some of its properties from a mixture. It was to be expected that the E.M.F. of an oxysulphate would differ from a corresponding mixture of sulphate and peroxide, and have some definite value, but experiments made with mixtures of sulphate and peroxide in different proportions, and with the product obtained by treating red lead with dilute sulphuric acid, showed that there was a difference of degree only between the red lead pastes and the mixtures.

With regard to Frankland's observations respecting the colour of the product formed on the peroxide plate during discharge, and the reducibility of the sulphate, the author points out that the colour is due to the incomplete reduction of the peroxide; and that careful examination of the plugs from a discharged cell shows that the base consists of practically unaltered peroxide of lead, and that the surface, which is rich in $PbSO_4$, is really a mass of partially reduced granules of peroxide of lead which are coated with sulphate.

Also, though pure lead sulphate is very difficult to reduce, it is well known that mixtures of lead sulphate and peroxide of lead, or other conducting substances, are reduced with comparative ease, and that it is very intimate mixtures of this nature which have to be dealt with as a rule in charging a cell.

In conclusion, the author points out—

That neither chemical nor electrical tests give any ground for supposing that any other sulphate than the ordinary white $PbSO_4$ is concerned in the interactions occurring in the cell;

That were the sudden lowering of the E.M.F. caused by a change in the nature of the chemical compounds formed on the plates, it is very difficult to account for the very rapid recovery of the E.M.F. exhibited by an apparently discharged cell.

In the second section the electrolyte is dealt with, and, after referring to the work of Berthelot, Richarz, Schöne, Traube, and others on the electrolysis of sulphuric acid solutions, the author describes experiments made to test the effect of the addition of sodium sulphate to the electrolyte, as recommended by Mr. Barbour Starkey, as it seemed probable it had a catalytic action on the "peroxides" always found in electrolyzed acid of the strength used in batteries.

Mr. Preece most kindly aided the investigation by allowing experiments to be carried out at the General Post Office, where one-half of the secondary cells contain 1 per cent. of sodium sulphate, and the other half ordinary dilute acid, sp. gr. 1180. It was found that the addition of sodium sulphate in about the proportion of 1 per cent. to freshly electrolyzed acid, or during electrolysis, always produced a diminution in the total quantity of "active oxygen," and brought the amount present in the plain cells down almost exactly to that found in the sodium sulphate cells.

Determinations were made of the amounts of "active oxygen" present as persulphuric acid and hydrogen dioxide respectively; and it was established that acid taken from the cell reduced peroxide of lead. The presence of hydrogen dioxide being thus established both directly and indirectly, its effect on the E.M.F. of a cell was tested. It was found that, while its addition to the acid in the case of a lead lead-peroxide couple in dilute sulphuric acid produced an annulment, or reversal, of the E.M.F., the introduction of hydrogen dioxide into the body of the peroxide paste produced an increase in the E.M.F. in the case of a platinum lead-peroxide couple.

The Post Office records showed that, while the general character of the temperature and specific gravity changes occurring during charge and discharge were the same in both types of cell, there was less sulphating with the sodium sulphate electrolyte.

The cause of the pink colour of the acid, noticed by Mr. Crompton and others, was investigated, and found to be permanganic acid, formed probably from the manganese present in commercial lead.

In conclusion, the author points out—

That peroxides are found in appreciable quantities in the electrolyte during charge and discharge;

That their influence must not be neglected in considering the behaviour of the Planté cell;

And that it is to the electrolyte, rather than to the plates, that attention must be directed if any considerable improvement is to be effected.

"Part II.—A Discussion of the Chemical Changes occurring in the Cell." By H. E. Armstrong, F.R.S., and G. H. Robertson.

The authors arrive in this paper at the following conclusions:—

(1) That the cooling observed in the Planté cell can only be explained as resulting from the dissociation of the dilute sulphuric acid; and as the values given by Messrs. Ayrton, Lamb, Smith, and Woods are in practical agreement with those calculated on the assumption that the acid used is sulphuric acid itself, H_2SO_4, that in all probability such acid, and not the dilute acid contained in the cell, is operative throughout.

(2) That the observed loss in efficiency cannot be due to temperature changes, as these arise through actions occurring out of circuit.

(3) That it is difficult, from a comparison of calculated with observed values of the E.M.F., to arrive at any final conclusion as to the exact nature of the changes which take place in the cell. On the assumption that sulphating occurs at both plates in circuit, and under the influence of H_2SO_4, the calculated value is considerably too high; while, if sulphating occur only at the lead plate, the value calculated is far too low.

(4) That a counter E.M.F. of about 0·5 volt would account for the observed departure from the highest calculated value. As peroxides are always present in the electrolyte, it is conceivable that such a counter E.M.F. may exist; moreover, there is also the possible influence of the lead support to be considered.

(5) That the observed loss of efficiency is to be attributed to the formation of peroxides in the electrolyte, and to the excessive sulphating occurring chiefly at the peroxide plate in the local circuit existing between the support and the paste.

June 18.—"Comparison of Simultaneous Magnetic Disturbances at several Observatories, and Determination of the Value of the Gaussian Coefficients for those Observatories." By Prof. W. Grylls Adams, D.Sc., F.R.S., Professor of Natural Philosophy in King's College, London.

After drawing attention to previous investigations on this subject, and pointing out the importance of adopting the same scale values for similar instruments at different Observatories, especially at new Observatories which have been recently established, the discussion of special magnetic disturbances is undertaken, especially the disturbances of a great magnetic storm which occurred on June 24 and 25, 1885, for which photographic records have been obtained from 17 different Observatories: 11 in Europe, 1 in Canada, 1 in India, 1 in China, 1 in Java, 1 at Mauritius, and 1 at Melbourne.

The records are discussed and compared, tables are formed of the simultaneous disturbances, and the traces are reduced to Greenwich mean time and brought together on the same plates arranged on the same time-scale. Plates I. and II. show the remarkable agreement between the disturbances at the different Observatories, and the tables show that the amount of disturbance, especially of horizontal magnetic force, is nearly the same at widely distant stations.

An attempt has also been made to apply the Gaussian analysis to sudden magnetic disturbances, and, with a view to their application in future work, the values of the Gaussian coefficients have been obtained for 20 different Observatories, and the numerical equations formed for the elements of magnetic force in three directions mutually at right angles, and also the equation for the magnetic potential in terms of the Gaussian constants to the fourth order.

The tables give the numerical values to be multiplied by the 24 Gaussian constants to give the values of the forces X, Y, and Z in the geographical meridian towards the north, perpendicular to the meridian towards the west, and towards the earth's centre respectively. The equations are also formed and the values obtained in terms of the 24 Gaussian constants for X_2, Y_2, and Z_2; X_2 being the horizontal force in the magnetic meridian, Y_2 the horizontal force perpendicular to the magnetic meridian, and Z_2 the vertical force. If then X_2, Y_2, and Z_2 be the observed values of any simultaneous disturbances, they may be at once substituted in the equations, the equations giving the 24 Gaussian constants may be solved, and the corresponding change of magnetic potential may be determined.

Physical Society, June 12, 1891.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Prof. W. G. Adams took the chair whilst Prof. Ayrton read a paper on alternate current and potential difference analogies in the methods of measuring power, by himself and Dr. Sumpner. In a paper read before the Society in March last, the authors pointed out that, for every method of measuring power in which readings of volts and amperes were taken, other methods in which amperes were read instead of volts, and volts instead of amperes, could be devised. More recently, Dr. Fleming had, by a transformation of a formula given by the authors in a communication made to the Royal Society on the measurement of power by three voltmeters, given the analogue in which three ammeters were employed. The two arrangements are represented in Figs. 1 and 2, whilst Fig. 3 shows a modification of Dr. Fleming's method (Fig. 2), in which the current in the non-inductive resistance *r* is measured by the aid of a voltmeter V across its terminals. This obviates the necessity of putting an electro-magnetic instrument in what should be a non-inductive circuit. The formula for the mean watts spent in the circuit *ab*, Figs. 1 and 2, are respectively—

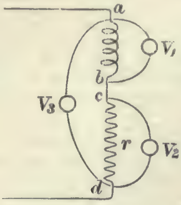

FIG. 1.

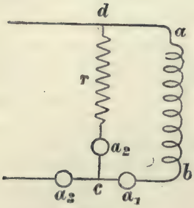

FIG. 2.

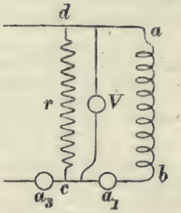

FIG. 3.

$$W = \frac{1}{2r}\left(V_3^2 - V_1^2 - V_2^2\right), \text{ and } W = \frac{r}{2}\left(A_3^2 - A_1^2 - A_2^2\right).$$

Mr. Blakesley's method of measuring power by a split-dynamometer was shown to be analogous to the original electrometer method in which the difference of two readings was proportional to the power, and Blondlot and Currie's double electrometer method was shown to be the analogue of the ordinary wattmeter. The wattmeter was defective in the fact that a solenoidal coil was inserted in a nominally non-inductive circuit. The error thus introduced is, as was shown by one of the authors some years ago, expressed by the formula—

$$\frac{\text{Apparent watts}}{\text{True watts}} = \frac{1 + \tan\theta \,.\, \tan\phi}{1 + \tan^2\phi},$$

where θ is the phase angle between the current and E.M.F. in the circuit in which the power is to be measured, and ϕ the phase angle for the approximately non-inductive circuit. It is now proved that the same formula expresses the error in any of the methods where resistances not wholly non-inductive are used. As is well known, Mr. Blakesley has applied his split-dynamometer to the measurement of phase differences between two currents; and an analogous method of finding the phase difference between two potential differences is described in the paper. In this method a high resistance split-dynamometer such as suggested by Mr. Rimington for measuring power is employed. Blondlot and Currie's double electrometer could also be used for the same purpose. Numerous diagrams illustrating the various analogies accompany the paper. Prof. S. P. Thompson inquired whether hot-wire voltmeters could be employed to measure the various potential differences, without introducing error. In reply, Prof. Ayrton said that, although no great error was introduced by the self-induction of these instruments, yet the fact that they required considerable current was a disadvantage, and as these currents were not always in the same phase as those in other circuits, troublesome corrections were sometimes necessary. Electrostatic instruments were preferable. Prof. Adams said he was glad to hear that the inductance of Cardew voltmeters introduced no serious error, for they were very convenient instruments to use.—Prof. O. Lodge, F.R.S., exhibited and described a clock for pointing out the direction of the earth's orbital motion in the ether. After mentioning the various motions to which a point on the earth's surface is subjected, he pointed out that the orbital motion was the largest component, and its direction at any instant not easy to conceive. An apparatus for pointing out this direction was therefore convenient when dealing with problems requiring a knowledge of the motion of a point through the ether. In one of two clocks shown, one spindle representing the earth's polar axis and another the axis of the ecliptic were inclined at an angle of 23½°, and coupled by a Hooke's joint. The latter axis was capable of rotating round the former. At its upper end the ecliptic axis carried a tube and a pointer, both being perpendicular to the axis and to each other. The clock keeping solar time rotated both axes, and when properly set the tube pointed in the direction of the sun, and the pointer therefore indicated the direction of the earth's orbital motion.—Some experiments with Leyden jars were then shown by Dr. Lodge. The first one was with resonant jars, in which the discharge of one jar precipitated the overflow of another, when the lengths of the jar circuits were properly adjusted or tuned. The latter jar was entirely disconnected from the former, and was influenced merely by electro-magnetic waves emanating from the discharging circuit. Lengthening or shortening either circuit prevented the overflow. Correct tuning was, he said, of great importance in these experiments, for a dozen or more oscillations occurred before the discharge ceased. The effect could be shown over considerable distances. In connection with this subject Mr. Blakesley had called his attention to an observation made by Priestley many years ago, who noticed that, when several jars were being charged from the same prime conductor, if one of them discharged the others would sometimes also discharge, although they were not fully charged. This he (Dr. Lodge) thought might be due to the same kind of influence which he had just shown to exist. The word *resonance*, he said, was often misunderstood by supposing it always had reference to sound, and as a substitute he thought that *symphoning* or *symphonic* might be allowable. The next experiment was to show that wires might be tuned to respond to the oscillation of a jar discharge just as a string could be tuned to respond to a tuning-fork. A thin stretched wire was connected to the knob of a jar and another parallel one to its outer coating, and by varying the length of an independent discharging circuit, a glow was caused to appear along the remote halves of the stretched wires at each discharge. Each of the wires thus acted like a stopped organ-pipe, the remote ends being the nodes at which the variations of pressure were greatest. By using long wires he had observed a glow on portions of them with the intermediate parts dark; this corresponded with the first harmonic, and by measuring the distance between two nodes he had determined the wave-length of the oscillations. The length so found did not agree very closely with the calculated length, and the discrepancy he thought due to the specific inductive capacity of the glass not being the same for such rapidly alternating pressures as for steady ones. He also showed that the electric pulses passing along a wire could be caused (by tuning) to react on the jar to which it was connected, and cause it to overflow even when the distance from the outside to the inside coating was about 8 inches. During this experiment he pointed out that the noise of the spark was greatly reduced by increasing the length of the discharging circuit. The same fact was also illustrated by causing two jars to discharge into each other, spark gaps being put both between their inner and outer coatings so as to obtain "A" sparks and "B" sparks. By putting on a long "alternative path" as a shunt to the B spark gap and increasing that gap, the noise of the A spark was greatly reduced. He had reason to believe that the B spark was a quarter phase behind the A spark, but the experimental proof had not been completed. He next described some experiments on the screening of electro-magnetic radiation, in which a Hertz resonator was surrounded by different materials. He had found no trace of opacity in insulators, but the thinnest film of metal procurable completely screened the resonator. Cardboard rubbed with plumbago also acted like a nearly perfect screen. In connection with resonators, he exhibited what he called a *graduated electric eye* or an *electric harp*—made by his assistant, Mr. Robinson—in which strips of tin foil of different lengths are attached to a glass plate, and have spark gaps at each end which separate them from other pieces of foil. One or other of the strips would respond according to the frequency of the electro-magnetic radiation falling upon it. Mr. Blakesley asked whether the pitch of the resonant jars altered when the distance between their circuits was varied, for according to theory the mutual induction should diminish the self-induction, and cause the oscillations to be more rapid. If this occurred, the method might be used for getting rapid oscillations. He also inquired whether the glow would appear in the same position on the two stretched wires if their ends were joined. Dr. Sumpner wished to know how the resistances, inductances, and capacities of the circuits and jars were determined, and whether any evidence of irregular distribution of the charges on the tin-foil had been noted. With reference to the overflowing of a jar caused by using a certain length of discharging circuit, he asked whether the overflow did not prove the existence of a higher potential than that which originally existed between the coatings of the jar, and, if so, where did the excess energy come from? Dr. Thompson asked if it would be possible to make a wire circuit analogous to an open organ-pipe by putting sheets of metal on the ends of the wires. Dr. Lodge, in reply, said Mr. Blakesley's suggestion was an important one, but he had not observed that any change in the adjustment was necessitated by varying the distance between the resonating circuits. Neither had he noticed any glow on wires joined to form a single loop, but this might be possible if the wires were long enough to give harmonics. In answer to Dr. Sumpner he said that the capacities were difficult to determine, for with such rapid oscillations the coatings were virtually enlarged. Lord Rayleigh had shown how to calculate the inductances, and the resistances he had practically measured by his alternative path experiments. The overflow of jars he thought was caused by the charges in some way concentrating on the edges of the foil, thus causing a kind of flood tide, at which the overflow occurred. The President asked Dr. Lodge what his views were as to the cause of the opacity of ebonite to light. Was it due to a selective absorption which cut off only the rays to which the eye was sensitive, or was the ordinary explanation, that it contained impurities which were conducting, and hence acted as screens, likely to be correct? Another possible explanation was that the motion of the ether particles may be in three dimensions, and light be due to the projection of this motion on a plane perpendicular to the ray, whilst electromagnetic induction might be due to the other component. Dr. Lodge said he believed that ebonite was not opaque because of conducting particles being present, and was inclined to think that it acted more like ground glass, in which the opacity was due to internal reflections. Such a substance would only be opaque to vibrations whose wave-lengths were comparable with the size of the particles.—A note on the construction of non-inductive resistances, by Prof. W. E. Ayrton, F.R.S., and Mr. T. Mather, was postponed until next meeting.

Zoological Society, June 16.—Dr. St. George Mivart, F.R.S., Vice-President, in the chair.—Mr. H. A. Bryden exhibited an abnormal pair of horns of a cow Eland obtained in the North Kalahari, and made remarks on the structure of the feet of the Lechée Antelope.—Mr. Howard Saunders exhibited and made remarks on a nearly white skin of a Tiger obtained in Northern India by Major D. Robinson.—Mr. Saunders also exhibited specimens of the eggs of a Gull (*Larus maculipennis*) and of a Tern (*Sterna trudeaui*) from Argentina.—Mr. Sclater read an extract from a letter received from Dr. Bolau, C.M.Z.S., describing two Sea-Eagles living in the Zoological Garden, Hamburg, and considered to be referable to Steller's Sea-Eagle (*Haliaëtus pelagicus*). One of these, received from Corea, Mr. Sclater pointed out, probably belonged to the species described in the Society's Proceedings by Taczanowski as *Haliaëtus branickii*.—Dr. R. Bowdler Sharpe gave a short account of the proceedings of the International Ornithological Congress recently held at Budapest, in which he had taken part.—Mr. G. A. Boulenger read a paper entitled "A Contribution to our Knowledge of the Races of *Rana esculenta* and their Geographical Distribution." Mr. Boulenger proposed to recognize four forms of this widely-spread species of Frog, and pointed out the characters upon which these races were based and the areas which they occupy.—Mr. Oldfield Thomas read some notes on various species of Ungulates, which he had made during a recent examination of the specimens of this group of Mammals in the British Museum.—Mr. Edgar A. Smith gave an account of a large collection of Marine Shells from Aden. To this were added some remarks upon the relationship of the Molluscan Fauna of the Red Sea with that of the Mediterranean.—A second communication from Mr. Smith contained descriptions of some new species of Shells, based on examples obtained during the *Challenger* Expedition.—Mr. H. A. Bryden read some notes on the present distribution of the Giraffe south of the Zambesi, and made some remarks on the best means of procuring living specimens of this animal for European collections.—A communication was read from Messrs. Mole and Urich containing notes of some of the Reptiles of Trinidad, of which they had transmitted living examples to the Society's Menagerie.—Mr. F. E. Beddard read some additional notes upon the anatomy of *Hapalemur griseus*, made during a recent examination of two specimens of this Lemur.—Mr. E. B. Poulton gave an account of an interesting example of protective mimicry discovered by Mr. W. L. Sclater in British Guiana. This was an immature form of an unknown species of Homopterous insect of the family Membracidæ, which mimics the Cooshie Ant (*Œcodoma cephalotes*).—This meeting closes the present session. The next session (1891-92) will begin in November next.

Royal Microscopical Society, June 17.—Dr. R. Braithwaite, President, in the chair.—The President said he regretted to announce the death of Prof. P. Martin Duncan, who as a past President of the Society, was well-known to the Fellows.—A negative of *Amphipleura pellucida*, produced with Zeiss's new $\frac{1}{16}$ of 1·6 N.A. and sunlight, by Mr. T. Comber, of Liverpool, was exhibited, and his letter was read suggesting that the want of sharpness was due to the employment of a projection eye-piece for a tube-length of 160 mm., whereas the objective was made for a tube-length of 180 mm. The illumination was axial with a Zeiss achromatic condenser of 1·2 N.A. Mr. Comber thought the resolution showed indications of so-called "beading," and he inferred that the ultimate resolution would be similar to that of *Amphipleura lindheimeri*. The mounting medium had a refractive index of 2·2, but was very unstable, granulations appearing in a very short time.—Mr. C. L. Curties exhibited Mr. Nelson's apparatus for obtaining monochromatic light. Mr. Mayall said the apparatus was so devised that the microscopist might employ any prism or photographic lens he possessed. If a prism was made specially, one of light crown-glass would probably answer better than the dense flint.—Mr. T. T. Johnson exhibited a new form of student's microscope which he had devised. Mr. Mayall said the special point was the application of a screw movement to raise and lower the substage, the screw being in the axis of the bearings of the substage and tailpiece; and the position of the actuating milled head, which projected slightly at the back of the stage, seemed to be most happily chosen.—Dr. J. E. Talmage, of Salt Lake City, Utah, U.S.A., a newly elected Fellow, having been introduced by the President, read a note on the occurrence of life in the Great Salt Lake, and exhibited some specimens of *Artemia fertilis* from the lake.—Prof. Bell said a paper was read at the February meeting, in which Dr. Benham described a new earthworm under the name of *Eminia æquatorialis*. The name *Eminia* having been already given to a bird by Dr. Hartlaub, Dr. Benham proposed to re-name the earthworm *Eminodrilus*.—A letter from Dr. Henri Van Heurck was read, replying to the criticisms of his microscope delivered at the previous meeting. A discussion followed, in which Mr. Mayall, Dr. Dallinger, and Mr. Watson joined.—Mr. T. D. Aldous exhibited the eggs of a water-snail which were attacked by a parasite which seemed to be destroying the gelatinous matter to get at the eggs.

Royal Meteorological Society, June 17.—Mr. Baldwin Latham, President, in the chair.—Mr. A. J. Hands gave an account of a curious case of damage by lightning to a church at Needwood, Staffordshire, on April 5, 1891. The church was provided with a lightning-conductor, but Mr. Hands thinks that when the lightning struck the conductor, a spark passed from it to some metal which was close to it, and so caused damage to the building.—Mr. W. Ellis read a paper on the mean temperature of the air at the Royal Observatory, Greenwich, as deduced from the photographic records for the forty years from 1849 to 1888, and also gave some account of the way in which, at different times, Greenwich mean temperatures have been formed.—Mr. Ellis also read a paper on the comparison of thermometrical observations made in a Stevenson screen with corresponding observations made on the revolving stand at the Royal Observatory, Greenwich. From this it appears that the maximum temperature in the Stevenson screen is lower than that of the revolving stand, especially in summer, and the minimum temperature higher, whilst the readings of the dry and wet bulb thermometers on both the screen and the stand, as taken at stated hours, agree very closely together.—Mr. W. F. Stanley exhibited and described his phonometer, which is really a new form of chronograph, designed for the purpose of ascertaining the distance of a gun from observations of the flash and report of its discharge, by the difference of time that light and sound take in reaching the observer. The instrument can also be used for measuring the distance of lightning by timing the interval between the flash and the report of the thunder.—A paper was also read by Mr. A. B. MacDowall, on some suggestions bearing on weather prediction.

Geological Society, June 24.—Sir Archibald Geikie, F.R.S., President, in the chair.—The following communications were read:—On wells in West Suffolk boulder-clay, by the Rev. Edwin Hill. It might be supposed that in a boulder-clay district water could only be obtained from above or from below the clay. But in the writer's neighbourhood the depths of the wells are extremely different, even within very short distances; and since the clay itself is impervious to water, he concludes that it must include within its mass pervious beds or seams of some different material which communicate with the surface. It would follow that this boulder-clay is not a uniform or a homogeneous mass. The visible sections are only those given, at hand by ditches, and at a considerable distance north and south by pits at Bury St. Edmunds and Sudbury. The appearances in these harmonize with that conclusion. Conclusion and appearances differ from what we should expect on the theory that this boulder-clay was the product of the attrition between an ice-sheet and its bed. The reading of this paper was followed by a discussion in which Prof. Prestwich, Dr. Evans, Mr. Clement Reid, Mr. Charlesworth, Mr. Topley, Mr. Goodchild, the President, and the author took part.—On the melaphyres of Caradoc, with notes on the associated felsites, by Frank Rutley.—Notes on the geology of the Tonga Islands, by J. J. Lister. (Communicated by J. E. Marr, F.R.S.)—On the Inverness earthquakes of November 15 to December 14, 1890, by C. Davison. (Communicated by Prof. Chas. Lapworth, F.R.S.) In this paper the author gives reasons for supposing that the Inverness earthquakes of last year were due to the subsidence of a great wedge of rock included between a main fault and a branch one; and he considers that there is little doubt that these recent earthquakes were the transitory records of changes that, by almost indefinite repetition in long past times, have resulted in the great Highland faults.—The next meeting of the Society will be held on Wednesday, November 11.

Paris.

Academy of Sciences, June 29.—M. Duchartre in the chair.—On persulphates, by M. Berthelot. Some new facts are stated in proof of the existence of persulphuric acid not merely as an anhydride, S_2O_7, but also as a compound capable of forming distinct salts, similar as regards composition to permanganates, perchlorates, permolybdates, and pertungstates.—Experiments on the mechanical actions exercised on rocks by gas at high pressures and in rapid motion, by M. Daubrée. The author shows that volcanoes of the same group have approximately the same height, and points out that it is probable that each group is the result of internal action at one centre. These considerations are applied to old volcanic rocks, which often exhibit a marked tendency to equality of level. The experiments which throw light on the disturbances investigated were previously described.—Action of sodium alcoholates on camphor: new method of preparation of alkyl camphors, by M. A. Haller.—On a cryptogam parasite of locusts, by M. Charles Brongniart.—On surfaces possessing the symmetry of plane systems, by M. S. Mangeot.—On homogeneous finite deformations: energy of an isotropic body, by M. Marcel Brillouin.—On the biaxial character of compressed quartz, by M. F. Beaulard.—The photogenic efficiency of different sources of light, by M. A. Witz.—On an electro-magnetic bell, by MM. Guerre and Martin.—Contribution to the study of atmospheric electricity, by M. Ch. André. It is generally admitted that atmospheric electricity is subject to a diurnal variation. A discussion of the observations made by M. Mascart at Lyons since 1884 shows that electric potential varies in much the same manner as barometric pressure and relative humidity. In fact, curves showing the annual variations of relative humidity and electric potential have precisely the same form.—On the oxidation of azo-compounds, by M. Charles Lauth.—On the formation of the mesentery and the intestinal canal in the embryo of the fowl, by M. Dareste.—On the sting of *Heterodera Schachtii*, by M. Joannes Chatin.—On Cladosporiæ Entomophytæ, a new group of parasitic fungi of insects, by M. Alfred Giard.—Contribution to the study of the differentiation of the endoderm, by M. Pierre Lesage.—On the destruction of *Peronospora Schachtii* of the beetroot, by means of compounds of copper, by M. Aimé Girard.—Influence of muscular exercise on the excretion of urinary nitrogen, by M. Chibret.

Brussels.

Royal Academy of Sciences, April 4.—M. F. Plateau in the chair.—On the characteristic property of the common surface of two liquids under their mutual affinity, Part iii., by M. G. Van der Mensbrugghe. The observations given in the first paper indicated that the common surface of two liquids which act upon one another is subjected to a force whose direction is away from the centre of curvature. In the present note the author gives some new facts which appear to render this *force d'extension* very manifest. When a drop of olive oil is put upon the surface of distilled water, it slowly breaks up into a lens-shaped drop on the water surface and a spherical drop which descends to the bottom of the containing vessel. It is shown that a slow diminution occurs of the tension of the surface common to the oil and water. This diminution apparently arises from a slow chemical action between the two liquids, and which, if sufficiently prolonged, is manifested by the formation of a thin pellicle separating them. Many such phenomena as these are stated and explained according to the new theory.—Fourth note on the structure of the equatorial bands of Jupiter, by M. F. Terby. The author remarks that he was the first to comment upon the structure of Jovian equatorial bands, and to make known the fact that it is observable in small instruments. In a recent publication Mr. Keeler has overlooked these observations, and rendered this rectification necessary.—On the number of invariant functions by M. Jacques Deruyts.—*A propos* the rotation of the planet Venus, by M. L. Niesten (see Nature, June 18, p. 164).—Geometrical calculation of the distances of remarkable points of triangles, by M. Clément Thiry.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Saturn's Kingdom: C. M. Jessop (Paul).—Collection de Mémoires relatifs à la Physique, tomes i. to iv. (Paris, Gauthier-Villars).—Charles Darwin: C. F. Holder (Putnam).—Solutions of Examples in Elementary Hydrostatics: Dr. W. H. Besant (Bell).—Practical Electro-Therapeutics: A. Harries and H. N. Lawrence (Low).—Popular Astronomy: Sir G. B. Airy; new edition (Macmillan and Co.).—The *Electrician* Primers, 2 vols. (*Electrician* Office).—Report on the Cahaba Coal Field: J. Squire (Montgomery, Ala.).—A Vertebrate Fauna of the Orkney Islands: T. E. Buckley and J. A. Harvie-Brown (Edinburgh, Douglas).—Manuel Pratique d'Analyse Bactériologique des Eaux: Dr. Miquel (Paris, Gauthier-Villars).—Outlines of Field Geology, 4th edition: Sir A. Geikie (Macmillan and Co.).—The History of Human Marriage: E. Westermarck (Macmillan and Co.).—Memorials of John Gunn: edited by H. B. Woodward and E. T. Newton (Norwich, Nudd).—Michigan Mining School Report 1886-91 (Marquette, Mich.)—Sommaire de Photogrammétrie: V. Legros (Paris).—Die Indo-Malayische Strandflora: A. F. W. Schimper (Jena, Fischer).—Vorlesungen über Maxwell's Theorie der Elektricität und des Lichtes, 1 Theil: Dr. L. Boltzmann (Leipzig, Barth).

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THURSDAY, JULY 16, 1891.

ORGANIZERS OF TECHNICAL EDUCATION IN CONFERENCE.

THE progress that has been made during the past year by English County Councils in the application of their grants under the Local Taxation Act to purposes of technical education is attested by the map which accompanies the fourth Annual Report of the National Association for the Promotion of Technical and Secondary Education, and which we reproduce. It will be seen from this map that the counties which have determined to use the whole of the new fund for education form a large majority of the whole number both in England and Wales; and that London and Middlesex enjoy an unenviable, and we hope temporary, distinction, in having been the only counties to grab for the rates the whole of the money which might have been used to organize the secondary and technical education of their districts.

But while the map and the Report offer sufficient evidence of the good intentions of the County Councils, the solid progress already achieved is still more emphatically shown by the Conference of organizing secretaries which followed the annual meeting of the Association on the 3rd of this month. The very post of organizing secretary is the creation of the past few months. A year ago no county had dreamt of appointing an official to look after its education, and the Technical Instruction Act was only in operation in a few scattered centres. Now nearly twenty counties and county boroughs have special educational departments, with paid organizing secretaries. We need hardly point out the wisdom of making such appointments, in view of the unwonted duties cast on County Councils by recent legislation. The task is one which needs all the ability which is available, and this ability is of a highly specialized character, not to be expected of the average County Councillor or Clerk of the Peace, who besides have not the time for the necessary detailed work of organization. To leave the work to clerks would be to court failure, for the work to be attempted within the next few years must be largely tentative, and the direction of the experiments must be in the hands of men of knowledge, ideas, and resource, as well as of tact and judgment.

The selection of such men is not easy, and we are glad to find that the secretaries of the Technical Association are prepared to suggest candidates to County Councils which may be in need of them. The appointments made hitherto have been of two kinds: as temporary organizers, to inquire into claims and applications, to visit every district in the county, and to draw up a detailed scheme as the result of such inquiry; and as permanent secretaries to the Technical Instruction Committees, charged with the work of carrying out the schemes and inspecting the instruction, either personally or through the employment of experts.

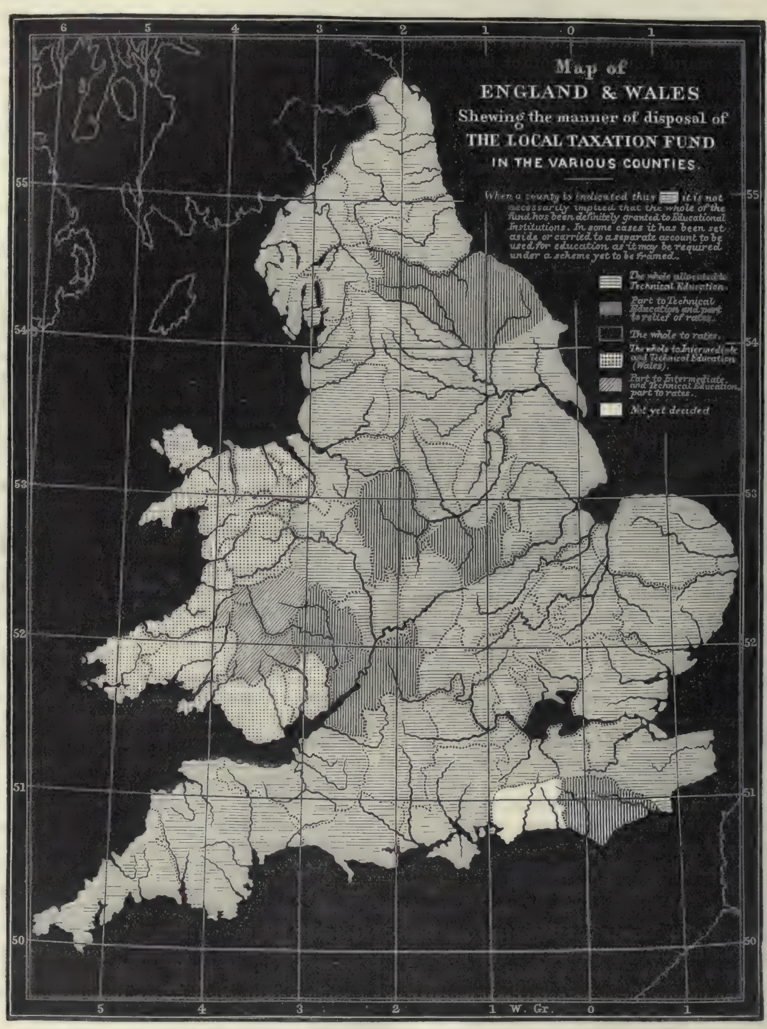
About two-thirds of the gentlemen who had been appointed up to the date of the Conference accepted the invitation to be present, the districts represented being Lancashire, Cumberland, Surrey, Sussex, Derbyshire, Devonshire, Oxfordshire, Nottinghamshire, and Hampshire, besides a few county boroughs. The Conference was private and informal, its object being rather the interchange of views and the comparison of notes than the adoption of any formal resolution.

The subject chosen for consideration was the relation of the local taxation grant to secondary schools—the most difficult, as well as the most important, of the questions with which the organizer finds himself face to face when preparing a scheme. Since Matthew Arnold wrote, the disgraceful condition of secondary education in England has been a common-place; but how inefficient many of the schools are, and what tracts of country are entirely without even such facilities as they offer, is probably scarcely realized by any except those who have made a minute study of the educational wants of an average county. The country grammar-school, with small endowment and ill-paid and lethargic head master assisted by a worse paid and more inefficient usher, is all that stands for secondary education in many a market-town. Many are without even the semblance of a school above the elementary rank, and the mass of the inhabitants, it is to be feared, hardly feel the want of anything more. Here and there an energetic master or governing body has succeeded in building up a good school in despite of local apathy and lack of funds, but the fee has to be pitched at a point which excludes wage-earners, and such schools are consequently "middle," not only in the character of their instruction, but also in the class by which they are attended. Meanwhile, the clever boy of the village national school, who might profit the nation by his brains and energy, is doomed, for lack of opportunity, to leave school at twelve for the hopeless rut of farm labour.

A country-side the general education of which is as here described is not a promising field for special technical instruction. A stupid set of uneducated farmers, and a scarcely less stupid class of uneducated labourers, form hardly a good soil in which to plant lectures on agricultural chemistry or the natural history of insect pests. And thus thoughtful observers have been driven everywhere to the conclusion, no less in country than in town, that access to good secondary schools is an even more crying need at the present day than the specialized instruction to which, indeed, a sound general education is the necessary preliminary.

What, in short, is wanted, is that within reach of every inhabitant of every county should be a good secondary school, with fees such as may be reasonably expected to be paid by small farmers and tradesmen, and to which all sons of artisans and labourers who can pass a reasonable examination before the age of twelve can have access by means of scholarships.

The question before the Conference was the best means of promoting this object under the powers given by the Technical Instruction Acts. It will be remembered that the definition of technical instruction in the Act of 1889 is sufficiently wide to cover most of the subjects taught in a secondary school, and it is therefore clear that aid can be given to such schools, provided that the County Council can be represented on the governing bodies, and that the schools are not conducted for private profit. As regards the erection of new schools, it is doubtful if the whole work of building could be undertaken, even if desired, by the County Council, but there is nothing to hinder contributions from being made towards the cost of laboratory, fittings, and apparatus; while a maintenance grant could be given to defray the expense of the teaching of scientific and technical subjects. It was stated at the Conference that the Charity Commissioners had shown every disposition to facilitate the work, by drafting amending schemes enabling the County Council to be duly represented on the governing bodies.


Map of
ENGLAND & WALES
Shewing the manner of disposal of
THE LOCAL TAXATION FUND
IN THE VARIOUS COUNTIES.
When a county is indicated thus ▤ it is not necessarily implied that the whole of the fund has been definitely granted to Educational Institutions. In some cases it has been set aside or carried to a separate account to be used for education as it may be required under a scheme yet to be framed.
The whole allocated to Technical Education.
Part to Technical Education and part to relief of rates.
The whole to rates.
The whole to Intermediate and Technical Education (Wales).
Part to Intermediate and Technical Education, part to rates.
Not yet decided
6
5
4
3
2
1
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1
55
54
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52
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50
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2
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0
1

But the subject which chiefly occupied the attention of the Conference was that of scholarships. That some scheme of scholarships should be devised to carry on picked scholars from elementary to secondary, and from secondary to higher institutions, was unanimously agreed; and it was further considered that the scholarships to secondary schools should not merely defray the fees, but should provide something towards the cost of maintaining the boy while at school. The advantage of choosing the scholars as young as possible, in order to give them the full advantage of secondary training, was also insisted upon.

The question whether the selection of scholars, by examination or otherwise, should be undertaken by the County Council, or left to the governing bodies of the secondary schools, or to the discretion of the teachers of the elementary schools, elicited some difference of opinion; but on the whole the Conference favoured the plan of examination by a board appointed by the County Council, acting as far as possible in co-operation with the head masters of the secondary schools of the county. On one point all were agreed: viz. that there should be two examinations, or at least two standards—one for the country districts and the other for the towns—lest the whole of the scholarships should be monopolized by the most favoured districts. The opinion was also expressed that it might be sometimes desirable (as apparently would not be illegal under the Technical Instruction (Amendment) Act, 1891) to make scholarships tenable at certain efficient private profit schools, where no public schools are available, although such schools are debarred from receiving direct assistance. Such a course, however, would have to be adopted with the utmost caution.

Finally, the Conference considered the relations of the County Councils to the Technical Association, and a unanimous opinion was expressed in favour of a closer connection, while a suggestion was thrown out for the establishment of a quarterly journal registering the progress made in the various counties, a proposal which we are glad to hear is receiving the careful consideration of the Association.

Altogether, the discussion was felt to be of considerable value to those who have the practical work of organization in hand. We hope that such a Conference will be held annually, even if not more often, for in the novel work which lies before the County Councils points of difficulty will continually occur, on which consultation will be most useful. By the way, why should not the organizing secretaries form a permanent Association, on the model of the two Associations of Head Masters?

THE EVOLUTION OF ANIMALS.

L'Évolution des Formes Animales, avant l'Apparition de l'Homme. By F. Priem. 384 pages. Illustrated. (Paris: Baillière et Fils, 1891.)

IN this addition to the series of volumes known as the "Bibliothèque Scientifique Contemporaine," we have a worthy companion to Prof. Gaudry's "Les Ancêtres de Nos Animaux," published three years ago. To some extent, indeed, the ground is covered by M. Gaudry's more ambitious "Les Enchaînements du Monde Animal," but since the latter is in three volumes, the present work ought to find numerous readers who might be repelled by the length of the other. Moreover, the work before us has the advantage of treating each group of animals throughout geological time in consecutive form, whereas in the "Enchaînements" the Palæozoic Invertebrates are described in one volume, and those of the Secondary period in another, while the Tertiary forms are not recorded at all. Again, our author enters much more fully into the probable origin of one group from another than is the case in Gaudry's work. It is true, indeed, that in most cases these views are not original; but since they are generally taken from the highest authorities on the several groups, they will commend themselves the more strongly to students. In most works on palæontology too little attention is, in our opinion, generally given to the evolution of the various groups of the Invertebrata from one another, and we can, therefore, give a hearty welcome to a volume like the present which is mainly devoted to this fascinating subject.

We need hardly say that Prof. Priem is an out-and-out evolutionist; and we trust that we shall not disparage his work by observing that in some cases—apparently carried away by the very natural desire to make the most of his subject—he appears to have gone rather too far, stating as facts what are at best but probable hypotheses. For instance, we find it definitely stated on p. 273 that the Stegocephalous (Labyrinthodont) Amphibians had a functional parietal eye, whereas there is, of course, no actual proof that this was the case.

The work is rendered attractive by the large number of woodcuts with which it is illustrated. We regret, however, that in some cases—and more especially among the Vertebrates—the execution of these figures is by no means satisfactory. Moreover, in the chapters devoted to the Vertebrates (some of which are the weakest portions of the work) there are figures which are not only bad, but are utterly untrue to nature. Thus on p. 266 the old figure of *Coccosteus*, with the maxillary bone doing duty for the mandible, once more reappears; while on p. 301 we have the reproduction of Goldfuss's erroneous restoration of *Pterodactylus crassirostris*, which is unfortunately given as an illustration of the short-tailed genus *Pterodactylus*, whereas that particular species belongs to the long-tailed genus *Scaphognathus*.

We notice that in many instances M. Priem gives his authority for his statements as to the phylogeny of particular groups, whereas in other cases such references are omitted. This is to be regretted, since it is often somewhat difficult to find out whether the author is promulgating his own views, or quoting those of others.

The volume commences with an introductory chapter on palæontological evolution, in which the phylogeny of the horse, and the well-known passage of *Paludina neumayri* into *P. hœrnesi* are instanced as the best examples we have of the derivation of one form from another. Following this chapter, we have the various groups of animals treated in detail, commencing from the lowest. In the main the classification adopted is fairly well up to date, although we shall note some instances where the author departs from the more usual modern arrangements.

For example, in treating of the classification of the Sponges on p. 36, the author disregards Prof. Sollas's separation of the Calcareous Sponges (Calcispongiæ) as a group of equal value with all the others (Plethospongiæ), so that we find the Soft, Horny, Flinty, and Calcareous Sponges ranked as equivalent groups. Again, in the Cœlenterates (or, as we prefer to call them, Zoophytes), the Palæozoic Corals are still classed under the primary divisions of Tabulata and Rugosa; the former group including such different forms as *Favosites* (belonging to the Zoantharia) and *Heliolites*, *Halysites* and *Chætetes* (usually referred to the Alcyonaria). Later on, however, pp. 62–64, the author recognizes *Heliolites* and its allies as the ancestors of the modern coral-like Alcyonarians, such as the Organ-pipe Coral (*Tubipora*), and we therefore fail to see his reasons for adopting the antiquated classification.

Some of the most interesting chapters in the volume are those devoted to the evolution of the Echinoderms, the author adopting Neumayr's view that the Palæozoic Blastoids, as well as Crinoids, Sea-Urchins, and Starfishes, are all separate branches springing from the Cystoids of the Palæozoic. The figures illustrating the gradual specialization of the Sea-urchins from the old Palæoechinoids, with their numerous rows of interambulacral plates, through the Triassic *Tiarechinus*, and thence to the Neocomian *Tetracidaris*, with its two rows of apical interambulacrals splitting into four near its equator, and thence to the modern "regular" Urchins, strike us as particularly well selected. Equally instructive is the transition from the "regular" modern Urchins (Neoechinoids) to the "irregular" forms—at first with the retention of the masticating apparatus, and subsequently with its loss.

Merely noticing that full justice is done to Neumayr's views regarding the phylogeny of the Brachiopods, we pass to the Mollusca, which we find treated in considerable detail and well illustrated. The author adopts the modern view of separating *Dentalium* as a distinct order (Scaphopods) from the Gastropods, and considers that both Pelecypods (Bivalves) and Scaphopods are derived from the latter. Nothing is said as to the origin of the Gastropods themselves, or, indeed, of the Cephalopods—probably for the very sufficient reason that nothing definite is yet known. In regard to the mutual relations of the various groups of Cephalopods, the author comes to the conclusion that the Ammonites should form a distinct order, "Ammonoidea," to be placed between the Tetrabranchiates (Nautilus) and Dibranchiates (Cuttlefish). Since, however, he adopts the view that their shells were really external, and that they are probably descended from Nautiloids, there seems but little necessity for this third order. The gradual increasing complexity in the sutures as we pass from Goniatites to Ceratites, and from the latter to true Ammonites, is held sufficient to prove the descent of the latter from the former; while Goniatites are considered to be the direct offshoots from Nautiloids, which are themselves derived from straight forms like *Orthoceras*. It would require too much space to enter on the consideration of the relations of the various genera of Ammonites to one another; but we may mention that the author fully adopts the modern views, such as the evolution of the keeled *Amaltheus* of the Jurassic from *Ptychites* of the Trias, and also that the uncoiled forms (*Hamites*, *Scaphites*, &c.) have had several distinct points of origin from true Ammonites. And here we may take the opportunity of mentioning that the terms *Ægoceras* and *Haploceras* applied to genera of Ammonites, are preoccupied by two well-known genera of Mammals, and therefore require changing. In regard to the Dibranchiate Cephalopods, it is considered that Belemnites have been derived from forms allied to Goniatites, and have themselves given origin to the modern Cuttle-fish. If this be the true phylogeny of the Cephalopods, it indicates a gradual increase in the complexity of the shell of the Tetrabranchiates, till it attained its maximum in the Jurassic and Cretaceous. Then the total disappearance of all the external-shelled forms with the exception of the *Nautilus;* while at the same time the Dibranchiates were gradually tending to develop less and less complex internal shells, till these culminated in the simple "pens" and "bones" of the modern cuttles and squids.

Coming to that portion of the work devoted to the Vertebrates, we find, as already mentioned, that the author has been in some places less successful than in the earlier chapters. We have already alluded to the misleading nature of one of the figures in the chapter on fishes, and we have to add that several of the few others with which that chapter is illustrated are highly unsatisfactory. It is probable, indeed, that the author had no opportunity of seeing the second volume of the "British Museum Catalogue of Fossil Fishes" before passing his proofs, as otherwise he would doubtless have modified some of his statements.

In his remarks on the difficulty of distinguishing between Dipnoid and Ganoid fishes (p. 265), the author seems to be totally unaware of the difference between the "autostylic" skulls of the former and the "hyostylic" of the latter; and when, on p. 267, he states that the Dipnoids are a lateral branch of the Crossopterygian Ganoids, he is directly at issue with the writer of the Museum Catalogue, who states (p. xx.) that, "concerning the evolution of the Dipnoi, palæontology as yet affords no information." Again, although Prof. Cope's observations as to the primitive structure of *Pteraspis* and its allies are referred to, we doubt whether the suggestion that the opening on the dorsal side of the head-shield corresponds to the aperture of a parietal eye will commend itself to the students of ichthyology. The chapter on the Batrachians is all too short; and, bearing in mind their resemblance to the Dipnoids in the autostylic structure of the skull, it is scarcely safe to make the statement (p. 282) that they are derived directly from Ganoids.

From his treatment of reptiles we fear that the author has but a very faint conception of the nature of a Theriodont or a Dicynodont, or else he would surely have made more of their affinity to the Batrachians on the one hand, and to Mammals on the other; while he would have also omitted any reference to the purely adaptive resemblance existing between the skull of *Udenodon* and that of a turtle. M. Priem might also have informed his readers that Dicynodonts are not confined to Africa. On the other hand, we are pleased to see that M. Priem rejects the heresy propounded by some of his countrymen, that Ichthyosaurs were derived primitively from marine reptiles, in favour of the more rational view of their near relationship to the Rhynchocephalians. In stating that the Plesiosaurs are likewise related to the Rhynchocephalians, the author is in accord with modern views, although he should also have referred to the many indications of affinity presented by these reptiles to the Chelonians. When, however, it is stated, on pp. 295, 296, that the latter were probably derived from the toothless Dicynodonts (*Udenodon*), the author at once proclaims his ignorance of some of the leading features of reptilian osteology. The statement on p. 297, that the gigantic Siwalik tortoise had a shell measuring four metres in length, leads us to wonder when this fiction will finally disappear from text-books. The author's treatment of the Pterodactyles and Dinosaurs calls for no separate mention, although we are led to wonder why the Crocodiles are so widely separated from these groups.

The whole chapter on birds is decidedly feeble; and we must certainly take exception to the statement that *Hesperornis* and *Ichthyornis* respectively connect *Archæopteryx* with the Ratitæ and Carinatæ.

Turning to the last chapters, on the Mammals, we find the author adopting the view that the Monotremes have had an origin totally separate from the other two subclasses. We then have a notice of the Secondary Mammals, in which we observe a lamentable lack of attention to recent work on their affinities, and also to the synonymy of the various genera. We also notice that the Jurassic *Plagiaulax* and its allies are still referred to the Diprotodont Marsupials (p. 327); so that on these points the author's evolutionary views are totally out of date. Following the Marsupials, we have a very fair, although brief, account of the most recent conclusions on Mammalian phylogeny, which needs but few remarks. We notice, however, that the author adopts M. Boule's views as to the dual origin of the *Canidæ*, according to which the Foxes (Alopecoids) are considered to have originated from *Cynodictis*, while the Wolves, Jackals, and Dogs (Thooids) trace their descent to *Amphicyon*. To ourselves, indeed, it has always appeared difficult to understand how these two groups of *Canidæ* have become so much alike if they had this dual origin; and this difficulty is increased by the author's statement that those Thooids known as *Cyon* differ from the other members of that group in having originated from *Cynodictis*.

On p. 343 the author makes a slip in stating that the Hydracoidea are now represented only by a single genus; while later on he appears to be uncertain whether the Siwalik beds should be regarded as Upper Miocene (pp. 349, 350) or Upper Pliocene (p. 366). Again, we notice on p. 353 some want of acquaintance with the recent literature relating to the ancestry of the horse, *Orohippus* being identified with *Pliolophus*, whereas the latter is really the same as *Hyracotherium*, while the former is identical with *Pachynolophus*. We are in full accord with the author when he states, on p. 361, that *Chalicotherium* (with which the supposed Edentate *Macrotherium* is now known to be identical) is an aberrant Ungulate, although we must be permitted to differ from him when he adds that it shows signs of affinity with the Edentates.

We must likewise take exception to the statement, on p. 370, that the Indian Nilgai is in any sense the progenitor of the Oxen; while the view expressed on the same page, that the Buffaloes, Bisons, and true Oxen have severally originated from three distinct groups of Antelopes, can be only regarded as another instance of the author's partiality for multiple phylogenies. Although M. Priem is careful not to commit himself to the view that the Cetaceans have been derived from the extinct Enaliosaurian reptiles, yet the prominence which he gives to the statement of that view may be taken as a sign that he has not thoroughly purged himself from that heresy.

Finally, although we have felt bound to call attention to a certain amount of imperfection in the later chapters, yet, as a whole, we can conscientiously recommend the work before us to those readers who are desirous of obtaining in a compact form a summary of the evidence afforded by palæontology of the progressive evolution of animal forms.

R. Lydekker.

METALLURGY.

Leçons sur les Métaux. Par Prof. Alfred Ditte. (Paris: Dunod, 1891.)

Traité pratique de Chimie Métallurgique. Par le Baron Hans Jüptner von Jonstorff. Translated from the German by M. Ernest Vlasto. (Paris: Gauthier-Villars, 1891.)

THESE two volumes, recently published, are both of unusual interest. The first, by Prof. Ditte, who is well known to English readers by his "Exposé de quelques Propriétés générales des Corps," may be said to mark a new departure in teaching the chemistry of metals. He points out that the principles of thermochemistry do not merely enable reactions to be explained, but to be predicted, and, on the other hand, when two sets of reactions are simultaneously possible, the laws of dissociation render it possible to rigorously define the conditions of equilibrium which are established in the chemical "systems" under consideration. It is often possible, with the guidance afforded by these laws, to say, in the absence of direct experiment, why one reaction is impossible and another certain to occur; or why a certain reaction begins without difficulty, and is arrested at a definite stage; or why a reaction which takes place readily under certain conditions cannot be effected under others that do not appear to differ greatly from those which were favourable to it. As a pupil of Deville, the author might have been expected to develop, in a treatise such as this, the teaching of his great master, and he has admirably performed his task. The classification of the work is excellent, the metals being first considered collectively, and then in detail with numerous tables of the data and constants which are so frequently required by metallurgists.

The work begins with a very clear account of Berthelot's labours in *mécanique chimique*, special care being devoted to the description of the calorimetric investigations, and to the appliances adopted in these important researches.

It appears to be a great advance for us in this country to read a chemical treatise in which the thermal values of the equations are stated in calories, side by side with the formulæ. As the book is too long to review in detail, it may be well to indicate the nature of one section only, as showing the author's care and thoroughness in the selection and arrangement of the materials. Take, for instance, the few pages devoted to carbides. The author points out that carbon in uniting with metals sometimes gives rise to the formation of true compounds, and at others to solutions of carbon in the metal. He then describes the orange-yellow product obtained by the action of carbon on metallic copper, and passes to the association of carbon with nickel, which does not confer upon nickel the property of being hardened by rapid cooling. The definite carbides of manganese, as well as the indefinite associations of carbon with iron and manganese, receive due attention, and the author proceeds to deal with the carbides of iron, and finally with the well-defined carbides of niobium and tantalium, which have respectively the formulæ Nb_2C_2 and Ta_2C_2.

A terse description is then given of the work of Troost and Hautefeuille on the heat of formation of carbides of iron and manganese, which led to the conclusion that the union of carbon and iron is attended with absorption of heat, while in the case of the union of carbon and manganese heat is evolved, the evidence leading to the belief that Mn_3C is a true compound possessing considerable stability. The action of heat on carbides is then dealt with, and a brief, but sufficient, reference is made to Forquignon's work on the action at a high temperature of hydrogen on cast-iron. The section concludes with a description of the modes of preparing carbides, and with a sketch of the formation and properties of the nitrocarbides, more especially those of niobium and titanium.

The sections of the work devoted to the consideration of tellurides, arsenides, and antimonides, are equally good. With regard to individual metals, in the portions of the work as yet published, only potassium, rubidium, cæsium, ammonium, thallium, sodium, lithium and the metals of the alkaline earths, barium, strontium, and calcium are dealt with, but sufficient evidence of the merits of the book has been given in this brief review to show that the rest of it will be gladly welcomed, for Prof. Ditte has earned his place among the great metallurgists of France.

We should be grateful for curves indicating the effect of definite elements on the physical constants of metals.

Baron Jonstorff's book is of an entirely different character, though it is not, in its way, less excellent or useful. He says that it issues from an ironworks, and is addressed to practical metallurgists. Its aim is, however, somewhat different from that of most treatises on analytical chemistry, the author's intention being not merely to guide the chemist in his analytical methods, but to enable a blast-furnace manager or an iron-master to realize what kind of services the laboratory can render, what questions relating to the routine of work the analyst can solve, and, above all, in what way the questions should be put.

The author deals with the more important special methods of analysis, and of assaying iron and steel, and he gives due attention to the examination of refractory materials—slags, fuel, and gaseous products—and his method is singularly clear and precise. An appendix gives tabular statements which will be useful in daily work.

The book, as a whole, shows incidentally the great difference between the works-laboratory of the present day and that of twenty years ago. There is still much room for improvement, no doubt, but the laboratory of an ironworks has, in many cases, ceased to be little better than a shed, erected, say, behind the boiler-house, with an analyst and a few boys as the scientific staff.

Those who have visited the author in his beautifully situated Styrian works, and have seen his manipulation, as the reviewer has, will appreciate the excellence of his labours, and will be glad that a good translation into French will make their results more generally known.

W. C. Roberts-Austen.

BACTERIA AND THEIR PRODUCTS.

Bacteria and their Products. By Sims Woodhead, M.D. Published in the "Contemporary Science Series." (London: Walter Scott, 1891.)

SCARCELY a year passes in England, France, or in Germany, without the publication of one or more treatises on the fascinating subject of bacteriology. Many of the more recent of these works have been written for the general reader rather than for the student, and have shown a considerable want of accuracy and lucidity, a circumstance which can only be accounted for by the fact that accomplished bacteriologists have not been their authors.

We have now before us "Bacteria and their Products," a work which we might infer from a glance at the cover, and general arrangement, to be certainly intended for the general reader. This view is strengthened by the several object-lessons and homely similes scattered throughout the text, with the fitness of which we totally disagree; witness, for example, the extraordinary comparison of a nodule of Actinomyces with two daisy heads placed base to base, "the sterile flowers in the centre" then corresponding to the club-shaped rays. The comparison is bad, but the botany is worse. Then there is the not very abstruse mathematical problem on p. 24, and the guide-like description of the Pasteur Institute, all intended, we must conclude, for the general reader rather than for the student.

On the other hand, there is a very large collection of facts, much information about fermentation and chemistry (although the interesting and oft-quoted experiments of Raulin are omitted), numerous references, and a very plentiful supply of formulæ, the whole requiring, in order to understand and appreciate them, a reader equipped with a thorough knowledge of the sciences bearing on the subject.

Putting this question aside, however, we candidly confess that we do not admire the style or arrangement of the book. There is a conspicuous want of lucidity, and of that accuracy of observation which one would have expected of the author. For instance, "What are Bacteria?" is the question propounded in chapter ii.; but the answer to this key-stone question is left in much doubt, as the description of the protoplasm, cell membrane, mode of division and reproduction of the "specks," is exceedingly confused. We should not choose Gram's method to demonstrate the capsules of Actinomyces, nor, indeed, any other capsules; and we have reason to doubt, after the beautiful monograph on Cladothrix by Billet, that the brown colour of that organism is due to iron. Again, what does the author mean when, speaking of cilia, he says, they "appear to develop only in those organisms that have special affinity for oxygen, for, as soon as the ciliated forms reach the surface of a fluid, they lose their cilia or they become much less active," &c.? Tables of classification are heaped in with scarcely any attempt to sift and reduce them to a form compatible with the scope of the book. Is this done because, as the author says (p. 47), "to the pathologist, however, these classifications are of comparatively little value"? We maintain that for a work of this kind the author has no right to take a one-sided view, and that to the science of bacteriology the study of morphology is as important as any other side of the subject.

Turning to the description of actinomycosis and anthrax, we are surprised to find McFadyean taken as the guide in the former. Why is not the author his own guide? Or why does he not, at least, use the recent results of Boström? Then in "anthrax" it is stated "that at the point of inoculation in animals there is usually no evidence at all that it has been the point of entrance of the bacilli." This is scarcely compatible with a thorough knowledge of this familiar organism.

Again, in the opening chapter, a number of bacteriologists' names are mentioned. We think the author hardly does justice to those of our own country, for amongst the four names placed by the author in honourable association with the great name of Sir Joseph Lister, neither Lankester, nor Tyndall, nor Lawes and Gilbert, nor Wooldridge, nor Lingard, finds a place. And yet not only are these amongst our highest authorities, but the observations of Lankester and the experiments of Wooldridge constitute cardinal points in the history of bacteriology.

Lastly, the question of illustrations is a difficult one. There are very many photographic processes to choose from, and considering that there are only 20 illustrations, the author might have employed collotypes (compare Fränkel's atlas) or copper blocks; or, having used zinc blocks, should have had them printed on separate sheets, for it ought to be more generally known that it is of no use expecting a good impression from blocks of this description when printed on ordinary paper and in the text.

OUR BOOK SHELF.

Our Country's Flowers. By W. J. Gordon. (London: Day and Son, 1891.)

THIS volume is intended to aid beginners to ascertain the botanical name of any British wild flower or fern with which they may meet. After a list of local English plant-names, the serious work of the book begins with an explanation of how plants are classified, interwoven with which are a sufficient number of the terms used in describing plants to make the book "not too technical, but just technical enough" for the reader who desires to have a "nodding acquaintance" with the wild flowers of his own country. This is given first in a chatty style, and then repeated in a convenient tabular form. Next, the essential characters of the natural orders are given, after which the buttercup order, or Ranunculaceæ, is treated of at some length as a pattern of how identifications can be made. This is followed by a glossary of botanical terms, in some of which, in attempting a condensed and popular style, the writer has somewhat distorted the meaning. "Cambium" is erroneously described as a layer of mucilage, instead of a tissue. The characters of the natural orders are again stated, this time in alphabetical sequence, followed by a chapter on the genera, each of which is accompanied by a woodcut, intended to show its diagnostic character, but it is doubtful whether (at least in some of the orders) this is accomplished, as is also the case with some of the specific diagnoses with which the volume closes.

In the 33 coloured plates 509 species are depicted. This crowding is unsatisfactory, and tends to obscure what might otherwise be very useful. The figure on plate 23, numbered 388, may possibly be intended for 368, the stinging-nettle, or it may be some abnormal state of the inflorescence of a grape-vine. *Centranthus ruber* (204) and *Plantago lanceolata* (346) are also wonderful specimens of those plants. The artist, apparently, is amongst those who do not regard colour (unless it be the quantity thereof) as of value in discriminating species. The volume will, nevertheless, be a pleasant and useful companion to many during a country holiday, and, with the author, we hope will lead on to deeper study.

C. H. W.

A Summary of the Darwinian Theory of the Origin of Species. By Francis P. Pascoe, F.L.S., &c. (London: Taylor and Francis, 1891.)

IT is difficult to understand why the author of this pamphlet should think it worth while to remind his readers periodically that he is an opponent of Darwinism. Some space was recently devoted in these columns to the consideration of a book on the same subject by Mr. Pascoe, and the present production is nothing more than an abstract of this work, delivered in the form of an address to the Western Microscopical Club. We have no new facts nor arguments; there is the same lamentable display of misconception, and the author has simply strung together some sixteen pages of excerpts from the writings of Darwin and others, without any attempt at connected reasoning either for or against the Darwinian theory. The author's position is practically this: here is the whole animal kingdom, consisting of about 600,000 species; you must explain every detail of specific structure, down to the most insignificant, by the theory of natural selection; if you cannot do this, the theory is untenable. The whole of Mr. Pascoe's writings in connection with Darwinism amount to this, and nothing more; he has reiterated this statement, if not literally, at any rate in spirit, on every available opportunity for the last twenty years. The present pamphlet will, let us hope, for the sake of the author's reputation, be the last declaration to the same effect, for there is surely nothing gained either by Darwinism or anti-Darwinism by squandering the systematic powers which he is known to possess in picking out scraps of sentences from the "Origin of Species," &c., and publishing these things "of shreds and patches" under grandiloquent and misleading titles.

R. M.

The Business of Travel: a Fifty Years' Record of Progress. By W. Fraser Rae. (London: Thomas Cook and Son, 1891.)

THIS year the well-known firm of Thomas Cook and Son celebrate their fiftieth anniversary, and Mr. Fraser Rae has taken the trouble to write the present work in order to mark the occasion. The firm, it seems, had very small beginnings. Its history may be said to date from the day when, in 1841, Mr. Thomas Cook, walking along a country road, suddenly reflected that a certain temperance meeting at Loughborough would probably be a brilliant success if a special excursion train could be run between that place and Leicester. Apparently, no such thing as a special excursion train had ever before been heard of. The idea was carried out, and attracted so much attention that Mr. Cook—who was at that time a wood-turner—was often asked afterwards for advice in the organizing of railway excursions; and by and by he devoted himself wholly to the task of developing "the business of travel." His son has been for many years the sole managing partner, but to the elder Mr. Cook belongs the credit of having conceived the system with which his name is now associated. To what vast proportions the system has grown everyone knows; but there are probably few who know much about the various stages through which it has advanced to its present position. Mr. Fraser Rae tells the story clearly and effectively, and most readers, when they have finished his narrative, will be disposed to agree with him in thinking that the jubilee of a firm which has played so prominent a part is an event of interest in the social history of the nineteenth century. Messrs. Cook, by their energy and enthusiasm, have given a powerful stimulus to the popular love of travel; and they may fairly claim that their establishment ranks to some extent among the influences which are tending to break down international prejudices.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Albert University.

I DESIRE to associate myself with Prof. Carey Foster and, to a great extent, with Prof. Ray Lankester in the statements made and the opinions expressed by them in your last issue. Present circumstances force me to do so as briefly as possible; but I should be the less satisfied to keep altogether silent because I had something to do with the drafting of the "Albert" charter in 1887.

For my part, that draft was never regarded as an effective solution of the problem of a University *for* London. I thought of it only as a handy weapon for forcing the appointment of a Royal Commission, and for shaking the London University Senate out of its happiness in the steady increase of untaught candidates for degrees.

A Commission was extorted; and it had the impartiality, at least, of ignorance. Its inquiry was short and hurried, yet it learned enough of what had been done for academic organization by the London Colleges, during sixty years, to condemn the sufficiency and self-sufficiency of the London University. That the Commission, notwithstanding, should first give the London University an opportunity of transforming itself for London's good, was natural and proper in all the circumstances.

We know what followed. The London University Senate was slow, very slow, to move at all towards meeting the London Colleges. But at last it woke up, and then after a time began to display a novel spirit of conciliation. Fifteen months ago, a real accommodation seemed to have been attained between the Councils of the Colleges on the one hand and the University Senate on the other. Even when the Senate thereafter, yielding to an irrelevant clamour from Provincial Colleges, decided to give these also a direct representation (in the teeth of the Commission's instruction and without warning to the London Colleges), I was one of those who here were still willing to try what could be made of the top-heavy and lumbering scheme. But trial there was never to be; for Convocation, which probably would reject any measure of reform, gathered itself up and made swift end of this one.

It looks now as if the "Albert University" were straightway to be upon us instead. I will not inquire into the agencies that have brought this result into such near view. Nor will I in your columns follow up my present and my late colleague's arguments against the prospective creation with others that seem to me of serious import. But I may be allowed to endorse emphatically what Prof. Lankester has said as to the absence of sanction by the professorial body here to the "Albert" draft charter. And nothing could be more to the point than Prof. Foster's observation that the "Albert" scheme has never been submitted to a meeting of the Governors of the College—which means, to the College as a corporate body.

Prof. Lankester is clearly right in contending that the whole question should now have been, or should still be, referred back to the Commission. I must, however, as a Scot, remark upon his assumption that the Northern Universities are professorially governed—free especially (as he urged in a former letter) from the mischievous lay influence of mere graduates. The fact is, that, ever since 1860, graduates, in "General Council" and also by direct representation in the "University Court," have not been without voice or influence; while, by the later reform of the other year, not only is graduate and other lay influence increased, but also the professorial powers of general management are largely diminished or even (as respects finance, &c.) abolished. There were more reasons than evidently Prof. Lankester knows of for curtailing the old professorial supremacy in Scotland. But it does not follow that in England, and more especially in London, there should not be a much franker recognition of professorial (that is, expert) knowledge of educational ends and means than appears in the "Albert" draft charter.

G. CROOM ROBERTSON.

University College, London.

P.S.—Since these remarks were put in print, a decision of the Privy Council has been announced in favour of an "Albert" (or "Metropolitan") University. They lose, therefore, most of whatever interest they may have had; but they may still appear, so far as I am concerned, if the Editor pleases. I regret the decision, and think the promoters of it may yet have reason to wish that their action had been less hurried. At the same time, one may acknowledge the remarkable energy and fertility of resource with which the enterprise has been conducted to its thus far successful issue.—G. C. R.

IF I may be allowed another word on this subject, I should like to say that, having been all along a keen advocate of the establishing of a strong professorial University in London, not necessarily in slavish imitation of the German system (of which I happen to know something), but combining the main features of its professoriate (of which I think I showed my appreciation in a paper read at Bath in 1888, before Section B of the British Association) with the essential elements of the present University of London, and believing that the draft charter of the Senate, which was presented to Convocation, contained in it the potentialities, out of which (with the exercise of a little common-sense to soften down such asperities as might cause friction in its initiatory working, together with a little patience to allow for the time necessary in all evolutionary changes) a strong professorial University could be developed, I voted for the Senate's scheme, and still think the adverse vote of Convocation the greatest disaster that has befallen the University in the half-century of its existence.

Of all the bitter things said by Prof. Lankester in his former letter, nothing was more to the point than his sarcastic challenge to the existing University to reform itself, if it can, with the "dead weight of graduates tied round its neck, and called Convocation." But must an institution, which has admittedly done so much good, be swamped because of the accident of a flaw in its constitution? Is there no power to remove this millstone from its neck? If anything can exceed one's admiration for Prof. Lankester's candour in penning the letter, which appears in NATURE this week (July 9, p. 222), it is the satisfaction one must feel at finding that the projected repetition of "federal futility" which is at present in a state of incubation at the Council Office, has no attraction for him. It is to be hoped that the main question will be referred back to the Royal Commission, and that the Commissioners will give such advice to the "powers that be," that the shortsighted decision of Convocation may be overruled, as Prof. Lankester has suggested twice over, and that (to use the words spoken to me, the morning after the vote, by a distinguished Oxford man, whose academical experience no one could challenge) "the Government will take up the matter, and pass an Act doing what sensible people wish to see done," by co-ordinating and harmonizing, instead of segregating, the present machinery for higher education in the metropolis, including the great medical schools.

Prof. Karl Pearson's idea of the "fusion" of the two Colleges (see NATURE, June 4, p. 102), as distinct from "federation," is splendid in theory; but will it work? Can the fluxing material be found, which shall make the iron and the clay interfuse without either Gower Street or Somerset House, or both, sacrificing those traditions which are the strongest element in that *individuality* which each values so highly and both seem so anxious to conserve?

A. IRVING.

Wellington College, July 10.

Name for Resonance.

ALTHOUGH inadvisable as a rule to correct errors in a report for which one is not responsible, there is one little mistake on p. 238 this week, which, uncorrected, may lead to the extinction of a useful suggestion.

In discussing the subject of "electric resonance" recently at Cambridge, I found that the term conveyed no correct meaning to the untechnically instructed. Its natural meaning implies echo or reverberation, and has a definite relation to sound. Now, although a sort of reverberation or repetition is part of the effect intended to be denoted by the phrase resonance, yet the most essential feature of that phenomenon, and the one most to be emphasized in the recent extensions of the term, viz. the accord of frequency or similar tuning between two vibrators, is not connoted at all. Hence, even in acoustics the term is hardly satisfactory, while its extension to other departments of physics may be misleading.

It was suggested, however, by Dr. Arthur Myers, that the existing word *σύντονος* has almost exactly the right connotation, and has no special limitation to sound; while the derivatives *syntony*, *syntonic*, and *syntonise* may readily become English without exciting repulsion.

The adjective "symphonic," suggested by the reporter of the Physical Society, does not strike me as so good, because it specially refers to sound again, and because the word "symphony" has already another definite meaning.

July 10. OLIVER J. LODGE.

Force and Determinism.

I DO not think there are many non-physicists who will attempt to gainsay the fact that, under physical constraint, the direction of motion may be determined without affecting the quantity of the energy concerned, and without expenditure of energy. This is seen when the earth and sun revolve around their common centre of gravity, or when I twirl my stick around my finger and thumb; the earth and sun in the one case, and the ferrule and knob of my stick in the other case, being bound into one system physically. But I do think that an able and clear-headed physicist like Dr. Oliver Lodge would be doing a great service to non-physicists if he would, in your widely-circulated columns, explain and solve, shortly and in non-technical language, the difficulties which trouble some of them; aiding them, for example, to comprehend the exact force of the words expenditure of energy, and helping them to see that in all known cases of change of direction of motion such change is effected under physical constraint. It is when they are told by a certain class of metaphysicians, who quote, or misquote, physics in support of their assumptions, that physical motion is controlled by will-power or volition, always acting at right angles to direction of motion, and therefore leaving the amount of energy unchanged; it is *then*, I say, that they begin to grow restive, and to demand definite and verifiable evidence that such metaphysical constraint is (*pace* Sir John Herschel) a necessary or philosophical conception, and that it is impossible to explain the phenomena without having recourse to it. If Dr. Lodge would consent to help non-physicists in this way, and would indicate what are the "important psychological consequences" to which he alludes, he would be doing some of us a good turn. C. LLOYD MORGAN.

University College, Bristol.

AS Prof. Lodge says he is glad to see that his statement, "although expenditure of energy is needed to increase the speed of matter none is required to alter its direction," called in question, and as he has so kindly answered one letter on the subject, may I ask him to criticize the following remarks?

The theory of kinematics is based on certain geometrical concepts, which may be summed up in the term space, and on the concept of time. The laws of motion, together with the assertion that mass is not a function of space or time, may logically be regarded as implicitly defining mass and force. Energy may similarly be defined, in terms of these kinematic concepts, as $\Sigma\frac{1}{2}mv$. For I think the progress of science is tending to show that the term "potential energy" is only a cloak to cover our ignorance of the kinetic energies which for the moment have escaped our ken. But in any case the statement quoted is logically only a truism, deduced from the definitions of its terms, and is therefore indisputable in all mechanical theorems. But if it is to be applied outside the sphere of pure mechanics, the moral will lie in the application of it—that is, it will be necessary to examine, before applying it to any new subject-matter, whether the definitions from which it was deduced apply to that subject-matter or not.

For example, by the third law of motion, mechanical force only acts *between two masses*, the momenta generated in them being equal and opposite. If, therefore, psychic force is to come under the definition of mechanical force, it can only act *between* two particles. And, therefore, if psychic force is to do no work, by reason of its always acting in a direction normal to the path of a particle, it can only act between two particles whose paths happen to have a common normal—an occurrence which must be infinitely rare. EDWARD T. DIXON.

12 Barkston Mansions, South Kensington, July 4.

Magnetic Anomalies.

THE discovery of very strong magnetic anomalies between Charkov and Kursk in Russia, to which A. de Tillo has lately referred in the *Comptes rendus* and in NATURE, raises the question whether the values there observed are strictly local, or extend over a relatively wide area. Thus, it would be of great interest to know if, on moving, say, some metres away from a station, the declination and inclination hold the same value. If not, there is clearly some cause which acts at a short distance; but if constancy is observed, a great step would be taken towards the settlement of the question as to the existence of strong variations common to a wide area.

When magnetic anomalies are observed, the first thing to be done is to ascertain whether the values found in a given locality have a definite meaning—that is, whether they do not change for slight displacements; otherwise, the determination of the magnetic elements has no meaning, as it is impossible to refer them to geographical co-ordinates.

The overlooking of this precaution has often led to serious mistakes. ALFONSO SELLA.

Biella, July 4.

Physical Religion.

AS a constant reader of NATURE from its commencement, and the possessor of its forty-three and a half volumes, I venture (after reading the review of "Physical Religion" in this week's number) to ask if it is intended to develop it into a theological journal. Because, however smart it may be to abolish Abraham without "even taking the trouble to discuss" him, or to dispose of *Lux Mundi* in a contemptuous sentence, it is hardly in accordance with scientific methods.

It is curious that many "Agnostics," though by their own showing (if they would talk Latin instead of Greek) they are *Ignoramuses* at best, should be so certainly sure of everything, when a little reflection and modesty might satisfy them that as "*Know-nothings*" (in plain English) they have no more right to *deny* than to *assert*.

The standing motto of your title might be improved by the addition of "Ne supra crepidam sutor."

Hampstead Heath, July 11. B. WOODD SMITH.

SOME APPLICATIONS OF PHOTOGRAPHY.[1]

ONE of the subjects to which I propose to invite your attention this evening is the application of instantaneous photography to the illustration of certain mechanical phenomena which pass so quickly as to elude ordinary means of observation. The expression "instantaneous photography" is perhaps not quite a defensible one, because no photography can be really instantaneous—some time must always be occupied. One of the simplest and most commonly used methods of obtaining very short exposures is by the use of movable shutters, for which purpose many ingenious mechanical devices have been invented. About two years ago we had a lecture from Prof. Muybridge, in which he showed us the application of this method—and a remarkably interesting application it was—to the examination of the various positions assumed by a horse in his several gaits. Other means, however, may be employed to the same end, and one of them depends upon the production of an instantaneous light. It will obviously come to the same thing whether the light to which we expose the plates be instantaneous, or whether by a mechanical device we allow the plate to be submitted to a continuous light for only a very short time. A good deal of use has been made in this way of what is known as the magnesium flash light. A cloud of magnesium powder is ignited, and blazes up quickly with a bright light of very short duration. Now I want to compare that mode of illumination with another, in order to be able to judge of the relative degree of instantaneity, if I may use such an expression. We will illumine for a short time a revolving disk, composed of black and white sectors; and the result will depend upon how quick the motion is as compared with the duration of the light. If the light could be truly instantaneous, it would of necessity show the disk apparently stationary. I believe that the duration of this light is variously estimated at from one-tenth to one-fiftieth of a second; and as the arrangement that I have here is one of the slowest, we may assume that the time occupied will be about a tenth of a second. I will say the words one, two, three, and at the word three Mr. Gordon will project the powder into the flame of a spirit lamp, and the flash will be produced. Please give your attention to the disk, for the question is whether the present uniform grey will be displaced by a perception of the individual black and white sectors. [Experiment.] You see the flash was *not* instantaneous enough to resolve the grey into its components.

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[1] Friday Evening Discourse, delivered at the Royal Institution of Great Britain, on February 6, 1891, by Lord Rayleigh, F.R.S., Professor of Natural Philosophy, R.I.

I want now to contrast with that mode of illumination one obtained by means of an electric spark. We have here an arrangement by which we can charge Leyden jars from a Wimshurst machine. When the charge is sufficient, a spark will pass inside a lantern, and the light proceeding from it will be condensed and thrown upon the same revolving disk as before. The test will be very much more severe; but, severe as it is, I think we shall find that the electric flash will bear it. The teeth on the outside of the disk are very numerous, and we will make them revolve as fast as we can, but we shall find that under the electric light they will appear to be absolutely stationary. [Experiment.] You will agree that the outlines of the black and white sectors are seen perfectly sharp.

Now, by means of this arrangement we might investigate a limit to the duration of the spark, because with a little care we could determine how fast the teeth are travelling—what space they pass through in a second of time. For this purpose it would not be safe to calculate from the multiplying gear on the assumption of no slip. A better way would be to direct a current of air upon the teeth themselves, and make them give rise to a musical note, as in the so-called siren. From the appearance of the disk under the spark we might safely say, I think, that the duration of the light is less than a tenth of the time occupied by a single tooth in passing. But the spark is in reality much more instantaneous than can be proved by the means at present at our command. In order to determine its duration, it would be necessary to have recourse to that powerful weapon the revolving mirror; and I do not, therefore, propose to go further into the matter to-night.

Experiments of this kind were made some twenty years ago by Prof. Rood, of New York, both on the duration of the discharge of a Leyden jar, and also on that of lightning. Prof. Rood found that the result depended somewhat upon the circumstances of the case, the discharge of a small jar being generally more instantaneous than that of a larger one. He proved that in certain cases the duration of the principal part of the light was as low as one twenty-five-millionth part of a second of time. That is a statement which probably conveys very little of its real meaning. A million seconds is about twelve days and nights. Twenty-five million seconds is nearly a year. So that the time occupied by the spark in Prof. Rood's experiment is about the same fraction of one second that one second is of a year. In many other cases the duration was somewhat greater; but in all his experiments it was well under the one-millionth part of a second. In certain cases you may have multiple sparks. I do not refer to the oscillating discharges of which Prof. Lodge gave us so interesting an account last year; Prof. Rood's multiple discharge was not of that character. It consisted of several detached overflows of his Leyden jar when charged by the Rhumkorff coil. One number mentioned for the total duration was one six-thousandth part of a second; but the individual discharges had the degree of instantaneity of which I have spoken.

It is not a difficult matter to adapt the electrical spark to instantaneous photography. We will put the lantern into its proper position, excite the electric sparks within it, causing them to be condensed by the condenser of the lantern on to the photographic lens. We will then put the object in front of the lantern-condenser, remove the cap from the lens, expose the plate to the spark when it comes, and thus obtain an instantaneous view of whatever may be going on. I propose to go through the operation of taking such a photograph presently. I will not attempt any of the more difficult things of which I shall speak, but will take a comparatively easy subject—a stream of bubbles of gas passing up through a liquid. In order that you may see what this looks like when observed in the ordinary way, we have arranged it here for projection upon the screen. [Experiment.] The gas issues from the nozzle, and comes up in a stream, but so fast that you cannot fairly see the bubbles. If, however, we take an instantaneous picture, we shall find that the stream is decomposed into its constituent parts. We arrange the trough of liquid in front of the lantern which contains the spark-making apparatus—[Experiment]—and we will expose a plate, though I hardly expect a good result in a lecture. A photographer's lamp provides some yellow light to enable us to see when other light is excluded. There goes the spark; the plate is exposed, and the thing is done. We will develop the plate, and see what it is good for; and if it turns out fit to show, we will have it on the screen within the hour.

In the meantime, we will project on the screen some slides taken in the same way and with the same subject. [Photograph shown.] That is an instantaneous photograph of a stream of bubbles. You see that the bubbles form at the nozzle from the very first moment, contrasting in that respect with the behaviour of jets of water projected into air (Fig. 1).

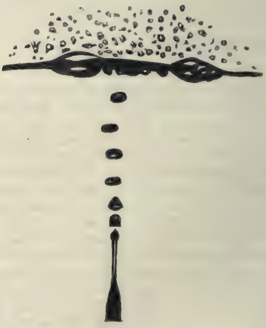

Fig. 1.

The latter is our next subject. This is the reservoir from which the water is supplied. It issues from a nozzle of drawn-out glass, and at the moment of issue it consists of a cylindrical body of water. The cylindrical form is unstable, however, and the water rapidly breaks up into drops, which succeed one another so rapidly that they can hardly be detected by ordinary vision. But by means of instantaneous photography the individual drops can be made evident. I will first project the jet itself on the screen, in order that you may appreciate the subject which we shall see presently represented by photography. [Experiment.] Along the first part of its length the jet of water is continuous. After a certain point it breaks into drops, but you cannot see them because of their rapidity. If we act on the jet with a vibrating body, such as a tuning-fork, the breaking into drops occurs still earlier, the drops are more regular, and assume a curious periodic appearance, investigated by Savart. I have some photographs of jets of that nature. Taken as described, they do not differ much in appearance from those obtained by Chichester Bell, and by Mr. Boys. We get what we may regard as simply shadows of the jet obtained by instantaneous illumination; so that these photographs show little more than the outlines of the subject. They show a little more, on account of the lens-like action of the cylinder and of the drops. Here we have an instantaneous view of a jet similar to the one we were looking at just now (Fig. 2). This is the continuous part; it gradually ripples itself as it comes along; the ripples increase; then the contraction becomes a kind of ligament connecting consecutive drops; the ligament next gives way, and we have the individual drops completely formed. The small points of light are the result of the lens-like action of the drops. [Other instantaneous views also shown.]

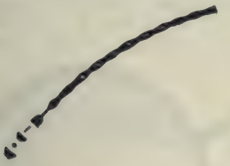

Fig. 2.

The pictures can usually be improved by diffusing somewhat the light of the spark with which they are taken. In front of the ordinary condensing lens of the magic lantern we slide in a piece of ground glass, slightly oiled, and we then get better pictures showing more shading. [Photograph shown.] Here is one done in that way; you would hardly believe it to be water resolved into drops under the action of a tremor. It looks more like mercury. You will notice the long ligament trying to break up into drops on its own account, but not succeeding (Fig. 3).

There is another, with the ligament extremely prolonged. In this case it sometimes gathers itself into two drops (Fig. 4).

[A number of photographs showing slight variations were exhibited.]

The mechanical cause of this breaking into drops is, I need hardly remind you, the surface tension or capillary force of the liquid surface. The elongated cylinder is an unstable form, and tends to become alternately swollen and contracted. In speaking on this subject I have often been embarrassed for want of an appropriate word to describe the condition in question. But a few days ago, during a biological discussion, I found that there is a recognized, if not a very pleasant, word. The cylindrical jet may be said to become *varicose*, and the varicosity goes on increasing with time, until eventually it leads to absolute disruption.

There is another class of unstable jets presenting many points of analogy with the capillary ones, and yet in many respects quite distinct from them. I refer to the phenomena of sensitive flames. The flame, however, is not the essential part of the matter, but rather an indicator of what has happened. Any jet of fluid playing into a stationary environment is sensitive, and the most convenient form for our present purpose is a jet of coloured in uncoloured water. In this case we shall use a solution of permanganate of potash playing into an atmosphere of other water containing acid and sulphate of iron, which exercises a decolourising effect on the permanganate, and so retards the general clouding up of the whole mass by accumulation of colour. [Experiment.] Mr. Gordon will release the clip, and we shall get a jet of permanganate playing into the liquid. If everything were pefectly steady, we might see a line of purple liquid extending to the bottom of the trough; but in this theatre it is almost impossible to get anything steady. The instability to which the jet is subject now manifests itself, and we get a breaking away into clouds something like smoke from chimneys. A heavy tuning-fork vibrating at ten to the second acts upon it with great advantage, and regularizes the disruption. A little more pressure will increase the instability, and the jet goes suddenly into confusion, although at first, near the nozzle, it is pretty regular.

It may now be asked "What is the jet doing?" That is just the question which the instantaneous method enables us to answer. For this purpose the permanganate which we have used to make the jet visible is not of much service. It is too transparent to the photographic rays, and so it was replaced by bichromate of potash. Here the opposite difficulty arises; for the bichromate is invisible by the yellow light in which the adjustments have to be made. I was eventually reduced to mixing the two materials together, the one serving to render the jet visible to the eye and the other to the photographic plate. Here is an instantaneous picture of such a jet as was before you a moment ago, only under the action of a regular vibrator. It is *sinuous*, turning first in one direction and then in the other. The original cylinder, which is the natural form of the jet as it issues from the nozzle, curves itself gently as it passes along through the water. It thus becomes sinuous, and the amount of the sinuosity increases, until in some cases the consecutive folds come into collision with one another. [Several photographs of sinuous jets were shown, two of which are reproduced in Figs. 5 and 6.]

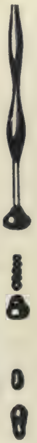

Fig. 3.

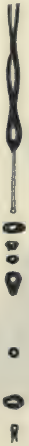

Fig. 4.

The comparison of the two classes of jets is of great interest. There is an analogy as regards the instability, the vibrations caused by disturbance gradually increasing as the distance from the nozzle increases; but there is a great difference as to the nature of the deviation from the equilibrium condition, and as to the kind of force best adapted to bring it about. The one gives way by becoming varicose; the other by becoming sinuous. The only forces capable of producing varicosity are symmetrical forces, which act alike all round. To produce sinuosity, we want exactly the reverse—a force which acts upon the jet transversely and unsymmetrically.

I will now pass on to another subject for instantaneous photography—namely, the soap film. Everybody knows that if you blow a soap bubble it will break—generally before you wish. The process of breaking is exceedingly rapid, and difficult to trace by the unaided eye. If we can get a soap film on this ring, we will project it upon the screen and then break it before your eyes, so as to enable you to form your own impressions as to the rapidity of the operation. For some time it has been my ambition to photograph a soap bubble in the act of breaking. I was prepared for difficulty, believing that the time occupied was less than the twentieth of a second. But it turns out to be a good deal less even than that. Accordingly the subject is far more difficult to deal with than are those jets of water or coloured liquids which one can photograph at any moment that the spark happens to come.

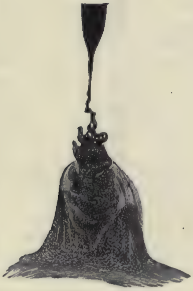
FIG. 5.

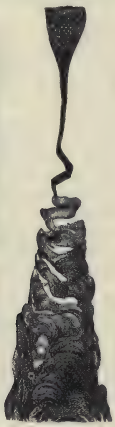
FIG. 6.

There is the film, seen by reflected light. One of the first difficulties we have to contend with is that it is not easy to break the film exactly when we wish. We will drop a shot through it. The shot has gone through, as you see, but it has not broken the film; and when the film is a thick one, you may drop a shot through almost any number of times from a moderate height without producing any effect. You would suppose that the shot in going through would necessarily make a hole, and end the life of the film. The shot goes through, however, without making a hole. The operation can be traced, not very well with a shot, but with a ball of cork stuck on the end of a pin, and pushed through. A dry shot does not readily break the film; and as it was necessary for our purpose to effect the rupture in a well-defined manner, here was a difficulty which we had to overcome. We found, after a few trials, that we could get over it by wetting the shot with alcohol.

We will try again with dry shot. Three shots have gone through and nothing has happened. Now we will try one wetted with alcohol, and I expect it will break the film at once. There! it has gone!

The apparatus for executing the photography of a breaking soap film will of necessity be more complicated than before, because we have to time the spark exactly with the breaking of the film. The device I have used is to drop two balls simultaneously, so that one should determine the spark and the other rupture the film. The most obvious plan was to hang iron balls to two electro-magnets, and cause them to drop by breaking the circuit, so that both were let go at the same moment. The method was not quite a success, however, because there was apt to be a little hesitation in letting go the balls. So we adopted another plan. The balls were not held by electro-magnetism, but by springs (Fig. 8) pressing laterally, and these were pulled off by electro-magnets. The proper moment for putting down the key and so liberating the balls, is indicated by the tap of the beam of an attracted disk electrometer as it strikes against the upper stop. One falling ball determines the spark, by filling up most of the interval between two fixed ones submitted to the necessary electric pressure. Another ball, or rather shot, wetted with alcohol, is let go at the same moment, and breaks the film on its passage through it. By varying the distances dropped through, the occurrence of one event may be adjusted relatively to the other. The spark which passes to the falling ball is, however, not the one which illuminates the photographic plate. The latter occurs within the lantern, and forms part of a circuit in connection with the *outer* coatings of the Leyden jars,[1] the whole arrangement being similar to that adopted by Prof. Lodge in his experiments upon alternative paths of discharge. Fig. 8 will give a general idea of the disposition of the apparatus. [Several photographs of breaking films were shown upon the screen; one of these is reproduced in Fig. 7.][2]

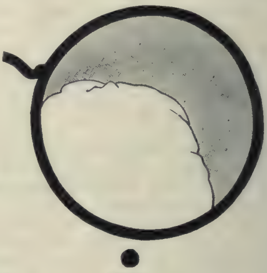
FIG. 7.

This work proved more difficult than I had expected; and the evidence of our photographs supplies the explanation—namely, that the rupture of the film is an extraordinarily rapid operation. It was found that the whole difference between being too early and too late was represented by a displacement of the falling ball through less than a diameter, viz. $\frac{1}{4}$ inch nearly. The drop which we gave was about a foot. The speed of the ball would thus be about 100 inches per second; therefore the whole difference between being too soon and too late is represented by $\frac{1}{300}$ second. Success is impossible, unless the spark can be got to occur within the limits of this short interval.

Prof. Dewar has directed my attention to the fact that Dupré, a good many years ago, calculated the speed of rupture of a film. We know that the energy of the film is in proportion to its area. When a film is partially broken, some of the area is gone, and the corresponding potential energy is expended in generating the velocity of the thickened edge, which bounds the still unbroken portion. The speed, then, at which the edge will go depends upon the thickness of the film. Dupré took a rather extreme case, and calculated a velocity of 32 metres per second. Here, with a greater thickness, our velocity was, perhaps, 16 yards a second, agreeing fairly well with Dupré's theory.

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[1] In practice there were two sets of three jars each.

[2] The appearance of the breaking bubble, as *seen* under instantaneous illumination, was first described by Marangoni and Stephanelli, *Nuovo Cimento*, 1873.

I now pass on to another subject with which I have lately been engaged—namely, the connection between aperture and the definition of optical images. It has long been known to astronomers and to those who study optics that the definition of an optical instrument is proportional to the aperture employed; but I do not think that the theory is as widely appreciated as it should be. I do not know whether, in the presence of my colleague, I may venture to say that I fear the spectroscopists are among the worst sinners in this respect. They constantly speak of the dispersion of their instruments as if that by itself could give any idea of the power employed. You may have a spectroscope of any degree of dispersion, and yet of resolving power insufficient to separate even the D lines. What is the reason of this? Why is it that we cannot get as high a definition as we please with a limited aperture? Some people say that the reason why large telescopes are necessary is, because it is only by their means that we can get enough light. That may be in some cases a sufficient reason, but that it is inadequate in others will be apparent, if we consider the case of the sun. Here we do not want more light, but rather are anxious to get rid of a light already excessive. The principal *raison d'être* of large telescopes is, that without a large aperture definition is bad, however perfect the lenses may be. In accordance with the historical development of the science of optics, the student is told that the lens collects the rays from one point to a focus at another; but when he has made further advance in the science he finds that this is not so. The truth is that we are in the habit of regarding this subject in a distorted manner. The difficulty is, not to explain why optical images are imperfect, no matter how good the lens employed, but rather how it is that they manage to be as good as they are. In reality the optical image of even a mathematical point has a considerable extension; light coming from one point cannot be concentrated into another point by any arrangement. There must be diffusion, and the reason is not hard to see in a general way. Consider what happens at the mathematical focus, where, if anywhere, the light should all be concentrated. At that point all the rays coming from the original radiant point arrive in the same phase. The different paths of the rays are all rendered optically equal, the greater actual distance that some of them have to travel being compensated for in the case of those which come through the centre by an optical retardation due to the substitution of glass for air; so that all the rays arrive at the same time.[1] If we take a point not quite at the mathematical focus but near it, it is obvious that there must be a good deal of light there also. The only reason for any diminution at the second point lies in the discrepancies of phase which now occur; and these can only enter by degrees. Once grant that the image of a mathematical point is a diffused patch of light, and it follows that there must be a limit to definition. The images of the com-

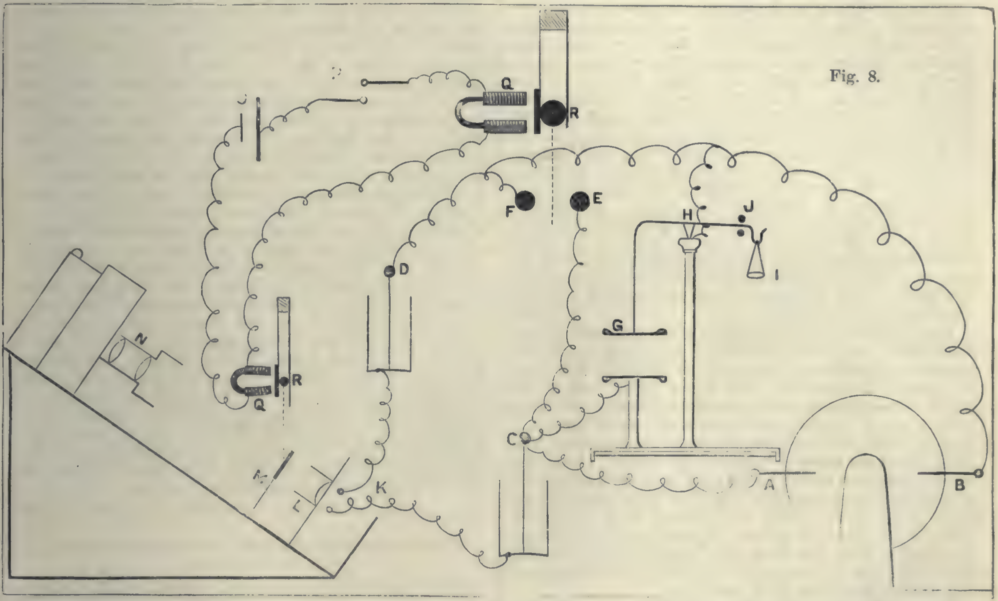

Fig. 8.

A, B, Electrodes of Wimshurst machine.
C, D, Terminals of interior coatings of Leyden jars.
E, F, Balls on insulating supports between which the discharge is taken.
G, Attracted disk of electrometer.
H, Knife edge. I, Scale pan.
J, Stops limiting movement of beam.
K, Sparking balls in connection with exterior coatings of jars. [These exterior coatings are to be joined by an imperfect conductor, such as a table.]
L, Lantern condenser. M, Soap film.
N, Photographic camera. O, Daniell cell.
P, Key. Q, Electro-magnets. R, Balls.

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[1] On this principle we may readily calculate the focal lengths of lenses without use of the law of sines (see *Phil. Mag.*, December 1879).

ponents of a close double point will overlap; and if the distance between the centres do not exceed the diameter of the representative patches of light, there can be no distinct resolution. Now their diameter varies inversely as the aperture; and thus the resolving power is directly as the aperture.

My object to-night is to show you by actual examples that this is so. I have prepared a series of photographs of a grating consisting of parallel copper wires separated by intervals equal to their own diameter, and such that the distance from centre to centre is $\frac{1}{10}$ inch. The grating was backed by a paraffin lamp and large condensing lens; and the photographs were taken in the usual way, except that the lens employed was a telescopic object-glass, and was stopped by a screen perforated with a narrow adjustable slit, parallel to the wires.[1] In each case the exposure was inversely as the aperture employed. The first [thrown upon the screen] is a picture done by an aperture of eight hundredths of an inch, and the definition is tolerably good. The next, with six hundredths, is rather worse. In the third case, I think that everyone can see that the definition is deteriorating; that was done by an aperture of four hundredths of an inch. The next is one done by an aperture of three hundredths of an inch, and you can see that the lines are getting washed out. In focussing the plate for this photograph I saw that the lines had entirely disappeared, and I was surprised, on developing the plate, to find them still visible. That was in virtue of the shorter wave-length of the light operative in photography as compared with vision. In the last example, the aperture was only two-and-a-half hundredths of an inch, and the effect of the contraction has been to wash away the image altogether, although, so far as ordinary optical imperfections are concerned, the lens was acting more favourably with the smaller aperture than with the larger ones.

This experiment may be easily made with very simple apparatus; and I have arranged that each one of my audience may be able to repeat it by means of the piece of gauze and perforated card which have been distributed. The piece of gauze should be placed against the window so as to be backed by the sky, or in front of a lamp provided with a ground-glass or opal globe. You then look at the gauze through the pin-holes. Using the smaller hole, and gradually drawing back from the gauze, you will find that you lose definition and ultimately all sight of the wires. That will happen at a distance of about $4\frac{1}{2}$ feet from the gauze. If, when looking through the smaller hole, you have just lost the wires, you shift the card so as to bring the larger hole into operation, you will see the wires again perfectly.

That is one side of the question. However perfect your lens may be, you cannot get good definition if the aperture is too much restricted. On the other hand, if the aperture is much restricted, then the lens is of no use, and you will get as good an image without it as with it.

I have not time to deal with this matter as I could wish, but I will illustrate it by projecting on the screen the image of a piece of gauze as formed by a narrow aperture parallel to one set of wires. There is no lens whatever between the gauze and the screen. [Experiment.] There is the image—if we can dignify it by such a name—of the gauze as formed by an aperture which is somewhat large. Now, as the aperture is gradually narrowed, we will trace the effect upon the definition of the wires parallel to it. The definition is improving; and now it looks tolerably good. But I will go on, and you will see that the definition will get bad again. Now, the aperture has been further narrowed, and the lines are getting washed out. Again, a little more, and they are gone. Perhaps you may think that the explanation lies in the faintness of the light. We cannot avoid the loss of light which accompanies the contraction of aperture, but to prove that the result is not so to be explained, I will now put in a lens. This will bring the other set of wires into view, and prove that there was plenty of light to enable us to see the first set if the definition had been good enough. Too small an aperture, then, is as bad as one which is too large; and if the aperture is sufficiently small, the image is no worse without a lens than with one.

What, then, is the best size of the aperture? That is the important question in dealing with pin-hole photography. It was first considered by Prof. Petzval, of Vienna, and he arrived at the result indicated by the formula, $2r^2 = f\lambda$, where $2r$ is the diameter of the aperture, λ the wave-length of light, and f the focal length, or rather simply the distance between the aperture and the screen upon which the image is formed.

His reasoning, however, though ingenious, is not sound, regarded as an attempt at an accurate solution of the question. In fact it is only lately that the mathematical problem of the diffraction of light by a circular hole has been sufficiently worked out to enable the question to be solved. The mathematician to whom we owe this achievement is Prof. Lommel. I have adapted his results to the problem of pin-hole photography. [A series of curves (*Philosophical Magazine*, February 1891), were shown, exhibiting to the eye the distribution of illumination in the images obtainable with various apertures.] The general conclusion is that the hole may advantageously be enlarged beyond that given by Petzval's rule. A suitable radius is $r = \sqrt{(f\lambda)}$.

I will not detain you further than just to show you one application of pin-hole photography on a different scale from the usual. The definition improves as the aperture increases; but in the absence of a lens the augmented aperture entails a greatly extended focal length. The limits of an ordinary portable camera are thus soon passed. The original of the transparency now to be thrown upon the screen was taken in an ordinary room, carefully darkened. The aperture (in the shutter) was 0·07 inch, and the distance of the 12 × 10 plate from the aperture was 7 feet. The resulting picture of a group of cedars shows nearly as much detail as could be seen direct from the place in question.

THE SMITHSONIAN ASTRO-PHYSICAL OBSERVATORY.

THE Smithsonian Institution, as we have already announced, has established as one of its departments a Physical Observatory which, with the instruments, has been supplied from the Smithsonian Fund. It occupies at present a temporary structure, though funds have been subscribed for a permanent building when Congress shall provide a suitable site. For the maintenance of the Observatory an appropriation has been made by Congress which became available on July 1. The actual instrumental work of the new Observatory will necessarily devolve largely upon a senior and a junior assistant, who can devote their entire time to research, and it is hoped that with the improved apparatus it will be possible to prosecute advantageously investigations in telluric and astro-physics, and particularly those with the bolometer in radiant energy.

In accepting the position of assistant secretary of the Smithsonian Institution in 1887, Mr. Langley retained the Directorship of the Observatory at Allegheny for the purpose of completing the researches begun there, and after his appointment as Secretary of the Institution, he still continued the titular Directorship, though but a limited amount of time could be spared from his official duties at the capital. With the completion of the equipment of the little Observatory at Washington, he, however, formally resigned, on April 30, the Directorship at Allegheny, which he had held since 1887; and he will, so far as his administrative occupations permit, give personal attention to the general direction of the investigations.

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[1] The distance between the grating and the telescope lens was 12 feet 3 inches.

The class of work which is referred to does not ordinarily involve the use of the telescope, and that which is contemplated is quite distinct from what is carried on at present at any other Observatory in the United States. The work for which the older Government Observatories at Greenwich, Paris, Berlin, and Washington were founded, and in which they are now chiefly engaged, is the determination of relative positions of heavenly bodies, and our own place with reference to them. Within the past twenty years all these Governments, except that of the United States, have established astrophysical Observatories, as they are called, which are, as is well known, engaged in the study of the heavenly bodies as distinct from their positions—in determining, for instance, not where, but what, the sun is, how it affects terrestrial climate and life, and how it may best be studied for the purposes of the meteorologist, and for other uses of an immediately practical nature.

The new Observatory is established for similar purposes. Its outfit includes a very large siderostat (recently completed by Grubb), which is mounted in such a way as to throw a beam of light horizontally in the meridian. It is intended to carry a mirror of 20 inches diameter, and is perhaps the most massive and powerful instrument of its kind ever constructed Within the dark room is mounted another large instrument—the spectrobolometer—which is, in effect, a large spectroscope with 20-inch circle reading to 5 seconds of arc, specially designed for use with the bolometer. It was made by William Grunow and Son, of New York, as the outcome of Mr. Langley's experience with smaller apparatus during his earlier investigations. The most important part of the instrumental equipment is completed by specially designed galvanometers, scales, and a peculiar resistance box; and these three instruments, used in conjunction with the bolometer, and perhaps with the aid of photography, will be employed in the investigations upon light, heat, and radiant energy in general, for which the Observatory is primarily intended, though some departments of terrestrial physics may also receive attention.

THE NEW GALLERY OF BRITISH ART.

We believe that the Committee appointed by the Corporation to consider the question of the grant of a site on the Embankment for the new gallery will soon make its report. The *Pall Mall Gazette* of Tuesday says:—"There is a vacant piece of just one acre at Blackfriars, on the land acquired some years ago and cleared of the old City gas-works by the Corporation. This land originally cost some £260,000; and on portions of it have been erected the City of London School and Sion College. The value of the entire holding has increased to at least £550,000; so that if the proposed piece, which is valued at about £120,000, were made over by the Corporation for the Art Gallery, the City would still be a gainer of some £170,000 by the transaction."

In the meantime, public opinion is rapidly growing, not only in favour of some of our national buildings devoted to art finding a home in the City, but also against the site at South Kensington—bought for scientific purposes, and required to meet existing needs—being diverted from its proper and natural use.

Both these views are expressed in the following Memorial, which, although circulated chiefly among men of science during the last few days, contains the names of many representative men in other branches. It has been transmitted to the Lord Mayor during the present week.

Memorial to the Right Honourable the Lord Mayor of London.

We, the undersigned, having heard that there is a possibility of the City of London finding a site on the Embankment for the National Gallery of British Art, which a munificent donor has proposed to build, venture to approach Your Lordship with our earnest request that you will yourself support, and use your best endeavours to urge upon the City authorities, the very great importance of giving effect to this proposal.

The memorial already presented to the Prime Minister will have made Your Lordship aware of the many strong objections, from the scientific point of view, to the site which was suggested for the gallery in the first instance.

It is unnecessary for us, therefore, to say more on this subject, except to remark that the greatest city in the world must be the first to suffer if, from any cause, the proper presentation of science and means for its study by its citizens are in any way crippled.

By affording a site on the Embankment, Your Lordship and the authorities you represent will be the means of preventing the lamentable result to which we have referred, and you and they will earn the gratitude of all interested in scientific progress, as well as confer a great boon on the art-loving public.

Among the signatories of the Memorial are the following:—

Sir William Thomson, D.C.L., LL.D., President Royal Society, Professor of Natural Philosophy, Glasgow.
Dr. John Evans, LL.D., F.R.S., Treasurer Royal Society.
Lord Rayleigh, F.R.S., Secretary Royal Society.
M. Foster, M.D., F.R.S., Secretary Royal Society.
Thomas H. Huxley, F.R.S., Dean of the Royal College of Science, London.
Lieut.-General R. Strachey, F.R.S., C.I.E., Chairman Meteorological Council.
Nevil Story Maskelyne, F.R.S., M.P., Professor of Mineralogy, University of Oxford.
Sir John Lubbock, Bart., M.P., F.R.S., Chairman London County Council, Past-President British Association.
Sir Richard Quain, Bart., M.D., F.R.S.
Sir William Roberts, F.R.S., M.D.
William Crookes, F.R.S., President Institute Electrical Engineers.
William Summers, M.P.
J. W. L. Glaisher, M.A., F.R.S.
Alfred Newton, F.R.S., Professor of Zoology, University of Cambridge.
T. E. Thorpe, F.R.S., Professor of Chemistry, Royal College of Science, Treasurer Chemical Society.
John W. Judd, F.R.S., Professor of Geology, Royal College of Science.
William Huggins, D.C.L., F.R.S., President-Elect of the British Association.
Sir G. G. Stokes, Bart., M.P., Past-President Royal Society, Lucasian Professor, University of Cambridge.
Sir Henry E. Roscoe, LL.D., F.R.S., M.P., Past-President British Association.
W. Grylls Adams, F.R.S., Professor of Physics, King's College, Past-President Physical Society.
J. Fletcher Moulton, Q.C., F.R.S.
E. A. Schäfer, F.R.S., Professor of Physiology, University College, London.
Herbert McLeod, F.R.S., Professor of Chemistry, Cooper's Hill.
Hugo Müller, F.R.S., Past-President Chemical Society.
Arthur W. Rücker, F.R.S., Professor of Physics, Royal College of Science, London, Treasurer Physical Society.
William Cawthorne Unwin, F.R.S., Professor of Engineering, City and Guilds of London Institute.
W. E. Ayrton, F.R.S., Professor of Physics, City and Guilds of London Institute, President Physical Society.

O. Henrici, F.R.S., Professor of Mathematics, City and Guilds of London Institute.
Henry E. Armstrong, F.R.S., Professor of Chemistry, City and Guilds of London Institute, Secretary Chemical Society.
R. B. Clifton, M.A., F.R.S., Professor of Natural Philosophy, University of Oxford.
J. Burdon Sanderson, F.R.S., Professor of Physiology, Oxford.
William Odling, F.R.S., Professor of Chemistry, Oxford.
William Esson, F.R.S., Oxford.
Edward B. Poulton, F.R.S., Oxford.
E. Ray Lankester, F.R.S., Deputy Professor of Anatomy, Oxford.
G. Carey Foster, F.R.S., Professor of Physics, University College, London; Past President Physical Society.
J. Hopkinson, F.R.S., Wheatstone Professor of Electricity, King's College, London.
Captain Abney, C.B., F.R.S.
The Very Rev. G. G. Bradley, D.D., C.B., Dean of Westminster.
William Black.
Lewis Morris.
W. H. M. Christie, F.R.S., Astronomer-Royal.
William Morris.
Walter Crane.
W. J. Russell, F.R.S., Professor of Chemistry, St. Bartholomew's Hospital, Past-President Chemical Society.
The Lord Tennyson, F.R.S., Poet Laureate.
Hallam Tennyson.

CARDINAL HAYNALD.

THE death of Cardinal Haynald, Archbishop of Kalocsa, is announced in the daily papers as having taken place on Saturday, the 4th inst. It was not an unexpected event, as his health had been gradually getting worse for some two or three years. Last year he celebrated the jubilee of his priesthood, and Dr. A. Kanitz, Professor of Botany in the University of Klausenburg, made it the occasion of publishing a eulogy on him as a botanist. This was translated into French by Prof. E. Martens, of Louvain. Although an excellent botanist, Cardinal Haynald was better known as a patron of botany than as a contributor to botanical literature. For the following particulars of his life and work we are mainly indebted to Dr. Kanitz's memoir.

Cardinal Haynald was born about 1816. His taste for botany was inherited from his father, who himself possessed a fine herbarium. During his stay at Vienna, in the Augustinæum, a theological college, he became acquainted with Edward Fenzl, then assistant curator of the botanical department of the Court, under whose tuition his botanical studies took a more practical shape. His priestly duties, however, did not allow him to follow his favourite study until he was appointed Bishop of Transylvania, when he began to investigate the flora of this country with indefatigable zeal. He became Archbishop of Karthago, and afterwards of Kalocsa, and after the accession of Leo the Tenth to the Papal chair, a Cardinal. He was a long time a prominent member of the Hungarian House of Magnates, and from 1873 also a member of the Royal Hungarian Academy of Science. Although always overburdened by the sacerdotal, political, and social duties of his high position, he found time to continue his botanical studies. He published only a few botanical papers, partly on Hungarian plants, and partly biographical sketches of botanists with whom he was more intimately acquainted, as Fenzl, Parlatore, and Boissier. His greatest merit, however, from a scientific point of view, was the assistance which he gave to botanical studies in Hungary by establishing a great private herbarium, which he placed in the most liberal way at anybody's disposal, and by the magnanimous generosity with which he always supported botanical enterprise, both in Hungary and abroad. The herbarium at his residence at Kalocsa was not only the richest in Hungary, but one of the largest private collections on the Continent. It was largely formed by the purchase of the herbaria of Heuffel, Schott, Kotschy, and Sodiro. Besides these and the plants collected by himself, he acquired most of the collections which have been distributed by subscription.

Hungary loses in Cardinal Haynald one of her greatest patriots, who was an honour to his profession, as well as to science, of which he was always a generous benefactor. Schur named after him a genus of grasses, founded on *Secale villosum*, Linn., which is reduced by Bentham and Hooker to Agropyrum, and Kanitz a genus of Lobeliaceæ.

OXFORD SUMMER MEETING OF UNIVERSITY EXTENSION STUDENTS.

THE process by which University Extension is carried throughout the country and made a vehicle for the further education of the adult student is well known, and is gradually becoming more and more appreciated in proportion as those who are responsible for the method improve the lines on which it is carried out. The machinery employed embraces lectures, classes, travelling libraries, &c., but one element vitally necessary to the University student is not supplied by these aids. This element is that of residence, and it was a happy suggestion on the part of the originators to propose that, for one month in the Long Vacation, arrangements should be made by which those who have profited by being brought into contact with a University lecturer should enjoy the additional advantage of being brought under the charm that haunts the colleges and cloisters of Oxford and Cambridge.

The Oxford summer meeting commences on July 31, and is continued throughout the month of August; but, for the benefit of students who are unable to be present during so long a period, the course is divided into two sections, the second commencing on August 12. It has been found desirable to remove as far as possible the fragmentary and isolated character of the lectures given at these meetings, and therefore, while the course will be complete and independent in itself, it will also form the first part of a cycle of study which for its full development will embrace a period of four summers.

That these lectures propose something more than to add piquancy to an agreeable picnic will be shown from the following slight sketch of the subjects treated—and treated by authorities of acknowledged reputation. To take the lectures on natural science first: in physiology, Mr. Poulton will discuss the recent criticisms of Weismann's theory of heredity, and Mr. Gotch will lecture on the functions of the heart. In chemistry, Prof. Odling lectures on the benzene ring, and under the supervision of Mr. Marsh a course of practical chemistry will be conducted in the laboratory of the University Museum. In geology, a course of practical instruction will be given by Prof. Green and Mr. Badger, to include excursions in the neighbourhood of Oxford. A class in practical astronomy will be welcomed at the University Observatory; while electricity finds an able exponent in Mr. G. J. Burch. But the distinguishing feature of this meeting is the attention given to agricultural science "designed for agricultural audiences under County Council schemes." This designation seems somewhat vague, and it will be very interesting to see the character of the audience attracted by this title. Four lectures are offered: the first is entitled, "The application of Science to the art of Agriculture." This description is sufficiently wide, but does not indicate whether the lecture is intended as a sample of those which State-aided Board schools in agricultural districts might well offer to lads who have passed through the successive standards, or as one addressed to the sons of farmers, and supplying that form of instruction which it is the duty of agricultural colleges to impart. Another lecture is offered on the management of poultry. This is more definite and more hopeful; and when we remember that the students who come up for these summer meetings are, for the most part, ladies, who can well be supposed to take an intelligent interest in this part of farming operations, we must admit that the subject is well chosen. Manures of various characters form the subject of the other two lectures, and will be doubtless of a sufficiently technical character.

The literature and history lectures are of special interest, and by the combination of many lecturers are made to cover with great completeness the mediæval period. Mr. Frederic Harrison gives, as an inaugural lecture, a survey of the thirteenth century, and strikes the keynote of this section; while in the entire course, which embraces some sixty lectures, we meet the names of Prof. Dicey, of Mr. York Powell, of Mr. Boas, and a host of others, affording alike a sufficient guarantee for the excellence of the work, and a happy augury for the success of the meeting.

THE PROPOSED TEACHING UNIVERSITY FOR LONDON.

On Monday, at the Council Office in Downing Street the Universities Committee of the Privy Council, consisting of the Lord President of the Council (Viscount Cranbrook), the Earl of Selborne, Lord Monk Bretton, Lord Basing, and Lord Sandford, reassembled for the purpose of giving their decision on the petition of King's and University Colleges for the grant of a charter for the establishment of a Teaching University for London.

The Earl of Selborne, in giving the opinion of their Lordships upon the draft charter of the proposed University, said, with regard to the opposition of the existing University of London, that some of the objections made might be treated as disallowed. It had been understood by their Lordships that a minimum course of two years' study at the new University would be required. If that was so, their Lordships were satisfied, and would say no more upon the point. The objections put forward by the medical faculty were generally disallowed. The word "London" would have to be omitted from the charter, but the University might be called either "the Albert University" or "the Metropolitan University." With regard to the suggestion that ten members of the Faculty of Medicine should be elected to the Council, their Lordships were of opinion that the medical schools should fill five places upon that body, or, if it were preferred, that each school should elect one member for the Medical Board of Study. If the Royal Colleges and the medical schools agreed to come in together, however, the number of members on the Council might be raised. Their Lordships did not approve of the proposed strength of the Council, and thought that four of the places might be accorded to the Faculty of Law. Teachers in any branch of science, their Lordships considered, should be admitted as members of the Science Faculty, and the six places on the Council which it was proposed to give to the Royal Colleges should be supplied according to the 29th paragraph of the Royal Commissioners' Report. If the medical schools and colleges declined to come in at first, provision ought to be made to allow them to do so in the future. Their Lordships thought that a place upon the Council might be given to the Apothecaries' Society, but they were not disposed to insist upon that being done. The view of their Lordships upon the question of honorary degrees was that no such degrees should be granted in medicine, and that the holding of an honorary degree should be no qualification for election to the Council. The ordinary degree in medicine should not be granted until the whole of the prescribed conditions had been fulfilled.

NOTES.

The decision of the Universities Committee of the Privy Council with regard to the proposed new University for London is one that might have been expected from a body of non-experts. It is hasty, and will give satisfaction to no one by whom the subject has been seriously considered. It may throw back the higher teaching in London for half a century.

Mr. Walter Besant, in an imaginary "Page from the Kaiser's Diary," notes that there are not to be seen at Court any of "the people who make the real greatness of the country —its traders, its manufacturers, its men of science, art, and literature." It has been remarked that in this respect the City Corporation, last Friday, followed the example of the Court, no representative of science, or literature, or art, as such, having been invited to the Guildhall banquet. It would have been better to follow the precedent set at the time of the Czar's visit, when a large number of the leading scientific men were asked to the reception at the Foreign Office, and were personally presented.

At the ensuing British Association meeting at Cardiff, it is proposed to hold in Section A, if possible in conjunction with Section G, a discussion on "Units and their Nomenclature," having special regard to the new electrical and magnetic units now becoming necessary for practical purposes.

The Secretary of State for India in Council has appointed, on the nomination of the Government of India, the following persons to represent it on the permanent governing body of the Imperial Institute, for the year ending April 30, 1892:—W. T. Thiselton-Dyer, C.M.G., F.R.S., Director, Royal Gardens, Kew; General James T. Walker, R.E., C.B., F.R.S., late Surveyor-General of India; John W. P. Muir-Mackenzie, Under-Secretary to the Government of India Revenue and Agricultural Department.

Sir J. D. Hooker has been elected a Foreign Member of the Academy of Sciences in Buda-Pesth.

The Secretary of State for the Colonies has appointed, on the nomination of Kew, Mr. C. A. Barber, late Scholar of Christ's College, Cambridge, and University Demonstrator in Botany, to be Superintendent of the recently created Agricultural Department of the Leeward Islands. The Superintendent will reside in Antigua, and will have the general supervision of the botanical stations at Antigua, Dominica, Montserrat, and St. Kitts-Nevis.

The Council of University College, Liverpool, have appointed Mr. Francis Gotch, of Oxford, to their new Chair of Physiology.

The Foreign Office has expressed the wish that the "Flora of Tropical Africa," prepared at Kew under the editorship of Prof. Oliver, and of which three volumes have appeared, should be continued and completed. It is calculated that four more volumes will be required, and the Treasury has sanctioned a scheme by which the necessary funds will be provided.

The Accademia dei Lincei of Rome has awarded to Prof Saccardo, of Padua, in acknowledgment of his labours in mycology, the Royal prize of 10,000 francs intended for the encouragement of morphological researches.

THE Government has appointed the Council of the Society of Arts as a Royal Commission to direct the formation of the British Section at the Chicago Exhibition. If we may judge from the preparations which are being made in America, the Exhibition is likely to be one of great splendour. One of its attractions will be a collection of objects relating to ethnology and archæology. This is being organized by Prof. Putnam.

A COMMITTEE, as we recently stated, has been appointed for the reorganization of the Natural History Museum in Paris. By some who interest themselves in the question it is proposed that the Museum should be made the only institution in Paris for the study of natural history. According to this scheme, all natural history chairs in the Sorbonne and elsewhere would be suppressed, while all chairs in the Museum which do not belong to natural history proper would also disappear. The professors would have to examine all candidates in natural science.

A COMMITTEE appointed by the Photographic Society of Great Britain has presented a report on the proposal that the photographic societies of the United Kingdom should unite more closely for the better promotion of their common interests. The Committee advises that it should be open to photographic societies to affiliate themselves to the Photographic Society of Great Britain; and suggestions are made as to the way in which affiliation should be effected.

THE fifth session of the Edinburgh Vacation Courses will begin on August 3. M. Espinas, Professor of Philosophy and Dean of the Faculty of Letters in the University of Bordeaux, has been charged by his Government to report upon the educational scheme and methods of these courses, and also desires to inquire into Scottish higher education generally. Dr. H. de Varigny, who will deliver a series of lectures on general biology, is also to report to the French Government on the University Extension movement. The expected presence of these and other foreigners has suggested the idea that it might be well to hold, at Edinburgh, a small informal Congress, or rather a short series of meetings, for the discussion of curricula, higher educational methods, and other questions of immediate interest. Particulars on this subject will shortly be announced.

THE Royal Society of Antiquaries of Ireland hold their general meeting in the Town Hall, Killarney, on August 11. Excursions are planned for every day, except Sunday, from August 11 to 20.

THE Royal Archæological Institute will hold its annual meeting at Edinburgh from August 11 to 18. Sir Herbert Maxwell will preside.

THE German Anthropological Society will hold its twenty-second annual meeting at Danzig, from August 3 to 5.

HER MAJESTY'S Commissioners for the Exhibition of 1851, assisted by a committee of gentlemen experienced in scientific education, have made the following appointments to Science Scholarships for the year 1891. The scholars have been *bonâ-fide* students of science for at least three years, and were nominated for the Scholarships by the authorities of their respective Universities or Colleges. The Scholarships are of the value of £150 a year, and are tenable for two years (subject to a satisfactory report at the end of the first year) in any University at home or abroad, or in some other institution to be approved of by the Commissioners. The scholars are to devote themselves exclusively to study and research in some branch of science the extension of which is important to the industries of the country. A Scholarship was offered to the University of Sydney, but the Council found themselves unable to nominate a suitable candidate. Nominating institution—University of Edinburgh, scholar—John Shields, institution to which scholar proposes to attach himself—University of Edinburgh and a Continental University, probably Leipzig; University of Glasgow, James H. Gray (*a*), University of Glasgow; University of St. Andrews, William Frew, University of Munich; Mason Science College, Birmingham, John Joseph Sudborough, University of Heidelberg; University College, Bristol, Frederick Benjamin Fawcett (*a*), University College, Bristol; Durham College of Science, Newcastle-on-Tyne, William M'Connell, jun. (*a*), Durham College of Science; Yorkshire College, Leeds, Harry Ingle, a German University, probably Wurzburg; University College, Liverpool, Robert Holt (*a*), University College, Liverpool; Owens College, Manchester, Thomas Ewan, Owens College, first year; University College, Nottingham, Edwin H. Barton (*b*), South Kensington; Firth College, Sheffield, Annie J. Hoyles (*a*), Firth College, Sheffield; University College of South Wales and Monmouthshire, Franke Herbert Parker, first year same College, second year a German University; Queen's College, Belfast, Benjamin Moore, University of Leipzig; Royal College of Science for Ireland, Frederick William Dunn, first year University of Glasgow, second year Berlin; M'Gill University, Montreal, Percy Norton Evans, University of Berlin, and probably other German Universities; University of Melbourne, William Huey Steele (*a*), University of Melbourne. (*a*) These scholars have been recommended to spend part of the term of scholarship at some other institution. (*b*) This appointment is conditional on the candidate passing examination for B.Sc. London.

THOSE who require power for electric lighting may be interested to know that Messrs. Priestman Brothers have a good account to give of the success of their oil-engine. Many orders have been received for engines varying in size from 1 to 25 actual h.p. for electric lighting, and Messrs. Priestman, in order to meet the growing demand, have largely extended their works.

ACCORDING to a telegram sent through Reuter's Agency from San Francisco, July 11, an enormous cavern in Josephine County, Oregon, at a point twelve miles north of California and forty from the coast, has been discovered. It has two openings, and contains many passages of great beauty. There are numbers of semi-transparent stalactites, several giant milk-white pillars, and a number of pools and streams of clear, cool water. A week was spent in exploring the cavern, and innumerable passages and chambers were discovered. On penetrating one of these passages for a distance of several miles, the exploring party came across a lake of clear water and a waterfall thirty feet high. All kinds of grotesque figures were found in the various chambers; but the only signs of animal life were discovered a short distance from the entrance, where a few bones were found, indicating that bears had carried their prey there. The cavern appears to be fully as large as the Mammoth Cave in Kentucky.

DR. D. PRAIN, Curator of the Calcutta Herbarium, has published in the Journal of the Asiatic Society of Bengal, and separately, a memoir on new Indian *Labiatæ*. Nearly fifty species, belonging to upwards of twenty genera, are added to those described in the "Flora of British India." They are mostly from frontier extensions of the Empire, some from the east, some from the west; and nearly half of the species are new to science. Specially interesting among these is Prain's new genus *Microtæna*, founded upon the *Plectranthus Patchouli*, Clarke—a plant cultivated in Assam; and a second species, collected by Griffith, probably in Assam. The first has since been found wild in Muneypore, Burma, Tonkin, and South-Eastern China. Two very distinct species of the same genus have also been recently discovered by Dr. A. Henry, in Central China.

THE new "Flora of France," which is being prepared by Prof. G. Bonnier, with the assistance of a number of botanists, will be published under the auspices of the Minister of Public Instruction of France.

The annual publication of the very useful "Correspondance botanique" ceased with the death of its editor, Prof. E. Morren, of Liège. With the aim of supplying its place, the International Library, 4 Rue de la Sorbonne, Paris, has now issued a "Nouvelle Correspondance botanique: liste des botanistes de tous les pays, et des établissements, sociétés, et journaux de botanique."

PRINCE ROLAND BONAPARTE has issued, at his own expense, a handsome book on Corsica, recording his travels and the history of the island. He also gives a full bibliography relating to the subject.

A NEW quarterly scientific journal has made its first appearance in Paris, under the title *Revue des Sciences naturelles de l'Ouest*, devoted to the interests of zoology, botany, geology, mineralogy, anthropology, embryology, and teratology.

A MONTHLY journal of natural science, which may have many opportunities of doing good work, has just been started in Malta. It is called *The Mediterranean Naturalist*, and is edited by Mr. John H. Cooke, F.G.S.

THE "Dictionnaire d'Agriculture," by J. A. Barral and H. Sagnier, will soon be completed. Vol. iv. is nearly ready, and will be quickly followed by Vol. v.

A NEW edition of the Great Eastern Railway Company's "Tourist Guide to the Continent," edited by Mr. Percy Lindley, has been published. New editions of Mr. Lindley's "Walks in the Ardennes" and "Walks in Epping Forest" have also been published; and he has compiled two other useful little hand-books, "Walks in Holland" and "Holidays in Belgium."

MESSRS. GUY AND CO., Cork, send us "Guy's South of Ireland Pictorial Guide," in which are described and illustrated much fine scenery and various things interesting to students of natural history and archæology.

MESSRS. DULAU AND CO. have issued a catalogue of the works on geology which they are offering for sale.

THE results of an investigation concerning the cause of the insolubility of pure metals in acids are contributed by Dr. Weeren to the current number of the *Berichte*. De la Rive, so long ago as the year 1830, pointed out that chemically pure zinc is almost perfectly insoluble in dilute sulphuric acid. Hitherto, however, the hypotheses put forward attempting to account for this singular fact have been anything but satisfactory. The theory of Dr. Weeren is extremely simple, and is fully supported by the most varied experiments, physical and chemical. It may be stated as follows: "Chemically pure zinc and also many other metals in a state of purity are insoluble or only very slightly soluble in acids, because, at the moment of their introduction into the acid, they become surrounded by an atmosphere of condensed hydrogen, which under normal circumstances effectually protects the metal from further attack on the part of the acid." It is found that when a piece of pure zinc is immersed in dilute sulphuric acid, a slight action does occur during the first few succeeding moments, zinc sulphate and free hydrogen being formed in minute quantity. The free hydrogen, however, instead of escaping, becomes condensed by the molecular action of the zinc upon the surface of the latter, and is retained there with great tenacity as a thin mantle of highly compressed hydrogen gas, capable of affording perfect protection against further inroad of the acid. The experiments from which this simple and very probable explanation has been derived were briefly as follows. The amount of chemically pure zinc dissolved by the acid was first determined. It was, of course, an exceedingly minute quantity. Considering this amount as unity, it was next sought to determine what difference would be effected by performing the experiment *in vacuo*, when of course the escape of the hydrogen would be greatly facilitated. The solubility was found under these circumstances to be increased sevenfold. Next the experiment was performed at the boiling temperature of the dilute acid, first when ebullition was prevented by increasing the pressure, and secondly when ebullition was unhindered, thus again facilitating the removal of the hydrogen film. In the first case, when ebullition was prevented, the solubility was practically the same as in the cold; while in the second case, with uninterrupted ebullition, the solubility was increased twenty-four times. Finally, experiments were made to ascertain the effect of introducing into the acid a small quantity of an oxidizing agent capable of converting the hydrogen film to water. When a little chromic acid was thus introduced the solubility was increased 175 times, and when hydrogen peroxide was employed the solubility was increased three-hundred-fold. The explanation of the ease with which the metal becomes attacked when the ordinary impurities are present is that the hydrogen is not then liberated upon the surface of the zinc, but rather upon the more electro-negative impurities, leaving the pure zinc itself open to the continued attack of the acid. The same of course occurs when a plate of platinum is placed in contact with a plate of pure zinc in the acid. The action of nitric acid, the only common acid which does attack pure metals, is evidently due to the oxidation of the hydrogen film by further quantities of the acid, with formation of water and production of the lower oxides of nitrogen, and even under certain circumstances of ammonia.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. R. Armstrong; two Senegal Touracous (*Corythaix persa*) from West Africa, presented by Sir Brandforth Griffith, Bart.; two Rock Thrushes (*Monticola saxatilis*) from Italy, presented by the Rev. Hubert D. Astley; two Larger Hill-Mynahs (*Gracula intermedia*) from China, deposited; two Mule Deer (*Cariacus macrotis*), three Summer Ducks (*Æx sponsa*), seven Mandarin Ducks (*Æx galericulata*), five Chilian Pintails (*Dafila spinicauda*), two Australian Wild Ducks (*Anas superciliosa*), a Spotted-billed Duck (*Anas pœcilorhyncha*), three Night Herons (*Nycticorax griseus*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE STELLAR CLUSTER χ PERSEI.—Mr. O. A. L. Pihl has completed a micrometric examination, begun in 1870, of the group χ of the great star cluster in Perseus, and the results are published by Grondahl and Son, Christiania. His survey includes all stars down to 10·6 magnitude, and a number of fainter ones down to 11·7 magnitude, the total number of stars measured being 236. The positions obtained, joined to those determined in the *b* group by Prof. Krüger, with the Bonn heliometer, form one continuous survey of both components of the cluster. Prof. Vogel has determined the positions of 178 stars in the central part of the χ group, but Mr. Pihl's investigation covers more than four times the area. A comparison of the right ascensions of the stars measured by these two observers brought to light differences of a systematic character which appeared to be neither the result of observational errors nor of calculation. Upon closely inspecting the measures, Mr. Pihl found that his value for right ascension was less than Prof. Vogel's in the case of those stars which were brighter than the star to which positions were referred; whereas for all stars fainter than this his value was larger, and the fainter the star the greater the difference.

A ring and a bar micrometer were used in the observations, and the reductions were made by the ordinary method of taking half the sum of the moments of ingress and egress in the calculation—a mode of proceeding which depends upon the supposition that the half sum denotes the instant of the passage of the star through the middle of the ring or bar. This supposition, however, is shown to be erroneous. For stars of a less magnitude than 5·5 there is always a detention in the apparent time of emersion, which increases with the faintness of the stars observed. The cause of the error, therefore, is physiological, and due to the occulting micrometers employed. The law regulating it having been found, the necessary corrections have been applied to the measures, thus rendering the work of greater use.

The memoir represents the work of a business man over a period of twenty years, and with an instrument having an aperture of 3¼ inches. It contains much of interest, and will doubtless be appreciated as an important contribution to the knowledge of the stars in a cluster which is certainly one of the grandest of telescopic objects.

ON THE VEGETATION OF TIBET.

In the May number of the *Journal de Botanique* MM. Bureau and Franchet describe a number of new plants from the collections recently brought home by M. Bonvalot and Prince Henry of Orleans, and give a general summary of their character, of which the following is an abstract:—

The collection was made almost entirely in a narrow band of territory reaching from Lhassa eastward near the 30th parallel of north latitude by way of Batang and Sitang to Tatsienlow, in the province of Szechwan, in West China, from which place their route was deflected at a right angle to Yunnan.

Considered in its general aspect, the flora of this region, as shown in the collection, is marked by the stunted form of the shrubs and dwarf character of the herbaceous vegetation. Of the forest trees, Coniferæ and others, no specimens were brought. It is characteristically a vegetation of high peaks, where drought and strong winds are the main climatic features. The Papaveraceæ are represented especially by dwarf, large-flowered kinds of *Meconopsis*. The greater number of the species of *Corydalis* are not more than two or three inches high. The Cruciferæ, such as *Parrya ciliaris*, in the same way are dwarf and large-flowered. *Silene cæspitosa* may be compared with the most dwarf states of *S. acaulis* of our own high mountains. The honeysuckle of Tibet constitutes only a small bush about a foot high, with intertangled branches. But it is especially in the Rhododendrons and Primulas that this dwarf character is remarkable. All the Rhododendrons and Primulas found between Lhassa and Sitang—*R. principis*, *R. primulæflorum*, *R. nigropunctatum*, *Primula leptopoda*, *P. diantha*, and *P. Henrici* may be ranged amongst the dwarfest types of the genera to which they belong. It is the same with *Incarvillea*. The Tibetan species belong to a group found also in Kansu and Central Yunnan, with stem almost obliterated and corolla very large.

Passing eastward in Szechwan the flora puts on a different character. The leaves become larger, the number of flowers to each plant increases. There are many Rosaceæ, Orchids, and species of Pedicularis; amongst the Compositæ the genus Senecio is particularly well represented, and there are several Everlastings that approach the Edelweiss of the Swiss Alps.

The flora of this eastern part of Tibet and western region of Szechwan has a strong affinity both with that of the Sikkim Himalaya and that of Central Yunnan. *Meconopsis Henrici* represents the Himalayan *M. simplicifolia*, Hook. et Thoms.; *Astragalus litargensis*, *A. acaulis*, Benth., *Rubus xanthocarpus*, *R. sikkimensis*; *Brachyactis chinensis*, *B. menthodora*; *Gnaphalium corymbosum* answers to *G. nubigenum*; *Androsace bisulca* to *A. microphylla*; and there are many other similar parallels between the plants of Tibet and Sikkim, and in the same many parallels may be found between the new species found by our travellers in Tibet and those gathered by Delavay in Yunnan.

SOCIETIES AND ACADEMIES.

London.

Royal Society, June 18.—"An Apparatus for testing the Sensitiveness of Safety-lamps." By Frank Clowes, D.Sc. Lond., Professor of Chemistry, University College, Nottingham. Communicated by Prof. Armstrong, F.R.S.

The following apparatus has been devised to render easy the process of testing the sensitiveness of different forms of safety-lamps when used for detecting firedamp. To enable satisfactory tests to be made in the laboratory, it was necessary to insure (1) the easy and rapid production of mixtures of firedamp and air in known proportions; (2) to insure economy of the artificially prepared methane, which represented firedamp; and (3) to examine the flame of the lamp under conditions as satisfactory as those existing in the mine.

A wooden cubical box of about 100 litres capacity was constructed so as to be as nearly gas-tight as possible. It was then made absolutely gas-tight by painting it over with melted paraffin wax, which was afterwards caused to penetrate more perfectly by passing an ordinary hot flat-iron over the surface.

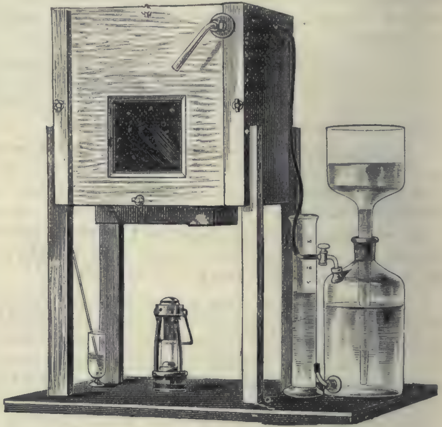

Fig. 1.

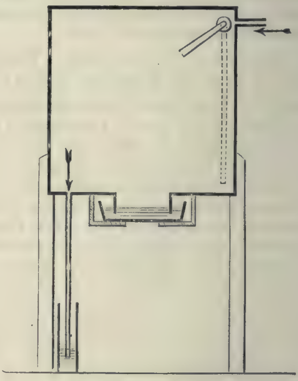

Fig. 2.

This testing chamber was furnished with a small inlet tube at the top, and with a similar outlet tube below. It had a plate-glass window in front for observing the lamp in the interior, and a flanged opening below for introducing the safety-lamp. This opening was closed by a water-seal consisting of a small zinc tray supported by buttons, and containing about 2 inches depth of water, into which the flange dipped. A mixer was arranged, which consisted of a light flat board, nearly equal in dimensions to the section of the chamber, and suspended by an axis from the upper corner of the chamber. The mixer was moved rapidly backwards and forwards from the side to the top of the interior of the chamber, by grasping a handle projecting through the front of the chamber.

When a mixture of air with a certain definite precentage of firedamp was required, the methane, prepared and purified by ordinary chemical methods, was introduced into the chamber in the requisite quantity by the top inlet. It displaced an equal volume of air, which escaped through the lower outlet, the exit end of which was sealed by being immersed just beneath a water surface. A vigorous use of the mixer secured a uniform mixture of gas and air throughout the interior of the chamber in the course of a few seconds. The lamp was then introduced into the chamber, and placed in position behind the glass window. The simplicity of arrangement of the water-seal rendered the necessary opening of the chamber very brief, and the introduction and removal of the lamp many times in succession was not found to produce any appreciable effect upon the composition of the atmosphere inside the chamber. The appearance and dimensions of the "cap" over the flame were noted as soon as the cap underwent no further change. A lamp was left burning in the chamber for a considerable length of time, and its indications underwent no change, owing to the large capacity of the chamber and the very limited amount of air required to support the combustion of the small flame always used in gas-testing. The whole interior of the chamber and mixer were painted dead-black, so as to render visible pale and small caps against a black ground.

The methane was introduced from an ordinary gas-holder. A volume of water, equal to that of the methane to be displaced, was poured into the top of the gas-holder. The gas-tap of the holder was then momentarily opened, so as to produce equilibrium of pressure between the methane and the atmosphere. The gas-tap having then been placed in connection with the upper inlet of the chamber, the water-tap was opened, and the measured volume of water was allowed to flow down and drive the methane into the chamber. As soon as bubbles of air ceased to appear through the water at the outlet, the chamber was closed; the mixer was then vigorously worked for a few seconds, and the mixture of gas and air was ready for the introduction of the lamp. Before introducing the methane for a fresh mixture, the atmosphere of the chamber was replaced by fresh air by removing the water-tray from beneath the opening at the bottom of the chamber, and blowing in a powerful stream of air from a bellows to the top of the chamber.

The chamber was supported on legs, which were arranged so as to place it at a convenient height for observations through the window, and also for the introduction and removal of the safety-lamp.

The observations were usually made in a darkened room, but the flame-caps were easily seen in a lighted room, provided direct light falling on the eye or chamber was avoided.

The capacity of the chamber was 95,220 c.c.; accordingly, the following volumes of methane were introduced: for ½ per cent. mixture 476 c.c., for 1 per cent. 952 c.c., for 2 per cent. 1904 c.c., for 3 per cent. 2856 c.c., for 4 per cent. 3808 c.c., and for 5 per cent. 4760 c.c. It will be seen that a series of tests, in which the above-mentioned percentage mixtures were employed, involves an expenditure of only 15 litres of methane, a quantity far smaller than that required by any other method of testing as yet described.

Of many forms of safety-lamp tested in the above apparatus, the one which most satisfactorily fulfilled the two purposes of efficient illumination and delicacy in gas-testing was Ashworth's improved Hepplewhite-Gray lamp. This lamp is of special construction, burns benzoline from a sponge reservoir, and its flame is surrounded with a glass cylinder, which is ground rough at the hinder part; this latter device prevents the numerous reflected images of the flame, and the generally diffused reflections which are seen from a smooth glass surface, and which render the observation of a small pale flame-cap very difficult, if not impossible.

The wick of this lamp, when at a normal height, furnishes a flame of great illuminating power. When lowered by a fine screw adjustment the flame becomes blue and non-luminous, and does not interfere therefore with the easy observation of a pale cap. The following heights of flame-cap were observed, which fully bear out the unusual sensitiveness of this flame. With 0·5 per cent. of methane 7 mm.; with 1 per cent. 10 mm.; with 2 per cent. 14 mm.; with 3 per cent. 20 mm.; with 4 per cent. 25 mm.; and with 5 per cent. 30 mm. The cap, which with the lower proportions was somewhat ill-defined, became remarkably sharp and definite when 3 per cent. and upwards of methane was present. But even the lowest percentage gave a cap easily seen by an inexperienced observer.

It appears from the above record of tests that the problem of producing a lamp which shall serve both for efficient illuminating and for delicate gas-testing purposes has been solved. The solution is in some measure due to the substitution of benzoline for oil, since the flame of an oil-lamp cannot be altogether deprived of its yellow luminous tip, without serious risk of total extinction; and this faint luminosity is sufficient to prevent pale caps from being seen.

From further experiments made in the above testing-chamber with flames produced by alcohol and by hydrogen, it was found to be true in practice, as might be inferred from theory, that, if the flame was pale and practically non-luminous, the size and definition of the flame-cap was augmented by increasing either the size or the temperature of the flame. It is quite possible by attending to these conditions to obtain a flame which, although it is very sensitive for low percentages of gas, becomes unsuitable for the measurement of any proportion of gas exceeding 3 per cent. This must, for the general purposes of the miner, be looked upon as a defect; but it is not a fault of the lamp already referred to. It is of interest to note that with the Pieler spirit-lamp a flame-cap an inch in height was seen in air containing only 0·5 per cent. of methane.

Physical Society, June 26.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—The following communications were made:—The construction of non-inductive resistances, by Prof. W. E. Ayrton, F.R.S., and Mr. T. Mather. In making some transformer tests about three years ago, the authors had occasion to consider the construction of electric conductors the impedances of which should be practically equal to their resistances. This condition could only be fulfilled by making the inductance small in comparison with the resistance; and, as the former does not depend on the material employed (excepting iron) it was important to use substances of high specific resistance. Carbon or platinoid being available, the latter was chosen on account of its low temperature coefficient. One form of resistance exhibited consisted of strips of thin sheet platinoid about 6 metres long and 4 centimetres wide. Each was bent at the middle and doubled back on itself, thin silk being placed between the contiguous parts and narrow ribbon used to bind the parts together. Twelve such strips arranged in series had a resistance of 2·95 ohms, and would carry a current of 15 amperes without changing its resistance more than $\frac{1}{10}$ per cent. This strip-resistance was made by Messrs. C. G. Lamb and E. W. Smith, who at that time (1888) were students in the Central Institution, and to whom the author's best thanks are due for the praiseworthy manner in which they surmounted the difficulties which presented themselves. Another form of resistance designed for portability consisted of bare-wire spirals, each length having a left-handed spiral placed within a right-handed one of slightly larger diameter, and the two being connected in parallel. This device was found to reduce the inductance to $\frac{1}{10}$ or $\frac{1}{20}$ of that of a single spiral according as the diameters of the spirals approach towards equality. Where the spirals are made of platinoid wire, the ratio of inductance to resistance is very small, averaging about $\frac{1}{500000}$.—On the influence of surface loading on the flexure of beams, by Prof. C. A. Carus-Wilson. Referring to the practical treatment of problems on beam flexure as based on Bernoulli's hypothesis that the bending moment is proportional to the curvature, the author pointed out that this assumes that the cross-sections remain plane after flexure, and neglects the surface loading effect. The present paper describes experiments made to determine the actual state of strain in a beam doubly supported, and carrying a single load at the centre, the effect of surface loading being taken into account. The method of investigation assumes that (1) the true state of strain at the centre of a beam may be found by superposing on the state of strain due to bending only, that due to surface loading without bending; (2) the state of strain due to surface loading only, may be found with close approximation to truth by resting the beam on a flat plane instead of on two supports; (3) the strain due to bending alone, may be obtained from the Bernoulli-Saint-Venant results. Before proceeding to describe the experiments, a short account of the mathematical work previously done on the subject was given. The nearest approach to the particular case here dealt with had been worked out by Prof. Boussinesq, who had shown that for an infinite elastic solid bounded on one side by a plane surface and loaded along a line on that surface, the stress (y) on an element on the normal through the middle point of the line varies inversely as its distance (x) from the surface. The formula thus arrived at was $y = 0·64\frac{P}{x}$, whilst for a finite beam centrally loaded the author's experiments gave $y = 0·726\frac{P}{x}$. The experiments were made on glass beams mounted in a steel straining frame, and placed between the crossed Nicols of a polariscope. Steel rollers 2 mm. in diameter served as supports, and the central load was applied by a screw acting on a roller of similar diameter. Deflections of the beam were measured by a micrometer screw at a point opposite the central load, and traversing screws enabled the whole frame to be moved so as to bring any portion of the beam in the field of view. Circularly polarized light was sometimes used, and a micrometer eye-piece served to measure the distances between interference fringes produced by loading. By carefully chosen experiments the author had shown that if a beam of glass be laid on a flat surface and loaded across its upper surface, the shear at any point on the normal at the point of contact of the load is inversely proportional to the distance from the point of contact. In the first experiment the crossed Nicols were set at 45° to the axis of the loaded bar; a quarter-wave plate was then placed between the bar and the analyzer, and the position of the black spot at the point where the effect of the shear on the polarized light was equal and opposite to that produced by the quarter-wave plate was noted. A second quarter-wave plate was then superposed on the first; the black spot moved upwards to a point where the shear was double that at the first position. This position having been determined, one quarter-wave plate was removed, and the load diminished until the original spot moved up to the second position, and the processes repeated. By this means a series of positions at which the shears were in the proportions 1, 2, 4, 8, &c., were determined. Plotting the results showed the curve connecting the shear and the distance from the point of contact to be hyperbolic. Other experiments showed that the shear at any point was proportional to the load. By maintaining a constant load and measuring the distances between the interference fringes below the point of contact the hyperbolic law was confirmed. The effect of bending a beam is, according to hypothesis, to put the upper portion in longitudinal compression, and the shear (vertical stretch) varies as the distance from the centre of the beam; the shear due to surface loading is a vertical squeeze, and, as shown above, varies hyperbolically. When, therefore, the beam is subjected to both actions, the straight line representing the bending strain may intersect the hyperbola representing the shear due to surface loading in two points, and since, at the corresponding points in the central section, the shears are equal and opposite, the elements are only subjected to voluminal compression, and will exert no bi-refringent action. Hence, when viewed through crossed Nicols, black spots will be seen on a white field. Keeping the load constant and diminishing the span should cause the spots to approach each other, and when the line is tangential to the hyperbola, the spots coincide. These deductions were confirmed by experiment, and it was found that for a span of less than four depths, no point of zero shear exists on the central section. The strains in beams subjected to surface loading were thus shown to be of a character different from those usually assumed, the neutral axis instead of coinciding with the axis of the beam, being lifted up in the centre, and its shape depending on the load and span. Other ingenious and interesting experiments on beams were described, in some of which the lines of principal stress were mapped out. Remarkable results were obtained, showing that although the tension lines given by Rankine and Airy are nearly correct, the curves of compression may be very different, and have very curious shapes. Prof. Perry thought the local loading effect would not be so important in long beams, and inquired whether in ordinary test pieces local loading would affect the breaking strength. He also asked what effect the fact of the load making contact over a surface instead of along a line would have on the results, and in reply Prof. Carus-Wilson said the effect was to raise the asymptote of the hyperbola representing the surface loading stress above the surface of the beam.—On pocket electrometers, by C. V. Boys, F.R.S. This communication described modifications of electrometers adapted for portability. As quartz fibres increase the delicacy and diminish the disturbing influences affecting instruments, much smaller controlling forces can be employed than when silk is used for suspensions. He had, he said, pointed out some time ago the great advantages arising from making galvanometers small. Applying similar reasoning to electrometers, he remarked that making an instrument one-tenth the size of an existing one reduced the moment of inertia of the needle to $\frac{1}{10^5}$, whilst the deflecting couple for given potentials would only be $\frac{1}{10}$ of its former value. The small instrument would for the same periodic time be 10,000 times more sensitive than the large one, provided the disturbing influence could be reduced in the same proportion. This, however, was not ordinarily possible, for any method of making contact with the needle, such as by a fine wire dipping into acid or mercury, prevented very small controlling forces being used. Still, by suitable devices a large proportion of the full advantage could be obtained; a freely suspended needle without liquid contacts was essential to success. The first instrument described was one in which the needle was cylindrical, contiguous quarters being insulated and connected to the opposite ends of a minute dry pile placed within the needle; opposite quarters were thus at the same potential, and at a different potential to the other pair of quarter cylinders. This was suspended within a glass tube silvered on the inside and divided into four parts by fine longitudinal lines. In such an instrument the needle and quadrants are reciprocal, and the deflection depends on the product of the difference of potential between the quadrants and that between the parts of the needle. Owing to the dry pile not being constant, the instrument was found untrustworthy, but when working at its best a Grove cell would give 30 or 40 millimetres deflection. The next step was to make a cross-shaped needle of zinc and platinum, and rely on contact electricity to keep the parts of the needle at different potentials. This bold experiment proved remarkably successful, for the instrument was very sensitive. A disk-shaped needle with quadrants, alternately zinc and platinum, was then employed, and by this a small fraction of a volt could be measured. The weight of the disk was only $\frac{1}{20}$ of a gramme, and the instrument could be turned upside down or carried about in the pocket with impunity. Another small instrument with the stationary quadrants of zinc and copper was exhibited, and by rotating them through an angle of 90°, so as to bring them in a different position relative to the parts of the needle, a deflection of several degrees of arc was produced. In the course of his remarks Mr. Boys made several suggestions relating to ballistic electrometers and electrostatic Siemens dynanometers, and pointed out the possibility of instruments such as he had exhibited being of use in elucidating the obscure points in connection with so-called "contact electricity." The President complimented Mr. Boys on the beautifully simple and remarkably sensitive electrometers exhibited. He remembered that some years ago Mr. Gordon made a very small electrometer, but its insulation was insufficient for electrostatic work. He agreed with Mr. Boys as to the advantages of small instruments, providing sparking across or tilting of the needle could be prevented. On the other hand, he thought the use of small potential differences on the needle was a step in the wrong direction, when great sensibility was required. Prof. Perry asked if the needle could not be kept charged by occasional contacts with a charged acid cup. Mr. Boys said he had originally intended using a fairly highly charged needle, but had not yet done so. He also suggested that an electrometer of very small capacity might be made by reducing the quadrants surrounding a disk-needle, until they became like small tuning-forks.—A paper on electrification due to the contact of gases with liquids, by Mr. J. Enright, and one on the expansion of chlorine by heat, by Dr. Arthur Richardson, were taken as read.

Entomological Society, July 1.—Mr. Frederick DuCane-Godman, F.R.S., President, in the chair.—Mr. Jacoby exhibited a specimen of a species of Coleoptera belonging to the family *Galerucidæ*, with the maxillary palpi extraordinarily developed.—Canon Fowler, on behalf of Mr. Wroughton, Conservator of Forests, Poona, exhibited specimens of a bug imitating an ant, *Polyrachis spiniger*, and of a spider imitating a species of *Mutilla*, and read the following notes:—"I have taken a good many specimens of a bug which has achieved a very fair imitation of *Polyrachis spiniger* (under the same stone with which it may be found), even to the extent of evolving a pedicle and spines in what, were it an ant, would be its metanotum. Curiously enough, however, these spines are apparently not alike in any two specimens. Is it that this bug is still waiting for one of its race to accidentally sport spines more like those of

P. spiniger, and thus to set the ball of evolution rolling afresh? or is it that the present rough copy of *spiniger's* spines is found sufficient to deceive? The bug has also been found in the Nilgherries. Mr. Rothney remarks on the above species:—'I have not found the species mimicking *Mutilla*; but in Calcutta and Barrackpore, where *P. spiniger* is a tree ant, forming its net by spinning together the twigs of a shrub, the mimicking bug also assumes arboreal habits, and may be found on the trunks of trees with the ants.'"—Mr. Porritt exhibited living specimens of *Eupithecia extensaria* and *Geometra smaragdaria*: the position assumed by the former proved conclusively that it had rightly been placed in the genus *Eupithecia*.—Mr. Crowley exhibited two specimens of a *Papilio* from the Khasia Hills, belonging to an undescribed species allied to *P. papone*, subgeneric section *Chilades*. Colonel Swinhoe remarked that he possessed a specimen from Northern Burmah. Mr. Moore and others took part in the discussion which followed.—Mr. Dallas Beeching exhibited a specimen of *Plusia moneta*, recently taken by himself at High Woods, Tonbridge, and specimens of *Gonepteryx cleopatra*, lent him for exhibition, which were alleged to have come from the same locality.—Dr. Algernon Chapman exhibited the larva of *Micropteryx calthella*, and read notes on hem.—Colonel Swinhoe read a paper entitled "On New Species of Heterocera from the Khasia Hills."—Mr. Crowley read a paper entitled "On a New Species of *Prothoe*."—Mr. C. J. Gahan read a paper entitled "On the South American species of *Diabrotica*, Part 2," being a continuation of Dr. Baly's paper on the same genus published in the Society's Transactions for 1890, Part 1.—Mr. W. F. Kirby communicated a paper entitled "Notes on the Orthopterous family *Mecopodidæ*."—Prof. Westwood communicated a paper entitled "Notes on *Siphonophora artocarpi*."

Edinburgh.

Royal Society, June 15.—Mr. T. B. Sprague in the chair.—Dr. Johnson Symington and Dr. H. A. Thomson communicated a paper on a case of defective endochondral ossification in a human fœtus.—Dr. J. Berry Haycraft read a paper on the alkaline and acid salts of the blood and urine, and especially those of phosphoric acid.—Dr. J. M. Macfarlane presented the second part of a paper on the structure, division, and history of vegetable and animal cells, in which he stated that as a result of extended observation he still adhered to the view that a typical cell consists of protoplasm, nucleus, nucleolus, and endonucleolus, the whole usually surrounded by a cell wall; that the nucleolus is the important part equally in division and in sexual union of cells; that after division had ceased, successive fragmentation of endonucleolus, nucleolus, and nucleus occurred, though to a varying degree in different cells; that thus a multiendonucleolar was followed by a multinucleolar, and this by a multinuclear state. He regarded the nucleolus of every cell as the sexual centre directly derived from union of the chromatic substance of the male and female pronuclei of the ovum, and that from the nucleolus extremely fine radiating threads of chromatic substance passed out along the achromatic fibrils, which last he viewed as a finely differentiated reticulum of the ground protoplasm. By union of the radiating chromatic threads, the author considered that the nuclear membrane was formed, while continuations radiated outwards from this through the cell-protoplasm to convey stimuli to and from the sex-centre or nucleolus. He further stated that many facts and direct observations made tended to show that the radiating threads from the nucleus, and ultimately therefore from the nucleolus, of one cell are connected with corresponding ones from other cells, and this, if fully verified, would cause us to regard an organism as a sexual whole, and the male and female reproductive cells as being specially set aside to hand down hereditary and acquired conditions. He showed that this had a special bearing on the next communication submitted—a comparison of the minute structure of plant hybrids with that of their parents, and its bearing on biological problems. At a previous meeting of the Society (May 4) he directly demonstrated, by three parallel lantern exhibitions of micro-photographs, that the tissues of root, stem, leaf, and flower parts in the hybrid named by Dr. Masters *Philageria Veitchii*, are exactly intermediate, when of corresponding age, between those of the parents; and further, that when a structure is developed in one parent, but is absent in the other—*e.g.* the sepal honey gland of *Lapageria*—the hybrid shows it of half the size. He now referred to eleven other hybrids whose tissues he had worked over in detail, and selected points from about sixty others, examined more or less minutely. By triplets of micro-photographs the author not only demonstrated that a hybrid is, to its minutest details, a blended reproduction of both parents, but that where the parents show diverse morphological details, these may be handed down to the hybrid of half the size, or one only may be inherited. He advanced a theory to explain this, and then compared the tissues of *Cytisus Adami* (see also *Gard. Chron.*, July 1890, p. 94), which he regarded as a true graft hybrid. He concluded by referring to the colour, flowering period, and constitutional vigour of plant hybrids, and to the light shed by these inquiries on the effects of environment, on the influence of sex, and on heredity.—Prof. Tait communicated paper, by Prof. Stokes, on an optical proof of the existence of suspended matter in flames. The method consists in condensing sunlight on the flame. The light is scattered by the solid particles in an extremely thin layer both where the beam enters the flame and where it leaves it. It is polarized in the plane of reflection. The effect is not found in some flames—such as a Bunsen flame tinged with burning sodium. In the latter case this seems to be due to the fact that the sodium is in the form of vapour—not of solid particles.

Sydney.

Royal Society of New South Wales, May 6.—Annua Meeting.—Dr. A. Leibius, President, in the chair.—The Report stated that 25 new members had been elected during the year, and the total number on the roll on April 30 was 457. During the year the Society held eight meetings, at which the following papers were read:—Presidential address, by Prof. Liversidge, F.R.S.—On a compressed-air flying machine, by L. Hargrave.—On the treatment of slips on the Illawarra Railway at Stanwell Park, by W. Shellshear.—On native names of some of the runs, &c., in the Lachlan district, by F. B. W. Woolrych.—Remarks on a new plant rich in tannin, by Charles Moore.—Record of hitherto undescribed plants from Arnheim's Land, by Baron Ferd. von Mueller, F.R.S.—The theory of the repetition of angular measures with theodolites, by G. H. Knibbs.—On some photographs of the Milky Way recently taken at Sydney Observatory, by H. C. Russell, F.R.S.—Australian aborigines: varieties of food and methods of obtaining it, by W. T. Wyndham.—On the application of the results of testing Australian timbers to the design and construction of timber structures, by Prof. Warren.—Geological notes on the Barrier Ranges silver-field, by C. W. Marsh.—Some folk-songs and myths from Samoa, by the Rev. T. Powell and Rev. G. Pratt, with an introduction and notes by Dr. John Fraser.—The coal-fields of New South Wales and their associated eruptive rocks, by T. W. E. David.—Some remarks on the Australian languages, by Dr. John Fraser.—On the 74-ounce compressed-air flying machine, by L. Hargrave.—The Medical Section held seven meetings, at which nine papers were read; the Microscopical Section held seven meetings, at which interesting exhibits were shown.—The Clarke Medal for the year 1891 had been awarded to Prof. F. W. Hutton, Canterbury College, Christ Church, New Zealand.—The Council had issued the following list of subjects with the offer of the Society's bronze medal and a prize of £25 for each of the best researches if of sufficient merit:—(To be sent in not later than May 1, 1892) On the iron-ore deposits of New South Wales; on the effect which settlement in Australia has produced upon indigenous vegetation, especially the depasturing of sheep and cattle; on the coals and coal measures of Australasia. (To be sent in not later than May 1, 1893) Upon the weapons, utensils, and manufactures of the aborigines of Australia and Tasmania; on the effect of the Australian climate upon the physical development of the Australian-born population; on the injuries occasioned by insect pests upon introduced trees.—A most successful *conversazione* had been held in the Great Hall of the University on December 10, at which 800 guests were present.—The Chairman read the Presidential Address, and the officers and Council were elected for the ensuing year, Mr. H. C. Russell, F.R.S., Government Astronomer, being President.

Paris.

Academy of Sciences, July 6.—M. Duchartre in the chair.—On the lunar inequality of long period due to the action of Venus, and depending upon the argument $l + 16l' - 8l''$, by M. F. Tisserand. According to Delaunay, in calculations of this inequality it is possible to neglect powers of the inclination of the orbit of Venus higher than the second. M. Tisserand shows, however, that terms which contain the fourth power of the inclination may have a sensible influence, and diminish the coefficient of the inequality in question by a tenth of its value—that is, by about 1″·6.—On the manner in which the velocities are distributed from the entrance of a cylindrical tube of circular section widened at the mouth up to the points where uniformity is established, by M. J. Boussinesq.—The flight of insects studied by photochronography, by M. Marey. The author describes an apparatus which he has used to obtain photographs of flying insects. It allows exposures to be made so short as $\frac{1}{25000}$ of a second. His observations indicate that the wings of insects in flight, by meeting obliquely the resistance of the air in to-and-fro movements, act in a very similar manner to the sculls used to propel rowing-boats.—Study of the tetra-iodide of carbon, by M. Henri Moissan. By acting on carbon tetrachloride with boron tri-iodide, the trichloride of boron and the tetra-iodide of carbon are obtained by double decomposition. A detailed account is given of this reaction. The carbon tetraiodide thus prepared forms comparatively large crystals of a beautiful red colour, very similar to the rubies synthetically prepared by MM. Fremy and Verneuil. Several new reactions with this compound are described.—Compounds of camphors with the aldehydes: on a new mode of formation of alkyl camphors, by M. A. Haller.—The Eocene formations of Algeria, by MM. Pomel and Ficheur. It has been previously shown that the Eocene formations of Algeria may be divided into the three groups, lower, middle, and upper. The observations now stated indicate that the Middle Eocene formations only extend over a narrow zone, and that they are characterized by Nummulites of the groups *Numm. lævigata* and *Numm. perforata*. The Lower Eocenes are defined from a nummulitic point of view by *Numm. planulata*, *Numm. biarritzensis*, and *Numm. gizehensis*.—Method of ready transformation of the tubercular products of joints and certain other parts of the human body, by M. Lannelongue.—On the determination of the constants and coefficients of elasticity of nickel-steel, by M. E. Mercadier. Experiments have been made to determine the relation $\frac{\lambda}{\mu}$ for solid sonorous bodies, and, therefore, the coefficient of dynamical elasticity, by a method founded on Kirchhoff's theory of vibration of circular disks. From the results obtained it appears that the incorporation of a sufficient quantity of nickel with steel tends to make the alloy isotropic. The mean coefficient of dynamical elasticity for alloys containing about 5 per cent. and 25 per cent. of nickel is 18,600, whereas that of pure steel is 20,700.—Calculation of molecular volume, by M. G. Hinrichs.—On an explosive compound which results from the action of baryta water on chromic acid in the presence of oxygenated water, by M. E. Péchard. By adding baryta water in the presence of an excess of oxygenated water, a precipitate is produced, which, after desiccation, explodes violently by heat or percussion. The compound has the formula $BaO_2 . CrO_3$.—On the detection of small quantities of boric acid, by M. F. Parmentier.—On the structure of the ocellates of *Lithobius forficatus*, by M. Victor Willem.—Comparative study of the development and morphology of the parapodia of Syllidiæ, by M. A. Malaquin.

GÖTTINGEN.

Royal Society of Sciences.—The Proceedings of the Society for February, March, and May 1891 contain the following papers of scientific interest:—

No. 1.—W. Nernst: on Henry's law of chemical equilibrium in solutions.—F. Meyer: on discriminants and resultants of singularity-equations.—O. Venske: contribution to the integration of the equation $\Delta^2 u = 0$ for certain plane figures (the disk, the annulus, the rectilineal angle, the rectilinear strip with parallel sides, the annular sector).

No. 2.—W. Voigt: contributions to hydrodynamics (pulsating sphere or cylinder in an infinite liquid; stationary waves in a stream as an example of Kirchhoff's theory of liquid stream-rays; successive approximation to the irrotational motion of a heavy liquid with free surface; stationary combined motions depending on two co-ordinates in a liquid under a conservative system of forces; non-stationary current-motion, partly rotational, partly irrotational, within an ellipsoidal shell at rest).—O. Venske: integration of a special system of linear homogeneous differential equations, with doubly periodic functions as coefficients.—F. Meyer: on real properties of curves in space.

No. 3.—G. Tammann: on conduction through membrane-like precipitates.—O. Venske: a new apparatus for the determination in absolute measure of the internal thermal conductivity of badly conducting bodies.

STOCKHOLM.

Royal Academy of Sciences, June 10.—On the treatment of cancer through injections, by Prof. Rossander.—Analysis of a pyrite, which seems to contain a new element, by Herr L. J. Igelström.—A letter from Baron Ferd. von Mueller on the Australian contributions towards a South Polar expedition planned in Sweden, communicated by Baron Nordenskiöld.—The intensity of the radiation of gaseous bodies under the influence of an electric discharge, by Dr. K. Ångström.—On derivates of sulphur urates, iii., by Dr. Hector.—A solution of a mechanical problem which leads to the functions of Rosenhain, by Dr. Olsson.—Some experiments on the respiration of the Algæ, by Miss H. Lovén.—The African genera of the Calandrides related to the Oxypisthens, by Prof. Chr. Aurivillius.—A comparison between the methods of Ångström and Neumann for determining the conductibility of heat in bodies, Part iii., by Dr. Hagström.—On 1–6 dibrom-naphthaline, by Herr Forsling.—Triazol combinations produced from aldehydes and dicyan-phenyl-hydrazine, by Herr Holmqvist.—On the ammoniacal combinations of iridium, by Dr. Palmær.—On the formulas for calculating the mortality during the first year of human life, as derived from the statistics of the population, by Dr. G. Eneström.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Among the Butterflies: B. G. Johns (Isbister).—The Business of Travel, W. F. Rae (Cook).—The Melanesians: Dr. R. H. Codrington (Clarendon Press).—Verlags-Catalog von R. Friedländer und Sohn, 1830-90 (Berlin, Friedländer).—The Geology of Nova Scotia, &c., or Acadian Geology, 4th edition: Sir J. W. Dawson (Macmillan and Co.).—British Cage Birds, Part 15: R. L. Wallace (U. Gill).—North Midland School Cookery Book (Raithby).—Der Peloponnes Versuch einer Landeskunde auf Geologischer Grundlage, Abtg. 1: Dr. A. Philoppson (Berlin, Friedländer).—Darkness and Light in the Land of Egypt: Colonel Fraser (Sutton).—Die organischen Elemente und ihre Stellung im System: W. Preyer (Wiesbaden, Bergmann).—Destructive Locusts: C. V. Riley (Washington).—U.S. Department of Agriculture—Reports of Observations and Experiments (Washington).—Insect Life, vol. iii., Nos. 9 and 10 (Washington).—Stonyhurst College Observatory—Results of Meteorological and Magnetical Observations, 1889-90: Rev. W. Sidgreaves (Market Weighton).—Simple Recipes for Sick-Room Cookery: Mrs. Buck (Raithby).—Journal of the Royal Agricultural Society, vol. ii. Part ii. No. 6 (Murray).—Journal of the College of Science, Imperial University of Japan, vol. iv. Part 1 (Tokio).—Mind, July (Williams and Norgate).—The Pedagogical Seminary, vol. i. No. 2 (Worcester, Mass.).—The Economic Journal, No. 2 (Macmillan and Co.).—London and Middlesex Note-Book, No. 2 (E. Stock).

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THURSDAY, JULY 23, 1891.

THE TEACHING OF FORESTRY.[1]

A Manual of Forestry. By William Schlich, C.I.E., Ph.D. Vol. II. (London: Bradbury, Agnew, and Co., 1889.)

IN a loop of the Main river in Lower Franconia, east of Aschaffenburg, rises an extensive mountainous country, clothed with almost unbroken forest of singular beauty and of enormous value. It is the Spessart, in old times known as the home and haunt of great highway robbers, but also known from time immemorial as the home of the best oak timber in Germany. The red sandstone of the Trias, which everywhere is the underlying rock in this extensive forest country, makes a light sandy loam, which, where deep, is capable of producing tall, cylindrical, well-shaped stems. Having grown up, while young, in a densely crowded wood, the oak here has cleared itself of side branches at an early age. Hence these clean straight stems, which in the case of spruce, silver fir, and other forest trees, may justly be said to be the rule, but which the oak does not produce, save under these and similarly favourable circumstances. The species here is *Quercus sessiliflora*: this species does not form pure forests, but is always found mixed with other trees, the hornbeam, the beech, and on the lower slopes of the western Schwarzwald, the silver fir. In the Spessart, the beech is associated with the oak, in the same manner as the bamboo is the chief associate of the teak tree in Burma.

In publishing his manual of forestry, the author wished in the first instance to place in the hands of the students at the Coopers Hill Forest School a handbook to facilitate their studies. That Forest School was, it may be remembered, established in 1885, in connection with the Royal Indian Engineering College at the same place, in order to give the needful professional training to young Englishmen who desired to enter the Indian Forest Department. Accordingly, when the first volume of that manual appeared in 1889, it was natural that some, who took a strong interest in the progress of forest management in the British Indian Empire, were surprised that the book did not deal with Indian trees, and that its teaching related to the oak, the beech, the Scotch pine, and other trees of Europe. By some of these zealous friends of Indian forestry the book was pronounced a failure, because it did not treat of Indian forest trees.

The principles which guide the forester in the proper treatment of his woods are the same all over the world, in India as well as in Europe. But while the application of these principles to the treatment of Indian forests is not more than thirty-five years old, the methodical and systematic treatment of European forests is of old standing, and has stood the test of experience. In the teak forests of Burma, the bamboo has a position similar to that of the beech in the oak forests of the Spessart. Oak and teak are both trees with comparatively light foliage. Pure woods of these species, while young, are sufficiently dense to shade the ground, whereas at an advanced age the wood gets thin, the canopy light, and the result is that grass and weeds appear, and that by the action of sun and wind the soil hardens and is less fertile than the loose porous soil, which is shaded by dense masses of foliage. Hence the advantage of associates, which, like the beech in Europe and the bamboo in Burma, shade the ground with their dense foliage and enrich it by the abundant fall of their leaves. But it is not only the condition of the ground which is improved by these useful associates. Teak and oak have this specialty also in common, that, when growing up alone, their stems, instead of running up into clean cylindrical boles, are apt to throw out side branches, which greatly impair the market value of the log. But when growing up in dense masses with their natural associates, these latter, crowding in as they do on all sides, around the oak in the Spessart and the teak in Burma, prevent the development of side branches and thus produce clean and regularly shaped stems.

In these and many other ways are the associates of the teak and of the oak useful friends, so to speak. Under certain circumstances, however, and at certain periods of their life, they are dangerous enemies to their more valuable companions. On the sandstone of the Spessart and elsewhere, the beech, as a rule, has a more vigorous growth than the oak; it gets the upper hand, and, unless it is cut back or thinned out in time, the oak, if both are growing up in an even mixture, has no chance. The bamboo is even more formidable as an enemy of the young teak tree. Though the teak may have had a long start; if a crop of bamboos—either the shoots of old rhizomes, or perhaps the result of general seeding of the old bamboo forest, cleared away to make room for the teak—springs up among it, the teak is doomed. As soon as the rhizomes of the bamboo have acquired sufficient strength, they produce, within a few weeks, during the rains, such a profusion of full-sized shoots, say 20 to 30 feet high, that the young teak trees among them are throttled and extinguished.

The similarity in the relations of teak and bamboo in Burma, and of oak and beech in the Spessart, has led foresters in both countries to devise similar arrangements for the regeneration of these forests. In the Spessart, when the old timber in a compartment of the forest is cut, the best places for the growth of the oak are selected, and the oak, which here sells at the rate of from 2*s.* to 3*s.* a cubic foot for sound and well-shaped pieces, is sown on soil most suitable for its development; while the beech, the timber of which only fetches about one-fifth of that amount, is allowed to reproduce naturally from self-sown seedlings over the rest of the area. Among the oak also a certain but small proportion of beech springs up, and even where pure oak woods may be the result of these proceedings, it will not be difficult, when they are sufficiently advanced, to introduce such a proportion of beech as will secure their satisfactory development. In the same way in Burma, selected areas are cleared for the growth of teak in the original forest, the clearance being effected and the teak planted with the aid of that rude mode of shifting cultivation, known as the Toungya system.

Many other instances might be quoted, in which similar practices have developed in the rearing and tending of woods in Europe and in India. The principles of sylviculture are the same everywhere, and the application of these principles to the treatment of woods in different parts of the globe has, in many instances, led to the adoption of similar methods; hence Dr. Schlich was right in selecting the timber trees of Europe to illustrate the application of these principles in the manual before us.

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[1] See NATURE, vol. xli. p. 121.

Sylviculture, the author explains, is the formation and tending of woods, and he divides his subject into four chapters. The first of these chapters treats of preliminary works, such as choice of species, fencing and reclamation of the soil by draining, the fixation of shifting sands, the breaking through of an impermeable substratum (pan) and the like. In regard to the fixation of shifting sands, an interesting account is given on p. 33 of the methods which have been most successfully practised on the west coast of France, in order to stop the progress inland of the coast dunes, and to clothe these ridges of rolling sand with a productive forest of the cluster pine (*Pinus Pinaster*). A belt, in many places five miles wide, along the coast of Gascony, and considerably further north, has in this manner been reclaimed, and the steady progress of the sand, which had covered large areas of fields and meadows, and which had destroyed numerous villages, has thus been arrested.

Chapter ii. deals with the formation of woods by artificial and natural means. The Spessart, which has been mentioned above, is an instance in which both artificial and natural means are used in order to effect the regeneration of the forest, so as to insure the production of timber of the highest possible commercial value. In most large forest districts on the continent of Europe, both the natural and artificial method are employed. As the author says on p. 178, neither the one nor the other system "is the best at all times and under any circumstances; only a consideration of the local conditions can lead to a sound decision as to which is preferable in a given case." In France, for instance, a country highly favoured by a climate uniformly moist and mild, where most forest trees produce seed more frequently than in Germany, natural reproduction may, broadly speaking, be said to be the rule and planting the exception. But in France, also, planting operations on a large scale have been carried out on the dunes of the west coast as well as on bare mountain-sides of the Alps, the Cevennes, and the Pyrenees, and, wherever necessary, planting is resorted to, to supplement the natural regeneration of the forests.

An instance in which over a large extent of country the forests are regenerated artificially may be found in the State forests of the kingdom of Saxony, together with most of the communal and many of the private forests in that country. The State forests of Saxony cover an area of 432,000 acres, and by far the larger portion of this area is stocked with pure spruce forest treated on a short rotation of eighty years, and regenerated artificially by planting. The high prices realized in this industrious and thickly populated country, even for timber of small sizes, have gradually led to the adoption of this system; and the State forests of the kingdom of Saxony are a pattern of methodical and most successful management. The forest ranges, all in charge of highly trained superior forest officers, are small, containing not more than 2000 to 3000 acres each, and many of these ranges have a steady regular annual yield of 140 cubic feet of timber per acre, and furnish a net revenue, after deducting all charges, general and local, of 100 marks per hectare, which corresponds to forty shillings an acre.

But in Great Britain also, and in Scotland especially, is the system of rearing forests by planting well understood, and it is practised over large areas economically and successfully. French as well as German foresters of great practical experience have repeatedly expressed their high sense of the skill and ability with which large plantations are formed in Great Britain at a comparatively moderate cost. But even foresters and wood-managers in Great Britain may learn a good deal from this portion of Dr. Schlich's book. Their attention might specially be directed to the author's remarks on p. 113, regarding transplants which have developed a lopsided root system, "because the trenches, into which the pricked out seedlings are placed, are often made so shallow, that the root system of the plants, instead of assuming a natural position in the ground, is altogether bent to one side."

That section of the second chapter which deals with the natural regeneration of woods, necessarily divides itself into two portions: first, natural regeneration by seed; and second, by shoots and suckers (pollards and coppice). Concise brevity is one of the great merits of Dr. Schlich's manual, and it doubtless was necessary to curtail, and to make a rigid selection of the most important matters. But the treatment of coppice woods and of coppice under standards might perhaps have been a little less brief.

As regards natural regeneration by seed, the Black Forest in South-Western Germany may be quoted as an instance where, over extensive areas, the forest is chiefly regenerated by natural means. The splendid logs of spruce and silver fir, which are floated down the Rhine in numberless huge rafts, have all grown from self-sown seedlings, and most of the young timber now growing up has had the same origin. The timber which is brought to market from these forests is much older and heavier than that sold in the forests of Saxony, but the results of management are to some extent similar. There are some forest ranges in the Schwarzwald, both in the grand-duchy of Baden and in the kingdom of Württemberg, which yield the same annual quantity of timber per acre, and furnish the same rate of net revenue to their proprietors, as those of Saxony. The term of rotation, of course, is much longer, and the system of natural reproduction takes time, hence the money value of the growing stock of old timber is very large, much larger per acre than in Saxony. The interest, therefore, on the capital invested (value of land plus growing crop) is less in this case. The discussion of these matters, however, does not appertain to sylviculture, but to forest management, with which the author will deal in a subsequent volume of his work.

As already mentioned, in France the natural regeneration of forests is the rule, chiefly owing to its wonderfully favourable climate. Large areas, mainly of private and communal forest, are managed in admirable style, as coppice woods and as coppice under standards. The treatment of high timber forests also, and their regeneration from self-sown seedlings, by means of a regular system of successive cuttings, has in France been brought to a high state of perfection. This circumstance renders the French forests specially valuable as a field of instruction for foresters proceeding to India. For in that large country, though planting has been commenced and must necessarily be carried on in some instances on a large scale, yet every effort ought to be made to develop good systems of natural regeneration in the different provinces.

On pp. 132–64 the author gives a clear account of the different systems which have in course of time been devised, in order to effect the natural regeneration of woods by seed. Under the more favourable climate of France the desired object is generally effected by a simple and to some extent uniform system of successive cuttings. In Germany, on the other hand, where droughts are frequent, frosts severe, and where good seed years generally are of rare occurrence, the system of regular successive cuttings, which originated in Germany, in many cases was found to fail, and accordingly, some sixty or seventy years ago, the tide set in in favour of artificial reproduction. A reaction, however, has for good reasons taken place in many parts of the country, and during the last thirty years German foresters have been busy in adapting the system of natural regeneration to the peculiar conditions of each forest district. Indian forest students should go to France, in order to become impressed with the fact that under favourable circumstances natural regeneration of high forests may be effected by a simple and easy system of treatment. In German forests, on the other hand, they should learn how the difficulties of a climate frequently unfavourable have been successfully overcome by devising systems of treatment suitable to the requirements of each locality, and the knowledge here acquired will be most useful, nay, necessary, to them in India, where the conditions of climate by no means always favour the natural regeneration of the more valuable forest trees.

Space forbids a full discussion of this most important and interesting subject. This portion of Dr. Schlich's book, if supplemented by the study of forests on a large scale, particularly in Germany, will be most useful to foresters who may be called upon to devise methods of forest treatment in other parts of the globe, be it India, Australia, South Africa, or North America.

Closely allied to the subject just adverted to is what the author says in the fourth section of the same chapter regarding the formation of mixed woods. Pure woods, consisting of one species only, are exposed to various risks, from which mixed woods are exempt. Hence, in most Continental forests, there has of late years been a strong tendency in the direction of favouring the growth of mixed woods, such as oak and beech, oak and hornbeam, oak and silver fir, Scotch pine and beech, and the like. It goes without saying, that operations in this direction, in order to be successful, must be guided by careful study of the mode of growth and of the peculiar requirements of the different species in different places and under different conditions. Something has been said above regarding the treatment of mixed woods of oak and beech in the Spessart. But it does not follow that oak and beech behave in the same manner everywhere. On certain kinds of shale, belonging to the Devonian formation, for instance, the oak rather than the beech has the tendency to take the lead, and here mixed woods of oak and beech can be produced from self-sown seedlings much more easily than would be possible on the sandstone of the Spessart. Again, along the foot of the Western Schwarzwald, where, as already stated, the silver fir is associated with the oak, this tree, though a shade-bearer like the beech, renders it much easier for the oak to hold its own in an even-aged mixed wood, because in its early youth it grows very slowly, thus giving the oak a good start in life.

Chapter iii. teaches how woods should be tended during early youth and afterwards. Passing over what the author says regarding cleaning of young woods and pruning, we come to thinning operations. On p. 209 an interesting statement is given showing the number of trees per acre in certain mixed woods of the Schwarzwald. The figures are as follows:—

Age of wood in years.	Number of trees per acre.
20	3960
40	1013
60	449
80	346
100	262

Thus, during the life of a wood, and this holds good in all cases, the number of trees per acre decreases gradually from several thousand to a comparatively small number at maturity. When, as usual, the object is to produce high class timber, with clean well-shapen stems, the rule is, as the author correctly states it: "The wood should be thinned lightly until towards the end of the principal height growth; then the thinnings should gradually become heavier, so as to assist a selected number of trees by the gradual removal of all those which are inferior and diseased." In its youth the wood is crowded, the young trees maintaining a severe struggle for existence. The weaker trees are suppressed and some are actually killed, while the rest are either dominant trees, with their head well above the others, or dominated, though not suppressed. Formerly thinnings were generally done by rule of thumb, the dead, oppressed, and a portion of the dominated trees being removed. But it is obvious that, when the object is to produce valuable timber, thinnings must so be managed, that the trees which are destined to attain the term of rotation, and which will form the final crop to be cut down, in the example here given, 262 trees per acre 100 years old, shall be sound and regularly shaped. It is obvious that to attain this object dominant trees also may occasionally have to be removed, if unsound, spreading, or irregular shaped, and this is properly recognized by the author. He justly adds that in such cases dominated and even suppressed trees may have to be spared in order to keep the ground well under cover. Such would be the practice in the case of woods consisting of one species only, or of several species of equal value. Where one species, such as oak or teak, is of much greater value than the others, all thinnings must, as a matter of course, be so arranged as to favour this species at the expense of the rest.

So far concerning the thinning of crowded woods. The last section of the same chapter deals with the tending of open woods for the production of large timber. Into this subject, which is one of some difficulty, though of great importance, it would lead too far to enter on the present occasion.

Chapter iv. contains sylvicultural notes on British forest trees, with notes (by Prof. H. Marshall Ward) on botanical characters serving to distinguish the principal British forest trees. The two species of oak dealt with in the sylvicultural notes are *Quercus pedunculata* and *Quercus sessiliflora*. Botanists are well aware that the maintenance of distinctive characters between these two and others of the European species of *Quercus* is difficult; so much so, that the best authorities on English trees have decided to re-establish the old species of Linnæus, *Quercus Robur*, and to regard the two species named merely as forms or varieties. The forester has a different task, and for him the mode of growth and the requirements of these two oaks are so different that he must keep them separate. It will suffice to mention one point, which has not perhaps been brought out sufficiently by the author. The mixed woods in which *Quercus sessiliflora* is associated with the beech, the hornbeam, and the silver fir have been mentioned above. In natural high forests this species is only found in company with other trees, and particularly with the three kinds named. The pure or nearly pure coppice woods of *Quercus sessiliflora* in France and Western Germany are an exception; these, however, have been converted into pure woods by the long-continued cutting out of beech, hornbeam, and soft woods. *Quercus pedunculata*, on the other hand, *does* form pure high timber forests of considerable extent. Such are found both in Northern and Southern Europe, not on hilly ground, but always on deep alluvial soil. Instances are the forests on low ground along the Elbe and other rivers of North Germany, the magnificent pure forests of that tree on the banks of the Adour river near Dax in Gascony, and similar ones in the peninsula of Istria, south of Trieste. There is underwood on the ground in the forests named, but it merely consists of thorns and low shrubs. The two species, *Quercus sessiliflora* and *pedunculata* have different requirements and require somewhat different treatment. This, however, is a small matter. These sylvicultural notes are most valuable, and it is satisfactory that the Weymouth pine and the Douglas fir have been included among them.

The second volume of Dr. Schlich's manual, like the first, will be an immense help to the students who are trained at the Coopers Hill College for forest service in India. It will be a great boon to all who are charged with the management of forests in India, in the colonies, and in the United States of North America. And it may perhaps be hoped that in Great Britain also this excellent book will in course of time tend to awaken a more general interest in the good management of its woodlands, which are very extensive, and which some day may be of considerable importance and of great value to their proprietors. D. Brandis.

THE APPLICATIONS OF MODERN CHEMISTRY.

Dictionary of Applied Chemistry. Vol. II. (Eau–Nux). (London: Longmans, 1891.)

THE editor of a dictionary of applied science, such as the volume before me, has in these days no enviable task to perform: much is required of him, and the difficulties with which he has to contend are great. Prof. Thorpe has acquitted himself well, for though there may be, indeed there are, many points with which the expert can find fault, yet these are generally matters of detail, and on the whole the work has been satisfactorily done, so that the second volume will be found to be quite up to the high level of the first. The industries which owe their foundation to the science of chemistry now progress with such giant strides, that processes which last year were the newest and best may this year be so improved as to be rendered obsolete, so that an article printed at the commencement of a volume may become antiquated before the last article is in type, whilst data unattainable when the article was written are superseded by some more recently published. As an example of this, I may take that upon "gas coal," written by a most competent authority, Mr. Lewis Wright. On p. 177 will be found a table giving the weight of coal carbonized by all the authorized gas undertakings in the kingdom, exclusive of those of local authorities, for the year ending March 25, 1886. Since these tables were printed, a Board of Trade return for 1890 has been published. In 1886, 8,378,904 tons of coal were carbonized; in 1890 the figure rose to 9,663,011. In 1886 the mileage of mains was 18,967; in 1890 it had increased to 21,584. These numbers point out the enormous extent of the coal-gas industry in this country, and show clearly that it is not suffering from the competition of electric lighting; indeed, this competition is favourable to the sale of gas, for we see that our streets are now better lighted than formerly, and the consumption of gas in many shops is increased, in order to vie with the splendour of their neighbours' electric light.

As a critic is bound to criticize, I may point out some few faults of commission and omission which have struck me in reading through this generally excellent article.

The important steps which have recently been taken in many large works for charging and drawing the gas retorts by mechanical means are barely referred to. Great economy is doubtless effected where such labour-saving mechanical appliances have been adopted, and a description of these would have been of interest, as the labour agitation in our gas-works has brought engineers face to face with this question. Another point upon which a statement would have been of value is the most improved arrangements of the purifying house, and the methods adopted for charging and discharging the purifiers. That "the whole of the sulphuretted hydrogen, carbonic acid, and carbon disulphide can be easily and economically removed" (p. 200) by a combined system of oxide and lime, and with a proper arrangement of purifiers, is a statement to which many gas engineers will demur. The London companies, especially, who have a legal standard limit for sulphur compounds, find it both difficult and expensive to keep down the impurities to the necessary point. The illustrations given in this article are scarcely worthy of the letter-press. Figs. 22 and 23 do not give an idea of the construction of a modern gas-holder, some of which now have the enormous capacity of ten million cubic feet, and are marvels of engineering skill. A description of the latest improvements would have added interest to the article.

As an instance of the rapid progress of an industry interfering with an adequate account being published in the early pages of such a volume, I may refer to the article on electro-plating, by Prof. W. C. Williams, which, although giving a clear account of the older processes, scarcely represents the position of to-day. Thus no reference is found to recent methods of the electro-deposition of metals, as, for example, the Elmore copper process, or to that of plating by aluminium; nor does any mention occur of the electric power suitable or used for depositing metals.

To justify the opinion that this volume is no unworthy successor to that published last year, I would refer to a few articles which are certainly the best I know on their several subjects. First, "Explosives," by W. H. Deering, coming from the pen of one who has had long experience in the Chemical Department of the Royal Arsenal, Woolwich, is, as we should expect, up to the level of the time, and in every respect excellent. Second comes Prof. Percy Frankland's article on fermentation. No one is more competent than he to write on this most fascinating subject, and his article reads like a novel, and even better, for "truth is stranger than fiction"; and Percy Frankland tells his story so clearly and well that I will not spoil the pleasure of his readers—and they ought to be many—by any attempt to abstract its results. Thirdly, the article on "Matches," by Mr. Clayton, may be cited as an admirable treatise on this important branch of chemical manufacture, condensed into 24 pages. Not the least important contribution are the nine tables giving, in chronological order, lists of the numerous patented and other inventions in this department of chemical technology. Lastly, I will select Mr. Wynne's exhaustive article on naphthalene as perhaps the most able and valuable in the whole volume. When we learn that, although it occupies 65 pages of the dictionary, it treats exclusively of the derivatives of one hydrocarbon, and only of those of them which are now used in the arts, and valuable for industrial purposes, we begin to form an idea of the extent and importance of the results of modern organic research, which has opened out regions illimitable, leading to practical results such as the chemists of the last generation would have deemed impossible.

In a dictionary of applied science the question of selection is even more difficult than in a similar work of pure science. Here the knowledge and tact of the editor are especially called into play. Prof. Thorpe has, I think, chosen well, but here and there some pages are taken up with matters of which I should be glad to learn the present industrial value—for in the future all may have a use. Thus I find close together the following: elaidic acid, ericolin, erucic acid, erythrol—all, doubtless, compounds of scientific interest, but hardly, I would venture to suggest, of industrial importance.

As I said of the first volume, so I may say of the second—that it does credit to the authors of the articles, to the editor, and to the public-spirited publishers. It is good that English scientific literature keeps up its prestige for thoroughness, clearness, and conciseness, and that in this volume of the dictionary no falling off from this standard is visible.

H. E. Roscoe.

THE FISHES OF SWITZERLAND.

Faune des Vertébrés de la Suisse. Par Victor Fatio. Vol. V. "Histoire naturelle des Poissons." 2me partie, avec 4 planches, pp. 576. Supplémens, pp. 13. (Genève et Bale: H. Georg, 1890.)

As more than eight years have elapsed since the publication of the last volume of the "Faune des Vertébrés de la Suisse," I may preface this notice with a few words as to the general scope and progress of this important work. The first volume, published in the year 1869, was devoted to a detailed account of the Mammals of Switzerland; the third (1872) to the Reptiles and Batrachians; and the fourth (1882) to a part of the Fishes (Acanthopterygians and Cyprinoids); the second volume, which will contain the Birds, being still in course of preparation.

The part now published, which is the fifth of the series, treats of the remaining half of the fishes, notably the Salmonoids, which take up nearly two-thirds of the volume, and whose study has probably occupied the author by far the better half of the eight years which he has devoted to its preparation.

As regards the plan of the work, the thoroughness and originality with which the author treats his subject, and the fairness of his criticism of his predecessors, I may be allowed to refer to what I have said in my notice of the first volume of the Swiss ichthyology (Nature, vol. xxvii. p. 220); stating again that "this work rises far above the level of a local publication, and is of as great value to the student of European freshwater fishes as to the Swiss naturalist."

The species treated of in the present volume are the following: 3 loaches, 2 shad, 8 Coregoni, 1 grayling, 1 salmon, 1 trout, 1 char, 1 pike, 1 Silurus, 1 eel, 1 burbot, 1 sturgeon, 3 lampreys. These bring the total number of Swiss freshwater fishes to 51.

The hydrographic system of Switzerland comprises the head-waters of four rivers, viz. the Rhine, Rhone, Po, and Danube. The first contributes the largest contingent to the Swiss fish fauna, viz. 42 species; however, this number is reduced to 28 in the upper course of the river, above the falls of Schaffhausen. At an altitude of between 600 and 900 m. the majority of the Cyprinoids, and between 1000 and 1100 m. the perch, salmon, eel, and burbot disappear. Only five species remain at that altitude, viz. the miller's thumb, minnow, loach (*N. barbatulus*), grayling, and trout—species which likewise have the greatest horizontal range in a northward direction. Between 1800 and 1900 m., first the grayling and the loach are lost, and then successively the trout, miller's thumb, and minnow. The trout, however, can still subsist in lakes up to 2630 m., into which this fish has been introduced. The Rhine contributes five types of fishes to the Swiss fauna which are not found in the other hydrographic systems, viz. *Acerina* (the pope), *Rhodeus*, the salmon, the sea lamprey, and the stickleback. The absence in the southern and eastern waters of the four first is readily accounted for by their distribution generally; but it seems very singular that a fish like the stickleback, which in the west of Europe extends far southwards, and reaches even Algeria, and which is supposed to be capable of easy transportation by aquatic birds, should not have made its way into the other river-systems.

The fishes contributed by the Rhone fall into two categories—one comprising those of the part of the River Doubs which is within the political boundaries of Switzerland; the other including the species of the Rhone proper above the "Perte." The latter are computed to be 20 in number, and do not call for special remarks.

The fishes of the Po show a marked difference from those of the Rhine and Rhone. This system is represented in Switzerland by the tributary Ticino, into which 23 species enter, out of a total number of 44 Po species. Although there is no mechanical obstacle to their ascent, the fishes of the Po, used to a warmer climate, avoid ascending into the cold waters from the Alps; and M. Fatio observes, also, that generally these southern fish do not ascend to the same high altitudes as those of the Rhine. Eight of the Ticino species are strangers to the rest of Switzerland, viz. a goby (*Gobius*), which has ascended from the sea; five Cyprinoids, which may be regarded as southern representatives of northern forms; *Cobitis tænia* and *Alosa finta*.

Of the 68 species belonging to the fauna of the Danube, only four find their way into Switzerland through the River Inn, viz. the miller's thumb, minnow, grayling, and trout. This is owing to the great elevation of this river at its entrance into the country (1000 metres).

Ichthyologists will turn with particular interest to that part of the volume which contains Dr. Fatio's views on, and his treatment of, the Salmonids; for my own part, I could not help feeling some surprise at what appears to me a somewhat inconsistent mode of dealing with this subject. Whilst the author distinguishes not less than eight Swiss forms worthy of binominal designation in the genus *Coregonus*, he admits, besides one species of char (*Salmo umbla*), two equivalent forms only in the genus *Salmo*, viz. the salmon and the trout, for which latter the collective term *Salmo lacustris* is chosen. If a student of the European fauna, or any part of it, arrives at the conclusion that the various forms of river, lake, and sea trout cannot, and should not, be held to be deserving of specific distinction, no one will deny that there are very strong arguments in favour of this view. In my own experience it does not seem to be desirable to adopt that course—first, because there are certain well characterized and well localized forms which the practical fisherman will always distinguish, and of which the naturalist has, somehow, to take notice; and, secondly, because the ichthyologist who goes beyond the narrow limits of a fauna, and has to deal with the trout of the whole northern hemisphere, is compelled by technical considerations to admit those distinctions. I myself go a step further, and consider it a mistake not to separate, specifically, from the extremely variable *Salmo fario*, such strongly differentiated forms as *Salmo lemanus*, *S. marsitii*, *S. venernensis*, or the Loch Leven trout of the older authors. But if, as is Dr. Fatio's opinion, no taxonomic value is to be assigned to the characters by which those forms of trout are differentiated, then I cannot see why in *Coregonus*, a closely related genus of the same geological age and distribution, similar organic modifications should be considered to have a different bearing.

As is well known, there are some very obscure facts in the life-history of Salmonoids which greatly contribute to the difficulties of their study. Dr. Fatio discusses them very fully, but we must pass over the deductions he draws from them, with the exception of the phenomenon of sterility as a cause of change in the outward appearance of a fish. Sterility among Salmonoids is apparently much more common in Switzerland than in British waters; but ever since Siebold has drawn attention to it, its effects seem to me to have been exaggerated. At any rate, I have received specimens as, and, indeed, with all the outward characters of, the so-called sterile trout of Lake Constance, which had fully matured ova.

Like errata, appendices of works are only too often overlooked; I would therefore mention that the present volume concludes with important supplements to those which contain the Mammalia and Reptilia.

The volume is illustrated with four plates—one representing the *Bondelle* of the Lake of Neufchâtel, the others various details of structure, chiefly of Salmonoids.

I trust that before many years Dr. Fatio will be able to complete his work, for which, not only his countrymen, but every student of the European fauna, owe him a debt of gratitude.

ALBERT GÜNTHER.

THE HISTORY OF MARRIAGE.

The History of Human Marriage. By Edward Westermarck. (London: Macmillan and Co., 1891.)

BY "history" our author means "natural history" (p. 19), and his reason for using the odd term "human marriage" is that "marriage, in the natural history sense of the term, does not belong exclusively to our species" (p. 6). According to him, "marriage is nothing else than a more or less durable connection between male and female, lasting beyond the mere act of propagation till after the birth of the offspring." In this sense marriage is "an almost universal institution among birds," and "occurs as a rule among the monkeys, especially the anthropomorphous apes, as well as in the races of men" (p. 20). Among mankind it is universal, and in all probability is "an inheritance from some ape-like progenitor" (p. 538). In this book, therefore, marriage is taken to mean what ordinary people call "pairing," and the professed subject of the volume is the natural history of the habit of pairing in the human race. But surely, on any proper use of terms, marriage is not simple pairing, but such pairing as is protected and regulated by law, or by the public opinion which in rude societies stands for law. And the history of an institution which is controlled by public opinion and regulated by law is not natural history. The true history of marriage begins where the natural history of pairing ends.

Mr. Westermarck's definition leads him to go at length into various topics that really belong to natural history, but have little or nothing to do with the history of marriage in the ordinary sense of the word; such as sexual selection, and the means used by one sex to attract the other. But he also deals with polyandry, kinship through females only, infanticide, exogamy—all of which belong to the sphere of law and custom, within which his definition of marriage is totally inapplicable. To treat these topics as essentially a part of the natural history of pairing involves a tacit assumption that the laws of society are at bottom mere formulated instincts; and this assumption really underlies all our author's theories. His fundamental position compels him, if he will be consistent with himself, to hold that every institution connected with marriage that has universal validity, or forms an integral part of the main line of development, is rooted in instinct, and that institutions which are not based on instinct are necessarily exceptional, and unimportant for scientific history. One does not expect a tacit assumption to be carried out with perfect consistency; but, on the whole, Mr. Westermarck's results correspond with his assumption, and have no evidence to satisfy anyone that is not prepared to share the assumption with him.

To show this at length would exceed the limits of a short review; let us, however, take, as a crucial test, Mr. Westermarck's explanation of the origin of exogamy. He believes that exogamy and all laws of incest originate in an instinctive aversion to sexual intercourse between persons living closely together from early youth (p. 320), and the origin of this instinct he explains as follows. He thinks it can be proved that consanguineous marriages are detrimental to the species. Now,

> "among the ancestors of man, as among other animals, there was, no doubt, a time when blood-relationship was no bar to sexual intercourse. But variations, here as elsewhere, would naturally present themselves; and those of our ancestors who avoided in-and-in breeding would survive, while the others would gradually decay and ultimately perish. Thus an instinct would be developed which would be powerful enough, as a rule, to prevent injurious unions. Of course, it would display itself simply as an aversion on the part of individuals to union with others with whom they lived; but these, as a matter of fact, would be blood relations, so that the result would be survival of the fittest" (p. 352).

The obvious and fatal objection to this theory is that it postulates the existence of groups which through many generations (for the survival of the fittest implies this) avoided wiving within the group. And this is, in fact, a well-established custom of exogamy, so that the theory begins by postulating the very custom that it professes to explain. Moreover, it is questionable whether Mr. Westermarck's theory even helps to explain the wide diffusion of exogamy. For where wiving outside the local group is the rule, all neighbouring groups mingle their bloods, and consanguineous marriages are not escaped.

It is not surprising that Mr. Westermarck, with his habit of looking at the whole subject from a biological point of view, should have little sympathy with the speculations of a man like McLennan, to whom marriage is not a mere fact of natural history, but a relationship resting on contract and approved by custom or law; and who in all his investigations gives weight to the action of human intelligence as the decisive factor in social progress. But it is a pity that this lack of sympathy has sometimes prevented our author from appreciating the full scope of McLennan's methods and arguments. What is said about the Levirate at pp. 510–14 could not have been written if Mr. Westermarck had carefully read the discussion of the subject in "The Patriarchal Theory"; nor, to mention a trivial matter, would he in that case have made the error of confounding the Hindu Levirate with the Nyoga (p. 514, *note*). And here I may also note that the criticism of McLennan's views of exogamy does not take account of the posthumous and very important paper published in the *English Historical Review* for January 1888.

These are details: what is more to be regretted is that Mr. Westermarck has not learned, as he might have done from McLennan, a sounder method of handling the evidence drawn from the usages of rude societies. The very possibility of reconstructing the history of human progress rests on the fact that all over the world mankind has been moving in the same general direction, but at very various rates, and that careful reasoning, aided especially by the observation of cases which exhibit a state of transition (*e.g.* from one type of kinship to another), enables us to bring out the order in which the various observed types of social structure succeed one another. Of all this, Mr. Westermarck does not seem to have the least idea. He collects facts about the prevalence of kinship through males or through females, about forbidden degrees, and so forth, without ever rising to the conception that the evidence is good for anything more than an *inductio per enumerationem simplicem*. This is not the way in which real progress can be made.

W. ROBERTSON SMITH.

OUR BOOK SHELF.

Geological Map of Monte Somma and Vesuvius. Constructed by H. J. Johnston-Lavis, M.D., M.R.C.S., B.-ès-Sc., F.G.S., &c., during the Years 1880–88. Scale, 1 : 10,000 (6·33 inches = 1 mile). In Six Sheets, with a Pamphlet entitled "A Short and Concise Account of the Eruptive Phenomena and Geology of Monte Somma and Vesuvius." (London: George Philip and Son, 1891.)

DURING the latter half of last century, the changes taking place in Vesuvius were carefully studied and faithfully chronicled by an English diplomatist—Sir William Hamilton; in the closing years of the present century, the famous volcano has found an equally indefatigable investigator and historian in the person of an English medical man resident in Naples—Dr. Johnston-Lavis. In 1884, Dr. Johnston-Lavis laid before the Geological Society an elaborate memoir, in which he detailed the theoretical conclusions at which he had arrived after long and patient study of the various sections exposed on the flanks of Somma and Vesuvius. He has now published a very valuable addition to this work, in the form of a map constructed on the basis of the topographical surveys of the Italian Government, and coloured in accordance with the views to which he has been led by his long and painstaking geological labours.

In his general memoir on the geology of Somma and Vesuvius, the author has divided the time covered by the history of the volcano into four "eras," and these again into eight "phases," while some of the latter are subdivided into "periods." In colouring the map, it has, of course, not been found possible to give expression to anything like such a minute classification of the rocks composing the mountain as is implied in such a scheme. The legend on the map recognizes as the great landmarks in the past history of the volcano the paroxysm of 79 A.D. and the great eruption of 1631. The pamphlet accompanying the map, however, gives a very useful and readable abstract of the earlier memoir; and the map and descriptive pamphlet together cannot fail to prove of the greatest service to all students of vulcanology. By their publication, Dr. Johnston-Lavis has added one more to the long list of valuable services which he has rendered to geological science.

Les Sciences Naturelles et l'Éducation. Par T. H. Huxley. Édition Française. (Paris: Baillière et Fils, 1891.)

THIS is a translation of various essays with which all English students of Prof. Huxley's writings have long been familiar. Most of them deal with various aspects of the question as to the true place of science in a proper system of education; and no one who has read them in their original form is likely to have forgotten the philosophical power with which the subject is discussed, or the admirable lucidity, strength, and grace of the writer's style. With his educational papers Prof. Huxley has associated his well-known essays on Descartes and Auguste Comte, which cannot fail to be of interest to French readers. He contributes to the volume a short preface, in which he refers with satisfaction to the astonishing advance that has been made in the recognition of science as an instrument of education. He warns men of the younger generation, however, that the battle has only been half won, and that much serious work will have to be done to secure the triumph of the principles for which he has contended. Of the translation it may be enough to say that Prof. Huxley cordially commends it as a faithful rendering of his thought.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

W. E. Weber.

IN the article on Wilhelm Weber (NATURE, July 9, p. 229) no mention is made of the fact that Weber and Gauss in 1833 invented and constructed a telegraph connecting the Physical Laboratory of the University in Göttingen with the Observatory. In Germany they are for this reason said to be the inventors of telegraphy. This is, to say the least, a somewhat sweeping statement, as the possibility of communicating by electricity was known long before that time. However, there is no doubt that Weber and Gauss played some part in introducing telegraphy into practice. For my part I consider the purely scientific work of either of the two men more glorious. For the enormous practical consequences of telegraphy have nothing to do with the scientific merit of the invention. Nevertheless I think that an article on Wilhelm Weber would not be complete without entering into this subject.

C. RUNGE.

Hanover, Technische Hochschule, July 13.

[Conducting wires were erected between the Göttingen Observatory and the Physical Cabinet of the University, distant about three-quarters of an English mile, in order to obtain accurate comparisons of the clocks. But, in addition to systematic daily transmission of time, the wires were from the first frequently used for telegraphic purposes, though, with the first arrangements, only two letters could be sent in a minute.—G. C. F.]

Earthquake Shocks in Italy and Australia.

BY a telegram from London, which appeared in the newspapers here on the 12th and 13th inst., information is given of a severe earthquake in Italy (about Vesuvius) on June 7 (Sunday). On that day, several distinct and well-marked shocks were felt over parts of the south of Australia, and as there may be some connection between these seismic disturbances in both hemispheres, I give below the times and other information of the disturbances experienced here.

None of the disturbances reported in Australia seem to have been more than a "shake" or *sharp tremor* sufficient to shake windows and rattle crockery, &c., but they were enough, in some instances, to produce feelings of nausea.

June 7, at 2.5 p.m., the first disturbance occurred, and was felt all around Melbourne and over a surrounding area of 360 square miles.

June 7, at 2.45, another shake (not so great as the first) was also felt; in this case it was felt most severely to the east of Melbourne.

June 7, at 7.20, smart shock felt at Kapunda, South Australia.

June 7, at 6.45, slight shock felt at Stockport, South Australia. The direction of motion is variously given as from north-west to south-east, south-east to north-west, and south-west to north-east, north to south, south to north, &c. The conclusion arrived at is that the wave was from south to north nearly. The approximate geographical positions of the several localities where these disturbances occurred are as follows:—

	Lat.	Long.
Stockport	34° 21′ S.	138° 57′ E.
Kapunda	34° 21′ S.	138° 46′ E.
Melbourne	37° 50′ S.	144° 58′ E.

Melbourne, June 15.

R. L. J. ELLERY.

P.S.—It is quite probable the shocks felt at Kapunda and Stockport were one and the same, as time is not very strictly kept in districts distant from large towns in Australia.

Force and Determinism.

I SEE nothing to criticize in Mr. Dixon's middle paragraph, wherein he accurately summarizes some of the definitions of mechanics, except that I should prefer to express the meaning of his last sentence by saying that, if in any department something simulated the functions of, say, energy, without obeying its precise mechanical laws, then the distinction between energy and that something should be clearly recognized, and another name be given to it.

I find it rather common for "life" to be thought of and classed under the head energy, either by the use of a phrase such as "vital energy," or in a more direct way; the reason being apparently that organisms while living simulate some of the functions of energy, and cease to do so when dead. It was against this confusion that I wrote on p. 491 (vol. xliii.).

Life has not yet been included in the domain of physics, neither has it, so far as I am aware, been much studied under the head biology.

And yet the disturbing action of live animals will have to be formulated and attended to some day, even in physics; for, though they generate no energy nor affect its amount in the slightest degree, they certainly control it and direct it in channels it would not otherwise have taken. The question is, How do they manage this? And one answer that may be given is, By exerting directive or guiding forces on matter.

Of course they are not limited to this, but in so far as they do work their action is fairly understood: the energy displayed by a gang of navvies is known to be derived from the little tin cans they bring with them: the energy is not theirs but their victuals', they simply direct it. But how comes it that they can direct the energy of victuals and atmosphere into the erection of the precise bridge or other structure which has been planned? What determines the direction of the transfer of energy?

The same question may doubtless be asked in connection with inanimate activity: I would not be understood as assuming for certain any clear or essential difference between the two cases; but in neither case do I know the answer.

The action of force in doing work, *i.e.* transferring and transforming quantities of energy, has been thoroughly attended to.

The action of force in directing and guiding the transfer of matter and energy does not seem to me to have been seriously contemplated.

In his most recent book ("The Philosophical Basis of Evolution") Dr. Croll attacks the problem, and says that guidance is effected by "determinism" not by force. But that cannot be admitted; for without force the motion of matter cannot be changed in direction any more than in speed. *Force* is certainly necessary to direct the motion of matter, it is *energy* only which is unnecessary; for any transfer of energy that may occur is an accidental, not an essential, concomitant.

I determine to move an object: it may be only my finger, or it may be a wheelbarrow. In so far as I do any work in the action I do so at the expense of my food, and there is nothing but a chemical difficulty about that. The mystery begins when one asks how I manage to direct that energy along a definite path so as to produce a willed result. The only answer I know is, "By a nervous impulse liberated from brain centres." But what is it that is thus liberated? and what pulls the trigger to liberate it?

By mechanical analogy one would say that energy can only be guided by force, and that force must therefore be exerted in the brain cells; but, if so, the relation between force, which is a mechanical thing, and will or life, or whatever it is, which is a psychological thing, demands investigation.

I trust that Mr. Lloyd Morgan will help me to get my ideas on these subjects straighter, and will point out if I have made any assertions which are obviously erroneous or grotesque. The borderland of psychology and physics is the last place in which I would like to dogmatize; and in a letter like this I see no harm in airing confessedly immature and groping notions, in the hope that ventilation may clear the air. So far as physics only is concerned, I have stated how I regard the phrase "expenditure of energy" in the *Philosophical Magazine* for June 1885

With regard to the crux raised in Mr. Dixon's last paragraph, that nothing but matter can exert force, because the acting matter must receive an equal opposite momentum, it may perhaps be just worth noticing that an infinite mass can absorb any amount of momentum without receiving a trace of energy or being itself in any way affected. OLIVER J. LODGE.

Liquid Prisms.

I OBSERVE in NATURE of July 2 (p. 207), that it is stated Herr Wolter has recently recommended α-monobromnaphthalene as a substance peculiarly fitted for study of the ultra-violet part of the spectrum, by reason of its high dispersive power and transparency for the ultra-violet rays.

Perhaps I may be permitted to state that Mr. Madan published an account of its dispersion and refractive power in the *Phil. Mag.*, and recommended its use in liquid prisms. Having made use of many other substances, including methyl salicylate, I gave this a trial. For ordinary work it would be excellent if colourless; but unfortunately, no matter how free from colour it may be when freshly prepared, long-continued use causes it to become yellow, and in considerable thicknesses even dark brown. For the ultra-violet rays it is undoubtedly better than carbon disulphide, but nevertheless practically useless, as the line N, which it is said to transmit, has a wave-length of 3580, so that only about one-half of the ultra-violet solar rays are observable with it. In metallic spectra almost all lines of interest lie between 3580 and 2000. A liquid which I considered to possess much superior optical properties is *mercuric methide;* it is perfectly colourless, and of such density that flint glass will float upon it. When the glass is immersed it becomes invisible, consequently the refraction and dispersion of the liquid are probably exceptionally high. As far as I can recollect, being without access to my notes, a thickness of 50 millimetres freely transmitted all rays to about λ 2900—that is to say, the entire solar spectrum. Unfortunately, it has its drawbacks, in being somewhat volatile, and its vapour highly poisonous.

Stonehaven, N.B. W. N. HARTLEY.

The Identification of Templeton's British Earthworms.

BETWEEN the years 1829 and 1836 the first series of Loudon's *Magazine of Natural History* appeared in nine volumes. In the last volume we find some notes on earthworms by Templeton, which have proved somewhat puzzling to students of more recent times. I have been fortunate enough to follow Templeton in some of his researches, and am able to correct and verify certain of his statements.

The *Lumbricus xanthurus*, Temp. (*op. cit.*, ix. 235), is the angler's gilt tail, and as such is synonymous with *Lumbricus puter*, Hoffm., and *Dendrobæna Boeckii*, Eisen. *Lumbricus gordianus*, Temp. (*loc. cit.*), is undoubtedly the mucous worm (*Allolobophora mucosa*, Eisen), or one of its near allies, all of which are to be found of a pale rosy colour coiled up into a knot at certain times of the year.

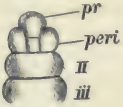
It is to *Lumbricus omilurus* (= *Omilurus rubescens*, Temp., *loc. cit.*), however, that I wish to direct special attention. Grube, in 1851 ("Familien der Anneliden," p. 101), placed it, with Templeton's other worms, in a list of species which were insufficiently characterized for systematic purposes. Vejdovsky, in 1884 ("System und Morph. der Oligochæten," p. 62), places it among the questionable species without note or comment, and, so far as I can gather, no one has been able to throw light upon it since.

Templeton says the worm is never larger than half the size of *L. terrestris*, L., is of a bright reddish-brown, with the tail very flat, and the body *unfurnished with a belt at the position of the sexual organs.* It would be very easy to suppose from this somewhat vague account that the writer had only seen immature specimens; but a little careful study of his words shows that he knew what he was writing, and that his worms were mature. Now a mature species of *Lumbricus* without a clitellum is certainly an anomaly, and needs investigation.

While collecting Annelids recently, I came across half-a-dozen specimens which at first sight exactly resembled *Lumbricus rubellus*, Hoffm. I took them home for verification, and immediately observed the difference. I had obtained with them typical specimens of *rubellus*, which enabled me to make a careful comparison of the two species in a living state.

The following is a description of the worm as I wrote it down before observing Templeton's account.

Colour dark brown, iridescent on the dorsal surface anteriorly, becoming lighter towards the posterior extremity, which is flesh-coloured or light red, pink ventrally. Prostomium dovetailing completely into the peristomium, and possessing a transverse groove in the middle, as shown in the accompanying sketch.



Lumbricus rubescens. Segments 1 to 3 with prostomium entirely cutting the first segment or peristomium.

Segments not annulated (or divided by transverse rings). Length about 3 inches, total number of segments about 120. Setæ in couples as in typical Lumbricus. Male or spermiducal pores on segment 15 with papillæ, which, however, do not extend over the neighbouring segments. Body cylindrical in front, flattened posteriorly. The dorsal pore between 5 and 6. It appeared at first between 7 and 8, but by using polarized light on the cuticle when spread on a glass slip the whole series of pores in one or two specimens became clearly visible from the fifth segment backwards.

On the ventral surface prominent papillæ appeared on segments 28 and 29, such as are often seen on typical *L. agricola*, Hoffm. Now came the crucial question, Is there no clitellum? By studying all the examples carefully, I found that they agreed in one particular. The segments 34 to 39 differed in structure from the rest on the dorsal surface. On the under surface from 33 to 40 were differentiated, and showed a glandular structure, while the band representing the tubercula pubertatis extended distinctly along the ventral surface of 35, 36, 37, 38.

This description of the external characters shows the worm to be a decided Lumbricus, tested by Dr. Benham's definition in "An Attempt to Classify Earthworms"; but it differs from every one of our British species, especially in the backward position and inconspicuous nature of the clitellum. I am unable to refer it definitely to any of the European species, and propose that for the present it should be known as *Lumbricus rubescens* (Temp.), thus retaining the two names from Templeton's synonyms which are most appropriate to what I regard as the species intended by him.

I may add that I have recently found one or two other earthworms in Yorkshire which have not yet been recorded as British, and will form interesting additions to our Annelid fauna.

Idle, near Bradford, July 15. HILDERIC FRIEND.

Copepoda as an Article of Food.

DURING recent years a good deal has been said amongst marine zoologists of the use, as a food supply, that might be made of the enormous numbers of Copepoda that swarm in the surface-waters of the sea, and the Prince of Monaco has pointed out the value this widely-distributed nutritious matter might have to shipwrecked sailors; but I am not aware that anyone has yet actually made the experiment of cooking and eating Copepoda, so the following record may be of some interest.

While townetting during the last few days about the North Cape, we have had some large hauls of Copepoda; and it occurred to us last night, while watching the midnight sun off the entrance to the Lyngen Fjord, that one gathering might be spared from the preserving bottle and devoted to the saucepan. We put out one of the smaller townets ($3\frac{1}{2}$ feet long, mouth 1 foot in diameter) from 11.40 p.m. to midnight, the ship going dead slow, and traversing in all, say, a mile and a half during the 20 minutes. The net when hauled in contained about three tablespoonfuls of a large red Copepod (*Calanus finmarchicus*, I think), apparently a pure gathering—what Haeckel would call a monotonic plankton. We conveyed our material at once to the galley, washed it in a fine colander, boiled it for a few minutes with butter, salt, and pepper, poured it into a dish, covered it with a thin layer of melted butter, set it in ice to cool and stiffen, had it this morning for breakfast on thin bread-and-butter, and found it most excellent. The taste is less pronounced than that of shrimps, and has more the flavour of lobster. Our 20 minutes' haul of the small net through a mile or two of sea made, when cooked in butter, a dishful which was shared by eight people, and would probably have formed, with biscuit or bread, a nourishing meal for one person. It would apparently, in these seas, be easy to gather very large quantities, which might be preserved in tins or dishes, like potted shrimps. W. A. HERDMAN.

S.Y. *Argo*, Tromsö, Norway, July 13.

Are Seedlings of *Hemerocallis fulva* specially Variable?

I SHALL be grateful to any of your readers who will write and let me know their experiences as to the variability of seedlings of *Hemerocallis fulva*, or who will raise it from seed in fair quantity, and kindly communicate to me their results, which shall be duly acknowledged.

My reason is this: there is in the formation of the pollen in this plant a peculiarity which, according to Weismann's views, should lead to exceptional variability in the seedlings; but, so far as I know, we have no evidence on the subject.

MARCUS M. HARTOG.

Royal University, Dublin, July 9.

The Green Sandpiper.

ON Sunday last, July 12, I saw flying round a large pool in Essex, a specimen of the *green sandpiper*. It flew leisurely round the pool, and seemed as if it were not far from its summer home. I think, therefore, that the bird must be nesting in the county, and probably in the neighbourhood.

Can any of your correspondents inform me whether the nest has been found anywhere, in recent years, in England?

ARGYLL.

Argyll Lodge, Kensington, July 17.

LIQUIDS AND GASES.[1]

ALMOST exactly twenty years ago, on June 2, 1871, Dr. Andrews, of Belfast, delivered a lecture to the members of the Royal Institution in this hall, on "The Continuity of the Gaseous and the Liquid States of Matter." He showed in that lecture an experiment which I had best describe in his own words:—

"Take, for example, a given volume of carbonic acid at 50° C., or at a higher temperature, and expose it to increasing pressure till 150 atmospheres have been reached. In the process, its volume will steadily diminish as the pressure augments; and no sudden diminution of volume, without the application of external pressure, will occur at any stage of it. When the full pressure has been applied, let the temperature be allowed to fall, until the carbonic acid has reached the ordinary temperature of the atmosphere. During the whole of this operation, no break of continuity has occurred. It begins with a gas, and by a series of gradual changes, presenting nowhere any abrupt alteration of volume, or sudden evolution of heat, it ends with a liquid.

"For convenience, the process has been divided into two stages—the compression of the carbonic acid, and its subsequent cooling. But these operations might have been performed simultaneously, if care were taken so to arrange the application of the pressure and the rate of cooling, that the pressure should not be less than 76 atmospheres when the carbonic acid had cooled to 31°."

I am able, through the kindness of Dr. Letts, Dr. Andrews' successor at Belfast, to show you this experiment, with the identical piece of apparatus used on the occasion of the lecture twenty years ago.

I must ask you to spend some time to-night in considering this remarkable behaviour; and, in order to obtain a correct idea of what occurs, it is well to begin with a study of gases, not, as in the case you have just seen, exposed to high pressures, but under pressures not differing greatly from that of the atmosphere, and at temperatures which can be exactly regulated and measured. To many here to-night, such a study is unnecessary, owing to its familiarity; but I will ask such of my audience to excuse me, in order that I may tell my story from the beginning.

Generally speaking, a gas, when compressed, decreases in volume to an amount equal to that by which its pressure is raised, provided its temperature be kept constant. This was discovered by Robert Boyle in 1660; in 1661 he presented to the Royal Society a Latin translation of his book, "Touching the Spring of the Air and its Effects." His words are:—

"'Tis evident, that as common air, when reduced to half its natural extent, obtained a spring about twice as forcible as it had before; so the air, being thus compressed, being further crowded into half this narrow room, obtained a spring as strong again as that it last had, and consequently four times as strong as that of common air."

To illustrate this, and to show how such relations may be expressed by a curve, I will ask your attention to this model. We have a piston, fitting a long horizontal glass tube. It confines air under the pressure of the atmosphere—that is, some 15 pounds on each square inch of area of the piston. The pressure is supposed to be registered by the height of the liquid in the vertical tube. On increasing the volume of the air, so as to double it, the pressure is decreased to half its original amount. On decreasing the volume to half its original amount, the pressure is doubled. On again halving, the pressure is again doubled. Thus you see a curve may be traced, in which the relation of volume to pressure is exhibited. Such a curve, it may be remarked incidentally, is termed an hyperbola.

We can repeat Boyle's experiment by pouring mercury into the open limb of this tube containing a measured amount of air; on causing the level of the mercury in the open limb to stand 30 inches (that is, the height of the barometer) higher in the open limb than the closed limb, the pressure of the atmosphere is doubled, and the volume is halved. And on trebling the pressure of the atmosphere the volume is reduced to one-third of its original amount; and, on adding other 30 inches of mercury, the volume of the air is now one-quarter of that which it originally occupied.

It must be remembered that here the temperature is kept constant; that it is the temperature of the surrounding atmosphere.

Let us next examine the behaviour of a gas when its temperature is altered, when it becomes hotter. This tube contains a gas—air—confined at atmospheric pressure by mercury, in a tube surrounded by a jacket or mantle of glass, and the vapour of boiling water can be blown into the space between the mantle and the tube containing the air, so as to heat the tube to 100°, the temperature of the steam. The temperature of the room is 17° C., and the gas occupies 290 divisions of the scale. On blowing in steam, the gas expands, and on again equalizing pressure, it stands at 373 divisions of the scale. The gas has thus expanded from 290 to 373 divisions, *i.e.* its volume has increased by 83 divisions; and the temperature has risen from 17° to 100°, *i.e.* through 83°. This law of the expansion of gases was discovered almost simultaneously by Dalton and Gay-Lussac in 1801; it usually goes by the name of Gay-Lussac's law. Now, if we do not allow the volume of the gas to increase, we shall find that the pressure will increase, in the same proportion that the volume would have increased had the gas been allowed to expand, the pressure having been kept constant. To decrease the volume of the gas, then, according to Boyle's law, will require a higher initial pressure; and if we were to represent the results by a curve, we should get an hyperbola, as before, but one lying higher as regards pressures. And so we should get a set of hyperbolas for higher and higher temperatures.

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[1] Lecture delivered by Prof. W. Ramsay, F.R.S., at the Royal Institution, on Friday, May 8.

We have experimented up to the present with air—a mixture of two gases, oxygen and nitrogen; and the boiling-points of both of these elements lie at very low temperatures: – 184° and – 193°·1 respectively. The ordinary atmospheric temperature lies a long way above the boiling-points of liquid oxygen and liquid nitrogen at the ordinary atmospheric pressure. But it is open to us to study a gas, which, at the ordinary atmospheric temperature and pressure, exists in the liquid state; and for this purpose I shall choose water-gas. In order that it may be a gas at ordinary atmospheric pressure, however, we must heat it to a temperature above 100° C., its boiling-point. This tube contains water-gas at a temperature of 105° C.; it is under ordinary pressure, for the mercury columns are at the same level in both the tubes and in this reservoir, which communicates with the lower end of the tube by means of the india-rubber tubing. The temperature 105° is maintained by the vapour of chlorobenzene, boiling in the bulb sealed to the jacket, at a pressure lower than that of the atmosphere.

Let us now examine the effect of increasing pressure. On raising the reservoir, the volume of the gas is diminished, as usual; and nearly in the ratio given by Boyle's law; that is, the volume decreases in the same proportion as the pressure increases. But a change is soon observed; the pressure soon ceases to rise; the distance between the mercury in the reservoir and that in the tube remains constant, and the gas is now condensing to liquid. The pressure continues constant during this change; and it is only when all the water-gas has condensed to liquid water that the pressure again rises. After all gas is condensed, an enormous increase of pressure is necessary to cause any measurable decrease in volume, for liquid water scarcely yields to pressure, and in such a tube as this, no measurements could be attempted with success.

Representing this diagrammatically, the right-hand part of the curve represents the compression of the gas; and the curve is, as before, nearly a hyperbola. Then comes a break, and great decrease in volume occurs without rise of pressure, represented by a horizontal line; the substance in the tube here consists of water-gas in presence of water; the vertical, or nearly vertical line represents the sudden and great rise of pressure, where liquid water is being slightly compressed. The pressure registered by the horizontal line is termed the "vapour-pressure" of water. If, now, the temperature were raised to 110°, we should have a greater initial volume for the water-gas; it is compressible by rise of the mercury as before, the relation of pressure to volume being, as before, represented on the diagram as an approximate hyperbola; and as before, condensation occurs when volume is sufficiently reduced, but this time at a higher pressure. We have again a horizontal portion, representing the pressure of water-gas at 110° in contact with liquid water; again, a sharp angle where all gaseous water is condensed, and again a very steep curve, almost a straight line, representing the slight decrease of volume of water produced by a great increase of pressure. And we should have similar lines for 120°, 130°, 140°, 150°, and for all temperatures within certain limits. Such lines are called isothermal lines, or shortly "isothermals," or lines of equal temperature, and represent the relations of pressure to volume for different temperatures.

Dr. Andrews made similar measurements of the relations between the pressures and volumes of carbon dioxide, at pressures much higher than those I have shown you for water. But I prefer to speak to you about similar results obtained by Prof. Sydney Young and myself with ether, because Dr. Andrews was unable to work with carbon dioxide free from air, and that influenced his results. For example, you see that the meeting-points of his hyperbolic curves with the straight lines of vapour-pressures are curves, and not angles; that is caused by the presence of about 1 part of air in 500 parts of carbon dioxide; also the condensation of gas was not perfect, for he obtained curves at the points of change from a mixture of liquid and gas to liquid. We, however, were more easily able to fill a tube with ether free from air, and you will notice that the points I have referred to are angles, not curves.

Let me first direct your attention to the shapes of the curves in the diagram. As the temperature rises, the vapour-pressure lines lie at higher and higher pressures, and the lines themselves become shorter and shorter. And finally, at the temperature 31° for carbon dioxide, and at 195° for ether, there ceases to be a horizontal portion at all; or rather, the curve touches the horizontal at one point in its course. That point corresponds to a definite temperature, 195° for ether; to a definite pressure, 27 metres of mercury, or 35·6 atmospheres; and to a definite volume, 4·06 cubic centimetres per gram of ether. At that point the ether is not liquid, and it is not gas; it is a homogeneous substance. At that temperature ether has the appearance of a blue mist; the striæ mentioned by Dr. Andrews, and by other observers, are the result of unequal heating, one portion of the substance being liquid, and another gas. You see the appearance of this state on the screen.

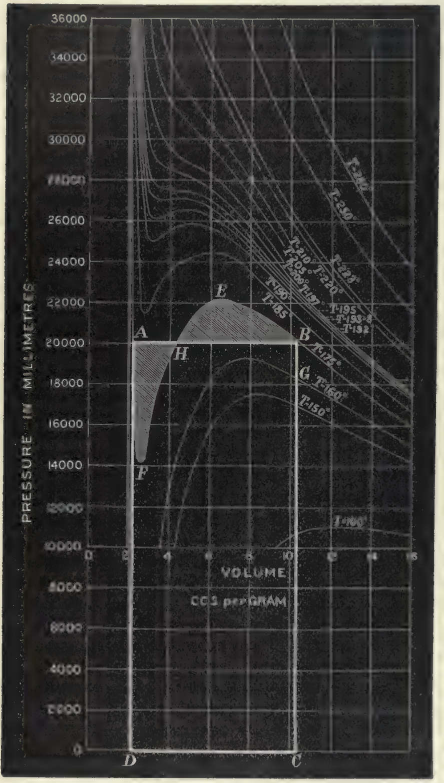
When a gas is compressed, it is heated. Work is done on the gas, and its temperature rises. If I compress the air in this syringe forcibly, its temperature rises so high that I can set a piece of tinder on fire, and by its help explode a little gunpowder. If the ether at its critical point be compressed by screwing in the screw, it is somewhat warmed, and the blue cloud disappears. Conversely, if it is expanded a little by unscrewing the screw, and increasing its volume, it is cooled, and a dense mist is seen, accompanied by a shower of ether rain. This is seen as a black fog on the screen.

I wish also to direct your attention to what happens if the volume given to the ether is greater than the critical volume—on increasing the volume, you see that it boils away and evaporates completely; and also what happens if the volume be somewhat less than the critical volume—it then expands as liquid, and completely fills the tube. It is only at the critical volume and temperature that the ether exists in the state of blue cloud, and has its critical pressure. If the volume be too great, the pressure is below the critical pressure; if too small, the pressure is higher than the critical pressure.

Still one more point before we dismiss this experiment. At a temperature some degrees below the critical temperature, the meniscus, *i.e.* the surface of the liquid, is curved. It has a skin on its surface; its molecules, as Lord Rayleigh has recently explained in this room, attract one another, and it exhibits surface-tension. Raise the temperature, and the meniscus grows flatter; raise it further, and it is nearly flat, and almost invisible; at the critical temperature it disappears, having first become quite flat. Surface-tension, therefore, disappears at the critical point.

A liquid would no longer rise in a narrow capillary tube; it would stand at the same level outside and inside.

It was suggested by Prof. James Thomson, and by Prof. Clausius about the same time, that if the ideal state of things were to exist, the passage from the liquid to the gaseous state should be a continuous one, not merely at and above the critical point, but below that temperature. And it was suggested that the curves, shown in the figure, instead of breaking into the straight line of vapour-pressure, should continue sinuously. Let us see what this conception would involve.



On decreasing the volume of a gas, it should not liquefy at the point marked B on the diagram, but should still decrease in volume on increase of pressure. This decrease should continue until the point E is reached. The anomalous state of matters should then occur, that a decrease in volume should be accompanied by a decrease of pressure. In order to lessen volume, the gas must be exposed to a continually diminishing pressure. But such a condition of matter is of its nature unstable, and has never been realized. After volume has been decreased to a certain point, F, decrease of volume is again attended by increase of pressure, and the last part of the curve is continuous with the realizable curve representing the compression of the liquid, above D.

Dr. Sydney Young and I succeeded, by a method which I shall briefly describe, in mapping the actual position of the unrealizable portions of the curve. They have the form pictured in this figure. The rise from the gaseous state is a gradual one; but the fall from the liquid state is abrupt.

Consider the volume 14 cubic centimetres per gram on the figure. The equi-volume vertical line cuts the isothermal lines for the temperatures 175°, 180°, 185°, 190°, and so on, at certain definite pressures, which may be read from a properly-constructed diagram. We can map the course of lines of equal volume, of which the instance given is one, using temperatures as ordinates and pressures as abscissæ. We can thus find the relations of temperature to pressure for certain definite volumes, which we may select to suit our convenience—say, 2 c.c. per gram; 3, 4, 5, 6, and so on. Now, all such lines are straight—that is, the relation of pressure to temperature, at constant volume, is one of the simplest; pressure is a linear function of temperature. Expressed mathematically—

$$p = bt - a,$$

where b and a are constants, depending on the volume chosen, and varying with each volume. But a straight line may be extrapolated without error; and so, having found values for a and b for such a volume as 6 c.c. per gram, by help of experiments at temperatures higher than 195°, it is possible by extrapolation to obtain the pressures corresponding to temperatures below the critical point 195° by simple means. But below that temperature the substance at volume 6 is in practice partly liquid and partly gas. Yet it is possible by such means to ascertain the relations of pressure to temperature for the *unrealizable portion* of the state of a liquid—that is, we can deduce the pressure and temperature corresponding to a continuous change from liquid to gas. And in this manner the sinuous lines on the figure have been constructed.

It is possible to realize experimentally certain portions of such continuous curves. If we condense all gaseous ether, and, when the tube is completely filled with liquid, carefully reduce pressure, the pressure may be lowered considerably below the vapour-pressure corresponding to the temperature of ebullition, without any change further than the slight expansion of the liquid resulting from the reduction of pressure—an expansion too small to be seen with this apparatus. But on still further reducing pressure, sudden ebullition occurs, and a portion of the liquid suddenly changes into gas, while the pressure rises quickly to the vapour-pressure corresponding to the temperature. If we are successful in expelling all air or gas from the ether in filling the tube, a considerable portion of this curve can be experimentally realized.

The first notice of this appearance, or rather of one owing its existence to a precisely similar cause, is due to Hooke, the celebrated contemporary of Boyle. It is noted in the account of the proceedings of the Royal Society on November 6, 1672, that "Mr. Hooke read a discourse of his, containing his thoughts of the experiment of the quicksilver's standing top-full, and far above the height of 29 inches; together with some experiments made by him, in order to determine the cause of this strange phenomenon. He was ordered to prepare those experiments for the view of the Society." And on November 13 "the experiment for the high suspension of quicksilver being called for, it was found that it had failed. It was ordered that thicker glasses should be provided for the next meeting."

There can be no doubt that this behaviour is caused by the attraction of the molecules of the liquid for each other. And if the temperature be sufficiently low, the pressure may be so reduced that it becomes negative—that is, until the liquid is exposed to a strain or pull, as is the mercury. This has been experimentally realized by M. Berthelot and by Mr. Worthington, the latter of whom has succeeded in straining alcohol at the ordinary temperature with a pull equivalent to a negative pressure of 25 atmospheres, by completely filling a bulb with alcohol, and then cooling it. The alcohol in contracting strains the bulb inwards; and finally, when the tension becomes very great, parts from the glass with a sharp "click."

To realize a portion of the other bend of the curve, an experiment has been devised by Mr. John Aitken. It is as follows:—If air—that is, space, for the air plays a secondary part—saturated with moisture be cooled, the moisture will not deposit unless there are dust-particles on which condensation can take place. It is not at first evident how this corresponds to the compressing of a gas without condensation. But a glance at the figure will render the matter plain. Consider the isothermal 175° for ether, at the point marked A. If it were possible to lower the temperature to 160°, without condensation, keeping volume constant, pressure would fall, and the gas would then be in the state represented on the isothermal line 160°, at G: that is, it would be in the same condition as if it had been compressed without condensation.

You saw that a gas, or a liquid, is heated by compression; a piece of tinder was set on fire by the heat evolved on compressing air. You saw that condensation of ether was brought about by diminution of pressure—that is, it was cooled. Now, if air be suddenly expanded, it will do work against atmospheric pressure, and will cool itself. This globe contains air; but the air has been filtered carefully through cotton-wool, with the object of excluding dust-particles. It is saturated with moisture. On taking a stroke of the pump, so as to exhaust the air in the globe, no change is evident; no condensation has occurred, although the air has been so cooled that the moisture should condense, were it possible. On repeating the operation with the same globe, after admitting dusty air—ordinary air from this room—a slight fog is produced, and, owing to the light behind, a circular rainbow is seen; a slight shower of rain has taken place. There are comparatively few dust particles, because only a little dusty air has been admitted. On again repeating, the fog is denser; there are more particles on which moisture may condense.

One point more, and I have done. Work is measured by the distance or height through which a weight can be raised against the force of gravity. The British unit of work is a foot-pound—that is, a pound raised through one foot; that of the metric system is one gram raised through one centimetre. If a pound be raised through two feet, twice as much work is done as that of raising a pound through one foot, and an amount equal to that of raising two pounds through one foot. The measure of work is therefore the weight multiplied by the distance through which it is raised. When a gas expands against pressure, it does work. The gas may be supposed to be confined in a vertical tube, and to propel a piston upwards, against the pressure of the atmosphere. If such a tube has a sectional area of one square centimetre, the gas in expanding a centimetre up the tube lifts a weight of nearly 1000 grams through one centimetre; for the pressure of the atmosphere on a square centimetre of surface is nearly 1000 grams—that is, it does 1000 units of work, or ergs. So the work done by a gas in expanding is measured by the change of volume multiplied by the pressure. On the figure, the change of volume is measured horizontally, the change of pressure vertically. Hence the work done is equivalent to the area ABCD on the figure.

If liquid, as it exists at A, change to gas as it exists at B, the substance changes its volume, and may be made to do work. This is familiar in the steam-engine, where work is done by water, expanding to steam and so increasing its volume. The pressure does not alter during this change of volume, if sufficient heat be supplied, hence the work done during such a change is given by the rectangular area.

Suppose that a man is conveying a trunk up to the first story of a house, he may do it in two (or, perhaps, a greater number of) ways. He may put a ladder up to the drawing-room window, shoulder his trunk, and deposit it directly on the first floor. Or he may go down the area stairs, pass through the kitchen, up the kitchen stairs, up the first flight, up the second flight, and down again to the first story. The end result is the same; and he does the same amount of work in both cases, so far as conveying the weight to a given height is concerned; because in going down-stairs he has actually allowed work to be done on him, by the descent of the weight.

Now, the liquid in expanding to gas begins at a definite volume; it evaporates gradually to gas without altering pressure, heat being, of course, communicated to it during the change, else it would cool itself; and it finally ends as gas. It increases its volume by a definite amount at a definite pressure, and so does a definite amount of work; this work might be utilized in driving an engine.

But if it pass continuously from liquid to gas, the starting-point and the end point are both the same as before. An equal amount of work has been done. But it has been done by going down the area stair, as it were, and over the round I described before.

It is clear that a less amount of work has been done on the left-hand side of the figure than was done before; and a greater amount on the right-hand side; and if I have made my meaning clear, you will see that as much less has been done on the one side as more has been done on the other—that is, that the area of the figure BEH must be equal to that of the figure AFH. Dr. Young and I have tried this experimentally—that is, by measuring the calculated areas; and we found them to be equal.

This can be shown to you easily by a simple device—namely, taking them out and weighing them. As this diagram is an exact representation of the results of our experiments with ether, the device can be put in practice. We can detach these areas which are cut out in tin, and place one in each of this pair of scales, and they balance. The fact that a number of areas thus measured gave the theoretical results of itself furnishes a strong support of the justice of the conclusions we drew as regards the forms of these curves.

To attempt to explain the reasons of this behaviour would take more time than can be given to-night; moreover, to tell the truth, we do not know them. But we have at least partial knowledge; and we may hope that investigations at present being carried out by Prof. Tait may give us a clear idea of the nature of the matter, and of the forces which act on it, and with which it acts, during the continuous change from gas to liquid.

EXPERIMENTAL RESEARCHES ON MECHANICAL FLIGHT.

THE following is a translation of a communication made by Prof. S. P. Langley to the Paris Academy of Sciences on July 13:—

I have been carrying out some researches intimately connected with the subject of mechanical flight, the results of which appear to me to be worthy of attention. They will be published shortly in detail in a memoir. Meanwhile I wish to state the principal conclusions arrived at.

In this memoir I do not pretend to develop an art of mechanical flight; but I demonstrate that, with motors having the same weights as those actually constructed, we possess at present the necessary force for sustaining, with very rapid motion, heavy bodies in the air; for example, inclined planes more than a thousand times denser than the medium in which they move.

Further, from the point of view of these experiments and also of the theory underlying them, it appears to be demonstrated that if, in an aërial movement, we have a plane of determined dimensions and weight, inclined at such angles and moving with such velocities that it is always exactly sustained in horizontal flight, the more the velocity is augmented the greater is the force necessary to diminish the sustaining power. It follows that there will be increasing economy of force for each augmentation of velocity, up to a certain limit which the experiments have not yet determined. This assertion, which I make here with the brevity necessary in this *résumé*, calls for a more ample demonstration, and receives it in the memoir that I have mentioned.

The experiments which I have made during the last four years have been executed with an apparatus having revolving arms about 20 metres in diameter, put in movement by a 10 horse-power steam-engine. They are chiefly as follows:—

(1) To compare the movements of planes or systems of planes, the weights, surface, form, and variable arrangements, the whole being always in a horizontal position, but disposed in such a manner that it could fall freely.

(2) To determine the work necessary to move such planes or systems of planes, when they are inclined, and possess velocities sufficient for them to be sustained by the reaction of the air in all the conditions of free horizontal flight.

(3) To examine the motions of aërostats provided with their own motors, and various other analogous questions that I shall not mention here.

As a specific example of the first category of experiments which have been carried out, let us take a horizontal plane, loaded (by its own weight) with 464 grams, having a length 0·914 metre, a width 0·102 metre, a thickness 2 mm., and a density about 1900 times greater than that of the surrounding air, acted on in the direction of its length by a horizontal force, but able to fall freely.

The first line below gives the horizontal velocities in metres per second; the second the time that the body took to fall in air from a constant height of 1·22 metres, the time of fall in a vacuum being 0·50 second.

Horizontal velocities ...	0m.,	5m.,	10m.,	15m.,	20m.
Time taken to fall from a constant height of 1·22 metres	0·53s.,	0·61s.,	0·75s.,	1·05s.,	2·00s.

When the experiment is made under the best conditions it is striking, because, the plane having no inclination, there is no vertical component of apparent pressure to prolong the time of fall; and yet, although the specific gravity is in this more than 1900 times that of the air, and although the body is quite free to fall, it descends very slowly, as if its weight were diminished a great number of times. What is more, the increase in the time of fall is even greater than the acceleration of the lateral movement.

The same plane, under the same conditions, except that it was moved in the direction of its length, gave analogous but much more marked results; and some observations of the same kind have been made in numerous experiments with other planes, and under more varied conditions.

From that which precedes, the general conclusion may be deduced that the time of fall of a given body in air, whatever may be its weight, may be indefinitely prolonged by lateral motion, and this result indicates the account that ought to be taken of the inertia of air, in aërial locomotion, a property which, if it has not been neglected in this case, has certainly not received up to the present the attention that is due to it. By this (and also in consequence of that which follows) we have established the necessity of examining more attentively the practical possibility of an art very admissible in theory—that of causing heavy and conveniently disposed bodies to slide or, if I may say so, to travel in air.

In order to indicate by another specific example the nature of the data obtained in the second category of my experiments, I will cite the results found with the same plane, but carrying a weight of 500 grams, that is 5380 grams per square metre, inclined at different angles, and moving in the direction of its length. It is entirely free to rise under the pressure of the air, as in the first example it was free to fall; but when it has left its support, the velocity is regulated in such a manner that it will always be subjected to a horizontal motion.

The first column of the following table gives the angle (a) with the horizon; the second the corresponding velocity (V) of *planement*—that is, the velocity which is exactly sufficient to sustain the plane in horizontal movement, when the reaction of the air causes it to rise from its support; the third column indicates in grams the resistances to the movement forward for the corresponding velocities—a resistance that is shown by a dynamometer. These three columns only contain the data of the same experiment. The fourth column shows the product of the values indicated in the second and third—that is to say, the work T, in kilogram-metres per second, which has overcome the resistance. Finally, the fifth column, P, designates the weight in kilograms of a system of such planes that a 1 horse-power engine ought to cause to advance horizontally with the velocity V and at the angle of inclination a.

	V	R	$T = \frac{VR}{1000}$	$P = \frac{500 \times 4554}{T \times 60 \times 1000}$
45 ...	11·2 ...	500 ...	5·6	6·8
30 ...	10·6 ...	275 ...	2·9	13·0
15 ...	11·2 ...	128 ...	1·4	26·5
10 ...	12·4 ...	88 ...	1·1	34·8
5 ...	15·2 ...	45 ...	0·7	55·5
2 ...	20·0 ...	20 ...	0·4	95·0

As to the values given in the last column, it is necessary to add that my experiments demonstrate that, in rapid flight, one may suppose such planes to have very small interstices, without diminishing sensibly the power of support of any of them.

It is also necessary to remark that the considerable weights given here to the planes have only the object of facilitating the quantitative experiments. I have found that surfaces approximately plane, and weighing ten times less, are sufficiently strong to be employed in flight, such as has been actually obtained, so that in the last case more than 85 kilograms are disposable for motors and other accessories. As a matter of fact, complete motors weighing less than five kilograms per horse-power have recently been constructed.

Although I have made use of planes for my quantitative experiments, I do not regard this form of surface as that which gives the best results. I think, therefore, that the weights I have given in the last column may be considered as less than those that could be transported with the corresponding velocities, if in free flight one is able to guide the movement in such a manner as to assure horizontal locomotion—an essential condition to the economical employment of the power at our disposal.

The execution of these conditions, as of those that impose the practical necessity of ascending and descending with safety, belongs more to the art of which I have spoken than to my subject.

The points that I have endeavoured to demonstrate in the memoir in question are:—

(1) That the force requisite to sustain inclined planes in horizontal aërial locomotion diminishes, instead of increasing, when the velocity is augmented; and that up to very high velocities—a proposition the complete experimental demonstration of which will be given in my memoir; but I hope that its apparent improbability

will be diminished by the examination of the preceding examples.

(2) That the work necessary to sustain in high velocity the weights of an apparatus composed of planes and a motor may be produced by motors so light as those that have actually been constructed, provided that care is taken to conveniently direct the apparatus in free flight; with other conclusions of an analogous character.

I hope soon to have the honour of submitting a more complete account of the experiments to the Academy.

ON THE SOLID AND LIQUID PARTICLES IN CLOUDS.[1]

IN this paper are given the results of some observations made while on the Rigi in May last, on the solid and liquid particles in clouds. It was noticed, when making observations on the number of dust particles in the atmosphere, that when the top of the mountain was in cloud, the number of particles varied greatly in short intervals; while previous experience had shown that at elevated stations the number was fairly constant for long periods. In order to investigate the case of this want of uniformity in the impurity of clouded air, extreme conditions were selected, and the air tested in cloud and in the clear air outside of it. When this was done the clouded air was found to have always more dust in it than the air outside. Its humidity was of course also greater. The relative amount of dust in pure and in clouded air varied greatly. Some parts of the cloud had only about double the number of particles there were in the clear air, while in other parts the proportion was much greater. The best example tested occurred on the 25th of the month, when there were observed 700 particles per c.c. in the clear air, while the number in cloud went up to over 3000, and in one cloud to 4200 particles per c.c. These observations were taken on the top of the mountain while the clouds were passing over it; the readings being taken in the cloud and again when it had passed and was replaced by clear air.

These observations at once showed the cause of the variability in the number of dust-particles in the clouds. The dust acted as a kind of ear-mark, and showed that the air forming the clouds was impure valley air, which had forced its way up into the purer air above. This impure air had become more or less mixed with the purer upper air. Where little of the impure air had mixed with the upper air, the number of particles was not large, and the clouding slight; but where the valley air was greatly in excess, the number of particles was great, and the clouding dense. It should be noted here that all the clouds tested were cumulus. It is quite probable that the conditions in stratus and other clouds may be different.

During this visit to the Rigi there were a number of opportunities of investigating the water particles in clouds. The apparatus used was the small instrument described to the Society in May last. With this instrument the water particles in clouds can be easily seen, and the number falling on a given area counted. The results are similar to those already communicated to the Society from observations made in fogs during last winter. On observing with this instrument in clouds, the water particles were distinctly seen showering down, and the number falling on the micrometer easily counted. The number of drops falling was observed to vary greatly from time to time. At times so quickly did they fall that it was impossible to count the number that fell on only 1 sq. mm. The greatest rate actually counted was 60 drops per sq. mm. in 30 seconds, but for a few seconds the rate was much quicker. Though the quick falls seldom lasted long, yet 30 drops per sq. mm. per minute were frequently observed for a considerable time. The maximum rate of 60 per sq. mm. per half minute gives 12,000 drops per square centimetre per minute, or 77,400 drops per square inch per minute. This does seem to be an enormous number of drops to fall on so small an area in the time. These drops, however, are so extremely small they rapidly evaporate, more than two or three being seldom visible at the same time on one square of the micrometer. The denser the cloud the quicker was the rate of fall, and as the cloud thinned away the drops fell at longer intervals, and they diminished in size at the same time.

It was frequently observed when the mountain-top was in clouds, particularly if they were not very dense overhead, that the surfaces of all exposed objects were quite dry; not only the stones on the ground, which might have received heat from the earth, but also wooden seats, posts, &c., were all perfectly dry, and if wetted they soon dried. While everything was dry, the fog-counter showed that fine rain-drops were falling in immense numbers. From the fact that the air was packed full of these small drops of water, it might have been assumed that the air was saturated, and tests with properly-protected wet and dry bulb thermometers showed that it was saturated. A few observations were therefore made to explain this apparent contradiction of surfaces remaining dry while exposed to a continued shower of fine rain and surrounded by saturated air. The explanation was found to be, simply, radiant heat. Though the cloud may be so dense, it is impossible to see the sun or even a preponderance of light in one direction to indicate its position; yet, as a good deal of light penetrates under these conditions, it therefore seemed possible some heat might do so also. A thermometer with black bulb *in vacuo* showed that a considerable amount of heat penetrated the clouds under the conditions, as it rose 40° to 50° above the temperature of the air while the observations were being made. This radiant heat is absorbed by all exposed surfaces and heats them, while they in turn heat the air in contact with them, and the fine drops of water are either evaporated in this hot layer of air or after they come in contact with the heated surfaces. Other observations made on Pilatus pointed to the same conclusion. All large objects, such as seats, posts, &c., were quite dry in cloud when there was any radiation; while small objects, such as pins, fine threads, &c., were covered with beads of water. The large surfaces being more heated by radiation than small ones, when surrounded by air, these surfaces evaporate the drops falling on them, while the small ones, being kept cool by the passing air, are unable to keep themselves dry.

The observations made with the fog-counter point to the conclusion that the density or thickness of a cloud depends more on the number of water particles than on the number of dust particles in it. The number of the dust particles in the clouds varied too much and too quickly to enable any conclusion to be drawn from observations made in clouds themselves. However, on comparing the thickness of a cloud on the Rigi and a fog at low level, when the number of water-drops was about the same, it is found that the fog, though thicker, was not greatly so, although there were only a few thousand dust-particles per c.c. in the cloud, while there were about 50,000 in the fog.

The observations with the fog-counter show that, whenever a cloud is formed, it at once begins to rain, and the small drops fall into the drier air underneath, where they are evaporated, the distance to which they will fall depending on their size and the dryness of the air. It is thought that much of the dissolving of clouds is brought about in this way.

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[1] Abstract of Paper read before the Royal Society, Edinburgh, on July 6, by John Aitken, F.R.S. Communicated by permission of the Council of the Society.

OLD STANDARDS.

BY a curious accident it has just been discovered that the standard yard and certain other measures and weights which were supposed to have been lost when the Houses of Parliament were destroyed by fire in 1834 are still in existence. The following account of the matter is condensed from a statement in the *Times*. A reference to the contemporary records shows that after the fire the standard bars of 1758 and 1760 were both found among the ruins, "but they were too much injured to indicate the measure of a yard which had been marked upon them." The principal injury to both of the standards was the loss of the left-hand gold stud, but whether this was caused by the action of the flames or otherwise is not known. When the Palace of Westminster was rebuilt the two bars were deposited in the Journal Office, and from that time, until the other day, they seem to have been wholly lost sight of. About a fortnight ago it happened to be stated in the lobby that one of the duties of the Speaker was to inspect once in every twenty years the standards immured in the sill of the Lower Waiting Hall. Inquiries at the Standards Department of the Board of Trade elicited the fact that, so far from any statutory requirement being imposed upon the Speaker in the direction indicated, Section 35 of the Weights and Measures Act, 1878, which provides for the care and restoration of the Parliamentary copies of the Imperial standards, specially exempts the walled-up copy from periodical inspection and comparison. It was found, however, that in 1871 Speaker Denison took cognizance of the standards; and this fact was brought to the Speaker's notice. While inquiries were being made as to Speaker Denison's inspection, an official in the Journal Office mentioned that when the contents of that office were recently being transferred to the new wing he had observed among the lumber some old weights and measures. These proved to be the missing standards. On Tuesday last they were examined by Mr. Chaney, the Superintendent of Weights and Measures; and on Wednesday the Speaker was to visit the Journal Office for the purpose of inspecting them.

The most important of the standards thus rescued from oblivion are the yard measures constructed by Bird in 1758 and 1760. The former was copied from a bar in the possession of the Royal Society, which was itself a copy of a standard preserved in the Tower; and the second was constructed under the directions of a Committee of the House of Commons from the 1758 standard. "Each of these two standard yards consisted of a solid brass bar 1·05 in. square in section and 39·73 in. long. Near each end of the upper surface gold pins or studs 0·1 in. in diameter were inserted, and points or dots were marked upon the gold to determine the length of the yard." The other standards in the custody of the Journal Office are two brass rods answering the description of the old Exchequer yard, and four weights supposed to be certain of the "copies, model, patterns, and multiples" ordered by the House on May 21, 1760, "to be locked up by the clerk and kept by him." The most important weight—the standard troy pound—is not amongst those now brought to light.

NOTES.

AT some little distance to the north and north-east of Cardiff lies a beautiful piece of hilly country, much frequented by pedestrians, and known as the Black Mountain or Black Forest district. It has not been found practicable by the Local Committee to arrange an official excursion to this district on the occasion of the visit of the British Association to Cardiff; but a project is now being unofficially forwarded for conducting small parties of not exceeding six visitors each to some of the choicest parts of this country, at a time so arranged as not to interfere with the sittings of the various Sections. Several local gentlemen, thoroughly familiar with the district, have offered to act as guides, and with fair weather most enjoyable excursions are to be anticipated. The country being essentially one for pedestrians, the excursions would take the form of an afternoon walk of from eight to twelve miles, with a further walk on the following day of from twenty-five to thirty miles. Any member of the British Association desirous of taking part in one of these excursions can obtain full particulars by applying to the Local Secretaries, 9 Bank Buildings, Cardiff, who will forward the applications to the promoters.

THE annual meeting of the French Association for the Advancement of the Sciences will be held at Marseilles, commencing on September 17. The special subject chosen for discussion in the Botanical Section is the best mode of arrangement and exhibition for different kinds of botanical collections, with the double purpose of the preservation of the specimens and the facilitating of study.

THE Technical and Recreative Institute established by the Goldsmiths' Company at New Cross was opened by the Prince of Wales on Wednesday. In addition to this Institute there are to be two Polytechnics south of the Thames, one in Battersea Park Road, the other in the Borough Road. The memorial stone of the one in Battersea was laid by the Prince of Wales on Monday.

PROF. M. W. HARRINGTON, the founder of the *American Meteorological Journal*, has been appointed Chief of the United States Weather Bureau, under the Department of Agriculture in Washington. Prof. Harrington was born in Illinois in 1848, and graduated at Michigan in 1868. In 1879 he was made Professor of Astronomy and Director of the Astronomical Observatory at Ann Arbor, Michigan. From a recent article by him, entitled "How could the Weather Service best promote Agriculture?" it appears likely that the energies of the new service will be devoted more to the interests of agriculture than to commerce, and that an attempt will be made to issue special weather predictions for the farmer, by means of the multiplication of local forecasting stations. There can be little doubt—seeing the large amount of funds under his control—that he will also still further advance the important work of international meteorology which has been so ably conducted by his predecessors.

THE half-yearly general meeting of the Scottish Meteorological Society was held in Edinburgh on Wednesday. The report from the Council of the Society was presented; and papers were read on certain relations of wind, pressure, and temperature at the Ben Nevis Observatories, by Dr. Buchan, and on influenza and weather of London in 1891, by Sir Arthur Mitchell and Dr. Buchan.

FROM the official record of the work done in the British Museum during 1890 it seems that there has been a serious decrease in the number of visitors. Special departments, however, have been used more than ever by students; and it is satisfactory to find that the zoological and geological collections in the Natural History Museum are being more generally appreciated.

GERMAN scientific papers record the death, on June 18, of Dr. Otto Tischler, well known as an archæologist of wide learning and sound judgment. He especially distinguished himself by his investigation of the burial-mounds of East Prussia. Dr. Tischler was forty-eight years of age.

PROF. A. RICCÒ, Director of the Catania Observatory, who has just returned from a visit to the volcano Stromboli, sends us the following notice of a recent eruption:—"On June 24, 45 minutes after noon (Rome mean time), the inhabitants of the Æolian Isles were alarmed by two strong shocks of earthquake, followed by two tremendous explosions of the volcano, which sent forth from four mouths a great quantity of smoke, cinders, incandescent blocks, and currents of lava that descended the mountain slopes to the sea. The sea, at the points where the lava entered it, steamed up, producing great noisy masses of vapour. The phenomena continued till July 1. Stromboli has now returned to its habitual state of moderate activity."

THE annual meeting of the Society for the Preservation of the Monuments of Ancient Egypt was held last week in the rooms of the Society of Antiquaries at Burlington House. Lord Wharncliffe, President, occupied the chair. The report stated that there was little to report of success attending the proceedings of the Society for the past year. Its energies had been directed principally to two points—the necessity for an official inspector or superintendent in Egypt, whose duty should be the care of the ancient monuments, and an endeavour to do something towards arresting the gradual destruction of the Great Temple at Karnak. Reports concerning a proposed scheme for barring the Nile below Philæ, to make a vast reservoir for purposes of irrigation, had appeared in the public papers from time to time, and recently various more definite communications had been received by the committee on the same subject. The result would be, it was acknowledged, to completely cover this beautiful island and temple with water. There had been some correspondence on this subject with the authorities in Egypt; but as nothing had as yet been decided as to any scheme of irrigation, and as a committee would be appointed to consider the whole question, it might be considered as suspended for the present, and the committee had thought it best to wait before taking any further action; but they would not lose sight of this important matter, and would oppose to the utmost of their power any engineering scheme which would involve injury or destruction to this world-renowned spot. General Donnelly moved the adoption of the report; and the motion was seconded by Sir Edmund Henderson, and agreed to. The committee for the coming year was then elected, and a discussion subsequently took place as to the proposed scheme for barring the Nile below Philæ, the opinion of the meeting being evidently strongly opposed to the adoption of any system of irrigation which should involve damage to the temple. Mr. J. Bryce, M.P., spoke of the wanton injury which was often inflicted on monuments in Egypt, and said that he thought it would be necessary, in dealing with that matter, to bring the question of jurisdiction to the attention of those from whom any system of inspection or care was to emanate. We may note that in answer to a question put by Mr. Bryce in the House of Commons on July 15, Sir J. Fergusson said that nothing definite had been settled as to the preservation of ancient monuments in Egypt; £E10,000 had been allotted in the Budget for the current year.

THE Pilot Chart of the North Atlantic Ocean for July contains a special account of a hurricane that moved along a track almost due north, about 500 miles east of Newfoundland on June 9 and 10, together with a chart of the conditions of barometer and wind between Newfoundland and Ireland, showing that the abnormal track was due to the approach of an anticyclone west of the British Isles. A supplement issued with the Pilot Chart illustrates the drift of every bottle paper returned to the United States Hydrographic Office since April 1889. There are 113 papers that contain the date of commencement and end of journey; the average number of miles that each bottle drifted is 869, and the average daily drift is 5·8 miles. This figure is rather below the true average rate per day, as any time the bottle lay upon the shore before discovery added to its time of drift.

M. PATOUILLARD has just returned from a scientific mission with which he was intrusted by the Minister of Public Instruction in France, an investigation of the mycological flora of Tunis, Carthage, and the adjacent regions.

IN one of the principal articles of the *Meteorologische Zeitschrift* for May, Herr R. Börnstein discusses the question of a connection between air-pressure and the hour angle of the moon, using as a basis the hourly observations of four German and Austrian stations. This investigation differs from the usual mode of treatment, as it takes no account of the moon's phases, or of its declination or distance from the earth, but only of the lunar day, and deals solely with atmospheric pressure. The results arrived at are: (1) that the existence of atmospheric tides is not plainly recognizable in the range of pressure; (2) at three of the stations the pressure exhibits a single oscillation during the lunar day. The maximum occurs at Hamburg and Berlin shortly before the setting of the moon, and at Vienna about the time of the lower culmination, while the minimum occurs at all stations near the time of the moon's rising.

WE have received vol. viii. of the *Anales de la Oficina Meteorologica Argentina*. It contains a summation of the records obtained at five different stations in the Republic during the years 1877–89. The organization of the Department appears to be now very complete, there being no less than twenty-eight stations fully equipped with ordinary and self-registering instruments.

REFERRING to a statement which has been publicly made, that the adoption of electric lighting in place of gas at the office of the Savings Bank Department of the General Post Office has been followed by a marked reduction in the amount of sick leave, the *Lancet* says it has good authority for believing that the statement in question is substantially correct. Although the time which has as yet elapsed—two years—since the introduction of the new illuminant has been insufficient for the collection of trustworthy statistics, our contemporary thinks there is every reason to believe that electric lighting will prove to be much more wholesome than ordinary gas flames. An electric lamp does not compete for the oxygen of the apartment in which it is placed, and this circumstance gives it a marked advantage over any open flame. It cannot, like some forms of gas-burner, be used to promote ventilation; but in ordinary situations its harmlessness is a much more important property.

METEOROLOGICAL observatories are generally ill adapted, by reason of dust and smoke, for observations on atmospheric electricity; and, with the view of inciting private individuals to such work, Herren Elster and Geitel, of Wolfenbüttel, have lately issued a *brochure* in which they indicate the ends to be sought and the instrumental means. Three things demand attention: first, systematic observation and measurement of electricity in the open air at different times in the day and in the seasons, humidity and air-temperature being determined at the same time; second, measurement of the fall of potential with a clear sky; and third, measurement of the fall of potential and its change of sign during rain, &c. The instruments and methods recommended are such as present little difficulty for private persons.

THE American National Geographic Society prints in the current number of its magazine a full and interesting account, by Israel C. Russell, of an expedition to Mount St. Elias, Alaska. The paper is illustrated by various excellent maps and diagrams.

THE Winchester College Natural History Society has just issued, under the title of "Geological Notes" (J. Wells, Winchester), a list of all the fossils as yet known from the chalk in the anticlinal of Winchester. The exact localities and zones are given; and, since the names appear not only to have been carefully determined, but to be well up to date, this very modest pamphlet will prove as useful a guide to the collector as it is valuable to the stratigraphical geologist.

MESSRS. WOODALL, MINSHALL, AND CO., Oswestry, have issued "A Flora of Oswestry and District," by T. P. Diamond, Honorary Secretary of the Offa Field Club. It contains a list of plants in the neighbourhood of Oswestry, arranged according to their natural orders; and at the end there is an index, in which both the English and the Welsh names of the plants are given. Mr. Diamond calls attention to the fact that his "list of over 700 plants includes representatives of 90 out of the 101 natural orders in the flora of the United Kingdom."

THE United States Department of Agriculture is printing—in the series entitled "Contributions from the United States National Herbarium"—what promises to be a valuable manual of the plants of Western Texas, by John M. Coulter. This district is described as "one of the richest regions in plant display, containing a flora particularly interesting on account of the intermingling of Mexican species." The manual is being published in parts because the author hopes that their successive appearance may call forth additional information that may be embodied in a final supplement.

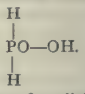
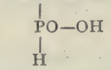
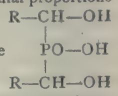
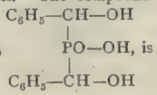
A SHEET dealing with the potato disease will shortly be issued by the Royal Agricultural Society of England. It was originally published by the Irish Land Commission, by whose permission it is being reproduced. In the text, by Mr. William Carruthers, F.R.S., all necessary information is given, and this is accompanied by coloured drawings illustrating various phases of the potato disease.

IN the July number of the *London and Middlesex Note-Book*, Mr. G. F. Lawrence says he recently obtained a drift implement of unusual form from the site of Mr. Peter Robinson's new premises in Oxford Street. The peculiarity consists of the curious curvature of one face of the implement compared with the flatness of the other side. He does not know of another like it, but suggests that, as attention is called to what may be a mere variation of an ordinary type, examples may be found in other collections. This specimen is of a somewhat ochreous colour, is lustrous and but slightly abraded or rolled, and it measures 5⅝ inches long by 3 inches wide. The occurrence of drift-implements in Central London is rather unusual. Mr. Lawrence thinks twelve would be rather over than under the number known.

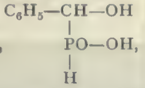
IN the current number of the Scientific Proceedings of the Royal Dublin Society (vol. vii. Part 2) Mr. E. W. L. Holt publishes a preliminary note on the fish obtained during the cruise of the s.s. *Fingal*, 1890, on the Society's survey of fishing grounds on the west coast of Ireland. Amongst the shore fishes, *Aphia pellucida*, Nardo, and *Crystallogobius nilssonii*, Düb. and Kor., are for the first time recorded from Irish waters. The second British specimen of *Arnoglossus grohmanni*, Bonap., is also recorded. From depths between 100 and 500 fathoms off Achill Head, *Pomatomus telescopium*, Risso, *Mora mediterranea*, Risso, and *Macrurus æqualis*, Gthr., are added to the British fauna; and a description is given of a new deep-sea eel, intermediate between Saurenchelys and Nettastoma, which has been named *Nettophichthys retropinnatus*, n. g. et sp. *Gadus esmarkii*, Nilsson, and *Macrurus rupestris*, Gunner, are added to the Irish fauna from similar depths, and *Argentina sphyræna*, Linn., from 52 to 80 fathoms. Amongst other fish recorded from depths exceeding 100 fathoms are *Chimæra monstrosa*, Linn., *Trigla lyra*, Linn., *Gadus argenteus*, Guich., *Phycis blennioides*, Brünn., *Haloporphyrus eques*, Gthr., *Macrurus cœlorhynchus*, Risso, *M. lævis*, Lowe, &c. A young Phycis is also recorded from 26 fathoms, and mention is made of the occurrence at the surface of a shoal of young *Gadus poutassou*, Risso, 34 miles from land.

HARDNESS is one of the most important properties of solid bodies; yet the measurement of it has not been very satisfactorily effected hitherto. Prof. Auerbach, of Jena, has recently described (*Repertorium für Physik*) an apparatus for the purpose, designed for transparent bodies. In it the spherical surface of a lens is pressed up by the short arm of a weighted lever against a small thick plate, on which the observer looks down through a microscope furnished with a micrometer, watching the effects of increasing pressure. Glass and rock crystal were observed. The author offers a theory of the subject, and tests it. A comparison of hardnesses with moduli of elasticity shows that, while the more elastic of those substances were also the harder, the hardness increases less than the elasticity.

FROM recent accounts it appears that the consumption of gas in Paris in 1890 exceeded that in 1880 by 26·2 per cent., while the number of consumers increased 56·8 per cent. The amount per consumer diminished 19·5 per cent., from 1642 to 1322 cubic metres. Electricity has evidently withdrawn many large consumers of gas. The same account states that in *three* years the number of arc and glow lamps has increased 140 and 170 per cent. respectively. The consumption of petroleum in France has increased 47 per cent in those ten years, while that of gas, in the whole of France, has grown 62 per cent.

A SERIES of addition compounds of aldehydes with hypophosphorous acid are described by M. Ville in the current number of the *Annales de Chimie et de Physique*. As is well known, aldehydes exhibit the characteristic property of uniting directly with many other substances, such as ammonia, hydrocyanic acid, acid sulphites, and hydroxylamine. Some time ago, it was shown by Fossek that trichloride of phosphorus was likewise capable of uniting directly with many aldehydes with production of liquid compounds decomposable by water. M. Ville now shows that a similar series of additive compounds are formed with hypophosphorous acid, and these compounds are of considerable importance as throwing more light upon the nature of this lower acid of phosphorus. Hypophosphorous acid, H_3PO_2, the acid derived from the as yet unisolated oxide P_2O, may be regarded as possessing the structure  $\begin{array}{l}\text{H}\\ |\\ \text{PO—OH}\\ |\\ \text{H}\end{array}$. By the direct action of aldehydes under the influence of a slight rise of temperature, two distinct classes of new compounds are obtained. When the aldehyde and hypophosphorous acid are allowed to react in the proportion of equal molecules, compounds of the type  $\begin{array}{l}\text{R—CH—OH}\\ \quad\;\; |\\ \quad\;\; \text{PO—OH}\\ \quad\;\; |\\ \quad\;\; \text{H}\end{array}$ are obtained, where R may represent the radicle of any aldehyde. If, however, two molecular proportions of aldehyde are employed, compounds of the type  $\begin{array}{l}\text{R—CH—OH}\\ \quad\;\; |\\ \quad\;\; \text{PO—OH}\\ \quad\;\; |\\ \text{R—CH—OH}\end{array}$ are formed. The aldehydes of the aromatic series lend themselves best to the formation of these compounds, those of the fatty series exhibiting a great tendency to the production of condensation products. The compound of the second type with benzoic aldehyde,  $\begin{array}{l}C_6H_5\text{—CH—OH}\\ \qquad\quad |\\ \qquad\quad \text{PO—OH}\\ \qquad\quad |\\ C_6H_5\text{—CH—OH}\end{array}$, is obtained by digesting together for several hours upon a water-bath benzaldehyde and hypophosphorous acid in an atmosphere of carbon dioxide. Crystals of the new compound soon commence to separate, and rapidly permeate the whole liquid. On draining and washing, they are found to consist of colourless radiating groups of lamellæ. They are not very soluble in water, but dissolve more readily in organic solvents, best in methyl alcohol. The aqueous solution is strongly acid, decomposing carbonates readily, and forming crystalline salts with bases. Curiously, though, it exerts no reducing action upon solutions of copper sulphate or silver salts.

In order to obtain the acid of the first type,



$$\begin{array}{rcl} C_6H_5— & CH & —OH \\ & | & \\ & PO & —OH, \\ & | & \\ & H & \end{array}$$

it is best to employ an excess of hypophosphorous acid. In this case, instead of crystals of the acid of the second type separating, the whole forms a homogeneous liquid which remains unprecipitated by water. It contains the acid of the first type, and this latter is best isolated by precipitating the lead salt by the addition of lead acetate and decomposing the salt, suspended in water, by means of sulphuretted hydrogen. On concentration of the filtered solution, a syrup is obtained which eventually yields deliquescent crystals of the pure acid. The solution of this acid does not reduce copper sulphate, but readily precipitates metallic silver from silver nitrate. Many similar compounds with other aldehydes have also been prepared, and found to present analogous properties more or less modified by the specific nature of the particular aldehyde employed.

THE additions to the Zoological Society's Gardens during the past week include two Ruddy-headed Geese (*Bernicla rubidiceps* ♂ ♂) from the Falkland Islands, presented by Mr. F. E. Blaauw, C.M.Z.S.; a Smooth Snake (*Coronella lævis*), British, presented by Mr. W. H. B. Pain; two great Eagle Owls (*Bubo maximus*), European, deposited; six Eyed Lizards (*Lacerta ocellata*), two Four-lined Snakes (*Coluber quadrilineatus*), a Back-marked Snake (*Rhinechis scalaris*), South European, purchased; a Burrhel Wild Sheep (*Ovis burrhel* ♂), a Japanese Deer (*Cervus sika* ♀), a Bennett's Wallaby (*Halmaturus bennetti* ♀), two Night Herons (*Nycticorax griseus*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

A CAUSE OF LUNAR LIBRATION.—A paper by Mr. S. E. Peal, "On a Possible Cause for Lunar Libration other than an Ellipsoidal Figure, and on Lunar Snow Mountains," has recently been published by Messrs. Dulau and Co. It is shown that evidence of several kinds points to the existence of a vast shoal, or submerged continent, some 1500 miles long by 400 across along the prime meridian. This is presumed to be of greater specific gravity than the refrigerated *maria* east and west of it, and to have been at one time situated in the southern hemisphere. The difference of attraction upon the shoal and the surrounding *maria* is shown to be sufficient both to cause and maintain libration. Since libration began, the shoal has placed itself geocentrically, in which case the south pole must have been drawn forward about 30°. The possibilities of the case seem to be as follows. The moon formerly had a physical constitution the same as that of the earth at the present time. The lunar ocean beds were then steadily subsiding, the lines of upheaval and weakness being on the continents, and causing a series of *quasi*-volcanic orifices. Whilst tidal friction was reducing the velocity of rotation, polar snow-caps were formed, and the atmosphere became rarer. The extension of the snow-cap to the equator was for ages prevented by the incidence of solar heat. This struggle between steadily-increasing refrigeration and solar heat should therefore be evidenced by the existence of an irregular belt about the (then) equator. Such a belt is found in the circular *maria* Smythii, Crisium, Serenitatis Imbrium, and part of Oceanus Procellarum. If the axis of rotation be shifted about 30°, so that the south pole occurs near Nach or Maginus, all these irregular *maria* form a chain of seas along the equator, which may represent the belt of solar influence referred to. Eventually these *maria* were refrigerated, and the meridional shoal, acting as a fixed tide during libration, caused the change in the direction of the axis of rotation, which shifted the belt of seas from their equatorial position to that at present occupied by them.

DOUBLE-STAR OBSERVATIONS.—In *Astronomische Nachrichten*, Nos. 3047 and 3048, Mr. S. W. Burnham gives the results of his double-star observations made in 1890 with the 36-inch equatorial of the Lick Observatory. The stars which have been re-observed are mainly those which cannot easily be measured on account of their being beyond the reach of any but the most powerful telescopes. Mr. Burnham also notes that his purpose has not been to find as many pairs as possible without reference to their character, but to make several measurements of interesting ones. The present catalogue of new stars contains 70 pairs, of which 39 have distances less than 1″, with an average distance of 0″·45.

The following naked-eye stars are included in the list of new binaries:—B.A.C. 230, 48 Cephei (H), 5 Camelopardus, τ Herculis, Ceti 199, 34 Persei, ν Geminorum, 24 Aquarii, 95 Piscium, B.A.C. 1142, 36 Geminorum, ψ^1 Aquarii, χ Persei, Tauri 148, 65 Geminorum; and the following pairs, previously known, have been found to be more closely double:—H 1981, S 409, Σ 809, OΣ (app.) 77, Σ 2476, OΣ 425, Σ 12 (app. II.).

OBSERVATIONS OF THE ZODIACAL COUNTER-GLOW.—An account of observations of the zodiacal counterglow, or *Gegenschein*, made at Mount Hamilton from 1888 to 1891, is contributed to the *Astronomical Journal*, No. 243, by Mr. E. E. Barnard. The changes of form previously noted have been confirmed. In the fall of the year the *Gegenschein* appears large and roundish. It afterwards becomes elongated, and connected with the zodiacal light by a narrow zodiacal band. The observations prove that the *Gegenschein* does not lie in the ecliptic, although very nearly so. Neither is it exactly 180° from the sun. The mean of sixteen observations assign the following longitude and latitude to the phenomenon:—

$$\odot - \lambda = 180^{\circ}\cdot 6\,;\ \beta = +\,1^{\circ}\cdot 3.$$

THE OBSERVATORY OF YALE UNIVERSITY.—The Report for the year 1890–91 of the Observatory of Yale University contains a report from Dr. Elkin, from which we make the following extracts:—

"In observational work with the heliometer I have been engaged almost wholly in the continuation of the series on the parallaxes of the first magnitude stars in the northern hemisphere. The scheme originally laid out has now been completed, and furnishes for each of the ten stars three (for Arcturus five) independent results.

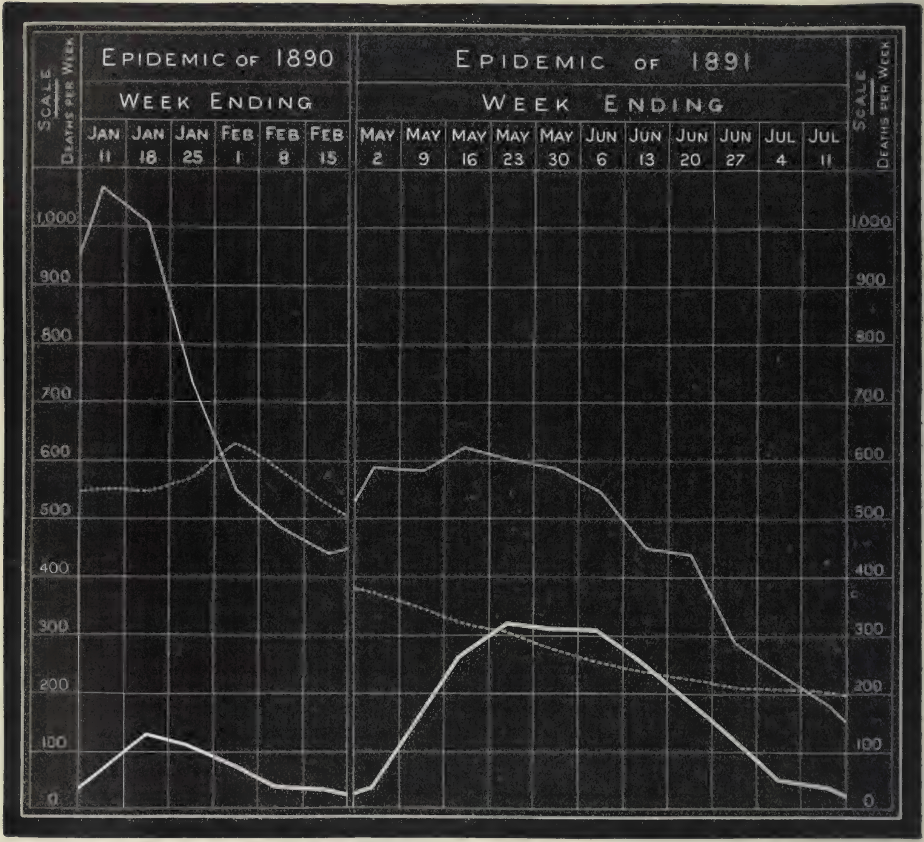
"The triangulation of the comparison stars for Victoria according to the plan drawn up by Dr. Gill has been carried out by Mr. F. L. Chase, who secured some 450 measures of these stars during the months of June to October 1890. Mr. Chase has also reduced the observations as far as it was advisable for us to do so here, and the results have been communicated to Dr. Gill, along with the reduced results of our observations of Victoria and Sappho in 1889. Since February 1891, Mr. Chase has been engaged in a triangulation of the principal stars in *Coma Berenices*, and up to date about one-half of the proposed measures have been obtained.

"It is proposed during the ensuing season to devote the heliometer to a series of measures on the satellites of Jupiter for the determination of their orbits and the mass of the planet, comparing them *inter se*, as has been done with such success by Hermann Struve at Pulkova with those of Saturn."

THE RECENT EPIDEMIC OF INFLUENZA.

THE mortality in London from influenza shows a steady decline week by week; and, although the number of deaths is still in excess of the average, there are good reasons for hoping that the epidemic will shortly disappear from our midst. The severity of the recent visitation, as compared with that which prevailed last year, is clearly shown by the accompanying diagram, in which the effects of the two epidemics are displayed side by side. The weekly mortality from influenza alone is represented by the thick curve, the number of deaths from diseases of the respiratory system by the lighter curve, and the average number of deaths from disorders of the latter class by a dotted line. The average mortality from influenza is too small to permit of any curve being drawn upon the scale shown in the diagram.

Taking into consideration, firstly, the mortality from influenza alone, we find that not only was the duration of last year's epidemic far less than that of the present year, but that the number of deaths in the earlier period was very much smaller than in the latter. The epidemic of 1890 set in with great severity and suddenness at the beginning of January. During the week ending December 28, 1889, there were no deaths in London from this disease, and in the following week only 4. In the week ending January 11, 1890, however, the number had risen to 67, while in the course of the ensuing seven days a maximum of 127 was reached. The mortality then declined steadily, the numbers in the four succeeding weeks being respectively, 105, 75, 38, and 30. As a serious epidemic the visitation may then be regarded as having spent itself, and in preparing the diagram I have not considered it advisable to include any period in which the weekly number of deaths fell below 25. It may, however, be remarked that, for three weeks subsequent to that shown by the curve, the mortality exceeded 20, while in the four succeeding weeks it ranged between 10 and 17. Taking as a whole the period of six weeks in which the epidemic prevailed with more or less severity, it appears that the total number of deaths in London was 442, giving an average of 74 per week.



The visitation of the present year may be said to have commenced at the beginning of May. During the whole of April there were a few deaths from influenza, the numbers in the four weeks embraced by the Registrar-General's returns being respectively 7, 3, 9, and 10. By the week ending May 2, however, the number had risen to 37, and from this time onward the disease continued to spread with alarming rapidity, the numbers in the three successive weeks being 148, 266, and a maximum during the week ending May 23 of 319. In the two following weeks the mortality again exceeded 300, after which it declined steadily to 249, 182, 117, and 56, while in the last period shown by the diagram the number had fallen to 40. Taking the period as a whole, it appears that during the eleven weeks ending July 11 the total number of deaths in London from influenza, irrespective of cases in which it was known to have been present in the course of other diseases, was 2027, giving an average of 184 per week, or about two and a half times as much as the average mortality in 1890. In no fewer than seven weeks out of the eleven the number of deaths exceeded the maximum attained during the epidemic of last year.

From an examination of the statistics given in a valuable paper read before the Scottish Meteorological Society on March 31, 1890, by Sir Arthur Mitchell and Dr. Buchan (an abstract of which appeared in NATURE, vol. xli. p. 596), it is quite evident that the recent epidemic of influenza has been the most severe we have had in London since the first publication of weekly records of mortality some forty-five years ago. As the figures are of great interest, we make no apology for reproducing the brief table given in that paper, showing the number of deaths which occurred in the five principal epidemics experienced since the year 1847. It will be observed that the number given for last year is considerably in excess of that quoted above, the period selected by the authors of the paper comprising the whole of the three months January to March. In the month last mentioned, the epidemic was certainly not of any great severity, but as the figures do not clash in any way with the general argument, I have not thought it advisable to alter the results. An addition has, however, been made to the table, by including the figures of mortality reached during the epidemic of the present year.

	Deaths.
December 1847 to April 1848	1631
March to May 1851	258
January to March 1855	130
November 1857 to January 1858	123
January to March 1890	545
May to July 1891	2027

It will be seen from the table that the mortality recently experienced has been far greater than at any other period during the forty-five years, the nearest approach to so severe an epidemic being in 1847–48, when the deaths amounted to about 400 less. Taking into consideration the fact that the population of London 45 years ago was very much smaller than it is now, it may at the first blush appear that, as regards severity, there was not very much to choose between the two visitations. It must not be forgotten, however, that in the earlier period the ravages of the disease extended over five months, while in the latter they were confined to about two and a half.

A very striking feature in the disease to which the somewhat misleading name of influenza has been given, is its peculiarly weakening effect upon the lungs and bronchial tubes; and as the epidemic is invariably attended by a high mortality from respiratory diseases, I have included in the diagram a series of curves showing the number of deaths from these attendant disorders. As regards the epidemic of 1890, it may at once be confessed that the curve is somewhat misleading. During the last few days of 1889 and the opening of the following year a sharp touch of anticyclonic cold was experienced over England; and in the metropolis this was accompanied, as is so commonly the case, by thick fog. Under such circumstances a high mortality from respiratory diseases followed as a matter of course, so that when we examine the curve we find that, at the time when the epidemic of influenza was only just appearing, the deaths from lung disorders were at their maximum. After the first week in January, however, the weather became unusually mild for the time of year, a long period of south-westerly winds setting in, with abnormally high temperatures. There can be little doubt, in fact, that at the time the influenza epidemic of 1890 was raging the effects of temperature and weather were so strong as to obliterate the influences of the miasmatic disorder upon diseases of the respiratory system. This year, however, the meteorological element may almost be eliminated from account, for, although cold winds were very frequent in May and the early part of June, the severity of the weather was not such as to lead to any material increase of mortality from the class of diseases in question. The spread of influenza was, however, soon followed by a serious rise in the death-rate, and in the course of the fortnight ending June 6 the mortality from respiratory complaints amounted to more than twice the average, the large excess being due chiefly to deaths from pneumonia and bronchitis. The subsequent decline of influenza was accompanied, as will be seen from the curve, by a corresponding decline of fatalities from respiratory diseases, but it was not until the last week of the period that the deaths fell short of the average. Taking the eleven weeks as a whole, it appears that the total mortality from respiratory disorders amounted to 5138, or about 75 per cent. more than the average. During the epidemic of 1890 the actual number was far larger, but in the winter months the average is also very much higher, and as a matter of fact the excess above the normal only amounted last year to 26 per cent.

The influence of the weather upon the two epidemics seems to have been exerted in entirely opposite directions. During the epidemic of 1890 temperature was, as we have already seen, for the most part very high for the time of year, and the prevalence of a strong current of south-westerly winds in January doubtless aided in the dispersal of the miasmatic germs. The weather was, in fact, as favourable as could have been desired, and the ravages of the epidemic, severe though they were, were doubtless much milder than they would have been had the winter been cold and foggy. The recent epidemic has not had so many foes to contend with, for in the earlier stages of its career the weather was not only cold for the time of year but also calm and quiet. The germ was therefore able to settle in our midst without serious opposition, and the ungenial nature of the atmosphere has doubtless been responsible for much of the lung and bronchial disease which has followed in its train. Deluded by the knowledge that the spring season was upon us, and forgetful of the fact that it had come in an unkindly guise, many a weakly convalescent has been emboldened to venture out into the chilly air, and has contracted a serious cold, from which in too many cases he has been unable to recover. FREDK. J. BRODIE.

THE MUSEUMS ASSOCIATION.

THE Museums Association held its second annual meeting in Cambridge on July 7, 8, and 9, under the presidency of Mr. John Willis Clark, Superintendent of the Museum of Zoology and Comparative Anatomy, Cambridge, and Registrary of the University.

The following representatives of Museums (outside Cambridge) and associates were present:—The Rev. H. H. Higgins, Mr. R. Paden (Liverpool); Mr. R. Cameron, Mr. J. M. E. Bowley (Sunderland); Mr. G. B. Rothera, Mr. J. W. Carr (Nottingham); Mr. Councillor P. Burt, Mr. J. Paton (Glasgow); Mr. T. W. Shore (Southampton); Lieut.-Colonel Turner, Mr. J. Tym (Stockport); Alderman W. H. Brittain, Mr. E. Howarth (Sheffield); Mr. Joseph Clarke, Mr. G. N. Maynard (Saffron Walden); Mr. J. Storrie (Cardiff); Mr. Butler Wood (Bradford); Mr. C. Madeley (Warrington); Mr. J. Lyon, Mr. J. J. Ogle (Bootle); Mr. W. E. Hoyle (Owens College, Manchester); Mr. H. M. Platnauer (York); Mr. F. W. Rudler, Mr. F. A. Bather, Mr. A. Smith Woodward.

The proceedings were opened by the Rev. H. H. Higgins (Past-President), who introduced the President, Mr. J. Willis Clark. The President then read his address, and gave a short and very interesting account of the early history of Cambridge and of the foundation of a few of the older Colleges. On the 8th and 9th the following papers and reports were read and discussed:—

"On some old Museums," by Prof. A. Newton, F.R.S.

"On the desirability of exhibiting, in Museums, unmounted skins of birds," by the Rev. H. H. Higgins.

"On difficulties incidental to Museum demonstrations," by F. W. Rudler.

"On the Dresden Museum cases," by Dr. A. B. Meyer.

"On the registration and cataloguing of Specimens," by W. E. Hoyle.

"Some recent Museum legislation," by E. Howarth.

"On the arrangement of Rock Collections," by H. M. Platnauer.

"Fossil Crinoidea in the British Museum" (an attempt to put into practice modern ideas of Museum arrangement), by F. A. Bather.

"On Tables and Chairs," by F. A. Bather.

The Report of the Committee appointed to consider the question of securing the aid of specialists.

The Report of the Committee appointed to consider the question of labelling in Museums.

The meeting was eminently pleasant and successful, thanks to the untiring energy and exertions of the President and of Mr. S. F. Harmer (Fellow of King's College), the Local Secretary and Treasurer. Under their guidance several colleges, libraries, and laboratories were visited. Prof. Middleton conducted a party over the Fitzwilliam Museum, and, through the kindness of Prof. Newton, a few of the members visited the Pepysian Library.

TECHNICAL EDUCATION IN INDIA.

SIR AUCKLAND COLVIN, the Governor of the North-Western Provinces of India, has issued an exhaustive minute on technical education in that country, in which the various steps towards the introduction of this system of instruction are summarized. The minute naturally refers chiefly to the North-Western Provinces, but is in fact a summary of what has been done elsewhere. It seems that the idea of introducing technical education in the North-West Provinces, where there has hitherto always been a steadily increasing demand for University education, was first mooted in September 1885, when the attention of the local Government was called to the Madras scheme, which aimed at promoting instruction in industrial arts and manufactures by offering grants-in-aid to encourage the teaching, in schools so aided, of technical science, arts, and handicrafts, and by testing that teaching by a system of public examinations. Nearly a year later the Home Secretary to the Government of India drew up a note on the subject generally, pointing out that there was room for improvement in this branch of education in the great north-west, and inquiring what was being done. The Director of Public Instruction replied that the question of establishing Faculties of Medicine and Engineering was under consideration in the Allahabad University, and also certain preparatory courses of study, while it was proposed to refer the question of agricultural and veterinary schools to the Local Records Department. In January 1888, Colonel Forbes, replying to questions addressed to him regarding instruction in engineering, said he considered that the practical instruction gained by natives in the large railway workshops at Allahabad, Lucknow, and Lahore, and at the Government workshops at Roorkee, was decidedly bearing fruit in the direction of enabling natives to take intelligent and independent control in these branches of technical industry. The railway and Government workshops he considered were the real technical schools so far as this branch of instruction was concerned, and there was no need, therefore, for the Government to establish technical engineering schools. Facilities might be given to selected students at the middle and high schools to go through a four or five years' course at these workshops, but more than that he held was unnecessary. Colonel Brandreth, the Principal of the Thomason College, was unfavourable to any school for technical education for the youthful masses, but would provide special opportunities for exceptional young men, though such opportunities need only be limited in number. "For the higher grades of engineering, I think the ordinary liberal education with a scientific knowledge is most suited, until a man is of an age to know his mind, and elect for the profession, when there should be a strictly technical education for a limited time, two or three years, followed by a careful apprenticeship on works." The late Colonel Ward contended that facilities should be given at the Roorkee College for practical instruction, in addition to the present theoretical course. "If such a technical practical class were formed at Roorkee, students from the schools might be allowed to attend it without going through the College theoretical course." Later on, the Director of Land Records and Agriculture sent in an opinion on the subjects immediately referred to him, and advocated nothing more than the creation of a normal school for survey only, at Cawnpore or Lucknow, suggesting also the establishment of small scholarships for the maintenance of boys in training at the various workshops in the provinces; of an art school at Lucknow; and of agricultural and veterinary schools or classes in high schools; and he proposed that drawing should be made compulsory, competency to teach drawing being prescribed as an essential qualification in all teachers in middle and high class schools. And finally, the Inspector-General of Civil Hospitals reported against the proposal to teach up to a higher standard than that of the hospital assistant class. Then, in March 1888, the Director of Public Instruction forwarded a second report adverse to the establishment of a school of art at Lucknow, and pointing out further that, however desirable was the proposal to introduce drawing into public schools, there were no funds available for the purpose. At the close of the year the Director forwarded a resolution, on the part of the Senate of the Allahabad University, to the effect that any steps to establish a College for training medical practitioners would at present be premature. At this point, says the *Times of India*, in discussing Sir Auckland Colvin's minute, the cold water current ceased. In the February of last year the Director of Public Instruction forwarded a minute by the Allahabad Senate, it which it was decided to establish a Faculty of Engineering, degrees being conferred on men who had passed at least a three years' theoretical course at a properly constituted Engineering College or school. On this subject Sir Auckland Colvin now reports that, so far as he is able to gather, the only place at which engineering can be studied in the North-West Provinces is Roorkee. The Public Works Department, he adds, is of opinion that if degrees are to be conferred by the Allahabad University the Roorkee certificate must be abolished, and the Department prefers Roorkee certificates. In this dilemma the resolution of the Senate has not yet been forwarded to the Government. Then the establishment by the University of a special examination of "a commercial and practical character," aiming apparently at forming a sort of training class for technical education, still remains under consideration. The general conclusion, Sir A. Colvin thinks, is that, on the whole, opinion points to nothing more urgent or pronounced than the expediency of giving greater facilities for obtaining instruction in the subordinate grades of practical engineering, and in the handicraft of the artisan. Sir Auckland Colvin then sums up the subsequent papers on the subject, relating to the offer of the British Indian Association, in July 1887, to establish and maintain, at a cost to the Association of Rs. 500 per month, a school of industry in one of the Wingfield Manzil buildings; the announcement of Munshi Imtiaz Ali of additional individual subscriptions, reaching Rs. 17,440 per annum; to the speeches of Sir Alfred Lyall on the subject; and to the draft rules forwarded by the British Indian Association.

Sir Auckland next devotes himself to a consideration of the systems of technical instruction at work in Bombay and Bengal. From a careful study of the facts and the more or less voluminous papers in which they were originally enshrined, he proceeds to define what is meant by technical education so far as it is applicable to the North-West Provinces. Technical education in Europe he illustrates by Mr. Scott Russell's words: "It is necessary that each individual shall, in his own special profession, trade, or calling, know more thoroughly its fundamental principles, wield more adroitly its special weapons, be able to apply more skilfully its refined artifices, and to achieve more quickly and economically the aim of his life, whether it be commerce, manufactures, public works, agriculture, navigation, or architecture;" and by an extract from Mr. Kirkham's report, in February 1889, to the Bombay Government: "The general principles that the real technical school is the actual workshop—that actual workshops are only called into existence by capital operating in accordance with its own law—that this training, so far as it can be given in schools or colleges, must be, in the main, preparatory and disciplinary, and that the improvement of science teaching all round, and the spread of a practical knowledge of drawing, are the indispensable preliminaries of any form of practical training." But however unanimous the authorities may be so far as the principle of the matter is concerned, directly they come to the practical details there is, as Mr. Kirkham admits, every degree of diversity of opinion, and every system is of course bound in a way to differ from every other system, just as the leading industries of different districts differ. Apart from this, however, the Bombay system was found to be far too elaborate for the North-West Provinces. From Bengal Sir Alfred Croft wrote a very practical and sensible letter, condemning the abolition of the Seebpore workshops, and urging that the primary point, so far as engineer students were concerned, was to learn how to use their hands. He also quoted Mr. Spring, who says there can be no question as to their superiority for public works employment if the men have gone through the course of manual training. "An engineer who has learned to use his hands is, other things being equal, an all-round better and more useful man than one who has not." Sir A. Croft goes on to further condemn the removal of the Seebpore shops from the point of view of the need of the mechanic class. "It may be freely admitted and taken as proved that the maintenance of the shops is undesirable from the point of view of the Public Works Department. But it is no less clear to me that the interests of that Department are in this matter antagonistic to those of technical education; and that the deliberations of the Committee have been chiefly governed by regard to the former." The Government, however, remained in principle unmoved; but happily in practice they agreed with the Director of Public Instruction, and the Government of India followed suit; thus establishing a very important principle in regard to technical education. Armed with all this experience, and conceding for the moment the existence of a demand for men competent to deal with machinery and familiar with all the lower forms of engineering, Sir A. Colvin proceeds to discuss what course the training should take, how best to secure it, and the sources from which the necessary funds could be obtained. With regard to the first point, he thinks that what would mostly be required are facilities for gaining a competent theoretical and practical knowledge of the more subordinate grades of mechanical engineering, such as is necessary to a foreman mechanic, more especially in connection with the steam-engine, the railway workshops, and the iron-foundry; and also of the processes of cotton-spinning as employed in the mills established in the North-West Provinces. "These are the two great branches of industry which in Bombay have been recognized as fields for native labour: which, though in a lesser degree, exist here (in the North-West Provinces), and in regard to which, at present, specialized means of instruction are unquestionably, in these provinces, wanting." With regard to the second point, there exists at Roorkee a Government Engineering College and Government workshops, and it seems probable that these will form the nucleus of the instruction necessary. As to the third point, Sir Auckland Colvin thinks it would be premature to enter into the question of funds until the dimensions of the scheme are definitely decided upon. Finally, to see how far all these views meet the industrial needs of the province, Sir Auckland has decided to seek the aid of a strong Committee, which will obtain from all available quarters information on the points indicated in the minute, deputing members to Calcutta, Bombay, and Madras, and subsequently reporting to Government the result of its inquiries, with its own recommendations, and with full details of any scheme which it may desire to see carried into effect.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The judges for the Johnson Memorial Prize, 1891, have awarded the prize to Mr. M. S. Pembrey, B.A., Christ Church. The judges also select the essays of the following as worthy of mention: Mr. T. I. Pocock, Scholar of Corpus Christi College, and Mr. F. T. Howard, B.A., Balliol College. The Johnson Prize consists of a gold medal of the value of ten guineas, together with the surplus dividends on the money invested. The prize is awarded every fourth year to the candidate who produces the best essay on some subject connected with astronomy or meteorology. The selection of a subject is left to the discretion of the candidates. This year there were six candidates.

Mr. Pembrey was a Fell Exhibitioner of Christ Church, gained a first class in the final honour school of natural science in 1889 (physiology), and obtained the Radcliffe Travelling Fellowship in 1890. Mr. Pocock was placed in the first class of mathematical moderations and also in the final mathematical schools, Trinity term 1891. Mr. Howard was placed in the second class of the final honour school in natural science (geology), and obtained the Burdett-Coutts Scholarship in 1890.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, June 18.—Prof. A. Crum-Brown, F.R.S., President, in the chair.—The following papers were read:—A note on some new reactions of dehydracetic acid, by Dr. J. Norman Collie. In preparing dehydracetic acid, by passing ethyl aceto-acetate through a red-hot tube, it is stated that alcohol is formed; the author finds that large quantities of ethylene gas and acetone are also produced. Dehydracetic acid is also volatile to a considerable extent with steam, and is decomposed by boiling with water to a small extent into carbon dioxide and dimethylpyrone. This latter decomposition is more readily effected by boiling the acid with strong hydrochloric acid. If 50 grams are boiled with ordinary fuming hydrochloric acid, the whole is converted into carbon dioxide and a soluble compound of dimethylpyrone with hydrochloric acid. The barium salt of dehydracetic acid, $(C_8H_9O_5)_2Ba$, seems to be not a salt of the compound $C_8H_8O_4$, but of the true tetracetic acid, $C_8H_{10}O_5$. A very stable copper salt of the formula $C_{24}H_{25}O_9N_3Cu$ is obtained if dehydracetic acid be added to a solution of copper acetate in a large excess of ammonia.—The lactone of triacetic acid, by Dr. J. Norman Collie. In a former paper on the constitution of dehydracetic acid (Trans. Chem. Soc., 1890, 189) the author pointed out that if the formula which he proposed for dehydracetic acid was correct, it would be the δ-lactone of tetracetic acid. And the following list was given showing the connection between the condensed acids formed from acetic acid: $CH_3CO.(CH_2CO)_2.CH_2COOH$, tetracetic acid; $CH_3CO.CH_2CO.CH_2COOH$, triacetic acid; CH_3COCH_2COOH, diacetic acid; CH_3COOH, acetic acid. At that time no acid corresponding to the triacetic acid was known. Since then the author has obtained the lactone of this acid by the action of 90 per cent. sulphuric acid on dehydracetic acid at a temperature of 130°–135°. The properties and reactions of the new compound are described.—The refractive power of certain organic compounds at different temperatures, by Dr. W. H. Perkin, F.R.S. The magnetic relations of substances when examined at temperatures wide apart show that certain variations take place after allowing for change of density. Experiments have been made by the author in reference to the refractive power of liquids under similar circumstances. The results show that the specific refractive power is not constant for all temperatures. By comparing the lines A and F it was found that the dispersion was slightly diminished by rise of temperature. The results were calculated by the formula $\frac{\mu - 1}{d}$. When calculated by Lorentz's formula the numbers gave higher results for high temperatures than for lower ones.—Note on a volatile compound of iron and carbonic oxide, by Ludwig Mond, F.R.S., and Dr. F. Quincke (see NATURE, July 9, p. 234).—The formation of salts, a contribution to the theory of electrolysis and of the nature of chemical change in the case of non-electrolytes, by H. E. Armstrong. The author draws attention to the recent researches of Claisen, W. Wislicenus, and others, which clearly show that ethereal salts form compounds with sodium ethylate, and to the bearing which these results have on the theory of the formation of salts generally. It may be supposed that the acid and the "base" in the first instance combine, and that the salt is formed by subsequent interactions within the molecule. In like manner, acids form dissociable compounds with water, and by the occurrence of change within such systems, under the influence of electromotive force, electrolysis is effected. When the compound is highly unstable, the opportunity for change within its system is slight, the acid is a weak one, and its solution of relatively low conducting power. In the case of non-electrolytes, the occurrence of change may be supposed to occur within complex systems formed by the union of the interacting substances.—Dibenzyl ketone, by Dr. S. Young. The author finds that, in preparing the ketone by heating calcium phenyl acetate in a combustion furnace, only 27 per cent. of the theoretical yield is obtained. However, if the calcium salt be heated by means of the vapour of boiling sulphur, the yield of pure ketone amounts to 76·6 per cent.—The vapour-pressures of dibenzyl ketone, by Dr. S. Young.—The vapour-pressures of mercury, by Dr. S. Young. Two additional observations of the vapour-pressures of mercury at 183°·75 and 236°·9 have been made, and, from the previous results of Ramsay and Young, the boiling-point and the vapour-pressures of mercury have been recalculated.

June 25.—Extraordinary General Meeting.—At the request of certain Fellows to the President, an extraordinary general meeting was summoned to consider a proposal for amending and altering the by-laws. The proposal was moved by Mr. James Wilson and seconded by Dr. Teed. Mr. Cartrighe moved the following amendment: "That this meeting declines to pledge itself to any amendment or modification of the by-laws which has not been approved and recommended to the Fellows for adoption by the Council." Sir F. A. Abel seconded the amendment. Mr. Cassell, Mr. Lloyd, and Dr. Newton spoke in favour of the original motion. Prof. Tilden, Mr. Warington, Mr. Page, Dr. Odling, and Mr. Friswell spoke in favour of the amendment. The amendment was carried by 137 votes to 47 votes.

PARIS.

Academy of Sciences, July 13.—M. Duchartre in the chair.—Calculation of the mean length that a circular tube widened at one end should have in order that a sensibly uniform *régime* might be established, and on the expenditure of the charge that entails the establishment of this *régime*, by M. J. Boussinesq.—Contribution to the study of what are called *natural* prairies, by M.

A. Chatin.—On alkyl cyanides, cyanobenzene, and orthocyanotoluene, by M. A. Haller.—Experimental aërodynamic researches and experimental data, by Prof. S. P. Langley (see p. 277).—Observations of solar spots and faculæ, made with the Brunner equatorial of Lyons Observatory, during the first six months of this year, by M. Em. Marchand.—On a modification of the method of supporting railway and tramway vehicles, by M. Féraud.—On the measurement of capacity, self-induction, and mutual induction by experiments on aërial wires, by M. Massin.—On a new copper hydride and the preparation of pure nitrogen, by M. A. Leduc. The new body was discovered in the course of some experiments on the preparation of pure nitrogen by passing undried air deprived of CO_2 over copper turnings in a glass tube heated to redness and then reducing the resulting oxide by hydrogen. The composition and properties of this hydride have not yet been studied, but from the fact that it is formed at red heat it appears to differ from the body discovered by Wurtz, which is broken up at about 60° C.—Action of light on silver chloride, by M. Guntz. The experiments indicate that when a layer of silver chloride is exposed to light it becomes divided into three superficial layers, the first of which is metallic silver; the second silver subchloride; and the third unaltered silver chloride. These three layers have a thickness which is a function of the duration of exposure, and of the primitive thickness of the layer of silver chloride experimented upon.—On a new gaseous compound, phosphorus pentafluochloride, by M. C. Poulenc. The formation of this compound is expressed by the formula $PF_3 + Cl_2 = PF_3Cl_2$, which also indicates that a contraction of volume occurs. This has been proved experimentally. The gas is colourless, and has an irritating odour. Its density is 5·40, and it may be liquefied at ordinary pressures by reduction to a temperature of − 8°. Reactions with sulphur, phosphorus, sodium, magnesium, mercury, and various other substances, are described. It appears to be a much less stable body than Prof. Thorpe's phosphorus pentafluoride.—Compound of boron bromide with phosphoretted hydrogen; phosphide of boron, by M. A. Besson. Bromide of boron absorbs phosphoretted hydrogen at ordinary temperatures, and the result of the combination is a white, amorphous, very light solid. The composition of this product appears to be represented by the formula BBr_3PH_3. At about 300° it changes colour, and hydrobromic acid is disengaged. The dark brown body that remains is found to contain only phosphorus and boron, the action that takes place being expressed thus—$BBr_3PH_3 = PB + 3HBr$. Boron phosphide has a density about the same as water, in which it is insoluble. Reactions with various substances have been investigated.—Researches on the zirconates of the alkaline earths, by M. L. Ouvrard. One interesting point brought out by the experiments is that an analogy exists between zirconium, tin, and titanium.—Artificial production of datolite, by M. A. de Gramont. By the action of a solution of borate of sodium on silicate of calcium (formed by the precipitation of calcium chloride by sodium silicate) at a high temperature and under pressure, a hydrated silico-borate of calcium has been obtained, which in composition and physical properties appears to be identical with datolite. This is the first silico-borate of definite composition, and corresponding to a natural product, which has yet been obtained.—Action of boron fluoride on nitriles, by M. G. Patein.—On the acid sulphate waters containing iron and aluminium of the environs of Rennes-les-Bains (Aude), by M. Ed. Willm.—On the formation and oxidation of nitrites during nitrification, by M. S. Winogradski.—On the larva form of Parmophori, by M. Louis Boutan.—On the circulatory and respiratory apparatus of some Arthropods, by M. A. Schneider.—On the genus *Euclea* (Ebenaceæ), by M. Paul Parmentier.—On the structure of the primary libero-ligneous system, and on the disposition of foliary traces in the branches of *Lepidodendron selaginoides*, by M. Maurice Hovelacque.—On a fall of small calcareous stones which recently occurred in the Department of the Aude, by M. Stanislas Meunier.

AMSTERDAM.

Royal Academy of Sciences, June 27.—Prof. van de Sande Bakhuyzen in the chair.—Mr. Pekelharing communicated that magnesium-sulphate-plasma or kalium-oxalate-plasma contains a substance which has no active power on pure fibrinogen, but acquires by a combination with lime-salts all the properties of fibrin ferment prepared from washed blood-clot. This substance is precipitated incompletely by dialysis, and completely by saturation with magnesium-sulphate. Its combination with lime is active also in the presence of ammonium-oxalate. In the formation of fibrin, lime is transferred from the ferment to the fibrinogen. Pepton prepared by neutralizing the hydrochloric acid of the digesting fluid with calcium-carbonate, injected in the jugular vein of the dog, does not prevent the clotting of the blood. Wooldridge's "tissue-fibrinogen," prepared from the thymus of the calf, causes coagulation of a pure solution of Hammarsten's fibrinogen when lime-salts are present.—Mr. van Bemmelen communicated a research of Mr. Schreinemaker's on the equilibriums which are possible between the double salt PbI_22KI and water, in the presence or the absence of an excess of one of the components, or of the double-salt itself, or of both. The results are in accordance with the investigations of Dr. Bakhuis Rooseboom.—Mr. Suringar presented to the Academy a new (third) contribution to our knowledge of the Melocacti of the West Indies.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Life of Thomas Sopwith, F.R.S.: B. W. Richardson (Longmans).—Plane Trigonometry: Todhunter and Hogg (Macmillan).—Science or Romance?: Rev. J. Gerard (London).—Les Science Naturelles et l'Éducation: T. H. Huxley (Baillière).—Glasgow and West Scotland Technical College Calendar, 1891–92 (Glasgow, Anderson).—Dictionary of Political Economy, Part 1; edited by R. H. I. Palgrave (Macmillan).—The Total Eclipse of the Sun, January 1, 1889; Report of Washington University Eclipse Party (Camb., Mass., Wilson).—Contents and Index of the first twenty volumes of the Records of the Geological Survey of India, 1886 to 1887 (Calcutta).—Natural Religion in India: Sir A. Lyall (Cambridge University Press).—Journal of Anatomy and Physiology, new series, v., Part 4 (Williams and Norgate).—Photographic Quarterly, vol. ii. No. 8 (Hazell).—Photographic Reporter (Hazell).—Memoirs of the Geological Survey of India, vol. xxiv., Part 3 (Calcutta).—Records of the Geological Survey of India, vol. xxiv., Parts 1 and 2 (Calcutta).

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THURSDAY, JULY 30, 1891.

THE HISTORY OF CHEMISTRY.

A History of Chemistry from the Earliest Times to the Present Day. By Ernst von Meyer, Professor of Chemistry in the University of Leipzig. Translated by George McGowan. (London: Macmillan and Co., 1891.)

OF all branches of natural science, none has a history more profoundly interesting or more fascinating than chemistry. And yet, strange to say, none has received less adequate treatment from the historian. The reason for this comparative neglect is not far to seek. The historian of science must have qualifications which are rarely united in one man: not only must he possess the attributes of the successful writer on social, political, or economic history, but he must also be a past-master in the special branch with which he deals, and be well informed on all its cognate branches. Germany has given us the classical volumes of Kopp; from France comes the learned work of Hoefer; whilst in England we have had, until quite recently, to be content with the somewhat trivial, disjointed, and partial narration of Thomas Thomson. In addition we have had a number of monographs, especially within recent years, on the labours of particular individuals: many of these, like Henry's "Dalton," Wilson's "Life of Cavendish," Bence Jones's "Life and Letters of Faraday," and the remarkable series of biographical sketches which we owe to the facile pen of Hofmann, are delightful works; but these, after all, are only *mémoires pour servir.* As a rule, the more formal and general histories which deal with the organized growth of the science are not very attractive; either their authors lack literary grace and charm, or they are superficial, ill-informed, and, in some cases, so obviously biassed as to render them altogether untrustworthy. And, moreover, not one of them has sought to grapple with the splendid achievements of the last half-century in any truly philosophic manner. Kopp and Hoefer have, between them, told us all that is known, or, in all probability, ever will be known, or need be known, respecting the beginnings of chemistry, and of its growth through the Middle Ages, and down to the end of the last century. We now require somebody to set about doing for this nineteenth century what the German and French historians have done for those that precede it. The labour would be stupendous, but the result might be magnificent. At no period in the history of the science have its generalizations been more brilliant, and its theories more comprehensive, more prolific, and, it may be added, more securely established. The birth of the century saw the extension of the atomic hypothesis to the explanation of the fundamental facts of chemical combination, and it has been the chief and most characteristic work of the century to place that theory on a foundation as sound and as firm as that on which the immortal conception of Newton is based. The historian of the chemistry of the nineteenth century need have no other text than that of the atomic theory; for round this dominant conception all other present-day theories are ranged; it is the centre of a system which it vivifies and feeds, and by which it itself is fed and strengthened in return.

Some attempt at what is here foreshadowed has been made in the book before us, but, excellent as the work is in many respects, it is even more suggestive of what remains to be accomplished. The book is divided into six chapters, of which the fifth and sixth are devoted to the history of chemistry from the death of Lavoisier to the present time, and these two chapters occupy nearly three-fourths of the volume. This portion is not only the larger, but is confessedly the most difficult of the whole. In weighing and criticizing current chemical doctrine, and in discussing the theories of the present, even the most conscientious historian is apt to be unconsciously biassed by the predilections and prejudices of his training and environment. Prof. von Meyer has not been unmindful of this possible danger, but after carefully reading his work we can heartily congratulate him on the success with which he has preserved the "objective attitude" which is essential to the true historian. As he tells us, it has been his earnest desire to shed a clear light upon the conflicting views respecting the development and importance of the chemical doctrines of to-day, and to endeavour to apply a calmer and juster criticism to the services of eminent investigators of quite recent years than has hitherto, in many cases, been meted out to them. It is possible that we apprehend Prof. von Meyer's meaning the more fully when we state that such a catholicity of sentiment and so judicial a temperament have not always characterized the occupant of the Chair of Chemistry in the University of Leipzig.

For the two chapters which treat of modern chemistry we have nothing but unqualified praise, and we earnestly commend them to the attention of those students who desire to have a *coup d'œil* at once comprehensive and accurate of the meaning and tendency of present-day doctrine. When we have regard to the enormous mass of material which has to be systematized, and, as it were, brought within focus, some errors and omissions are inevitable. And it is possible that here and there a slight lack of balance and due proportion may be discerned: some matters have been treated at comparatively great length, whilst others have been but scantily noticed. On this point differences of opinion are sure to arise: *tot homines, tot sententiæ.* But no candid reader can fail to be impressed with the singularly fair and impartial manner with which Prof. von Meyer has dealt with the labours of contemporary workers. It is a pleasure to read a work in which the writer has been superior to the petty Chauvinism which has disfigured certain historical productions of the last twenty years. We would specially indicate the critical notices of the labours and services of Lavoisier, Berzelius, Davy, Dumas, Liebig, and Wöhler, as models of historical acumen, sound judgment, and rigid candour. On the time-honoured question, "With whom should rest the merit of the discovery of the composition of water?" Prof. von Meyer is scrupulously just and impartial. He shows that Lavoisier was so far dominated by his *principe oxygine ou acidifiant* that, in burning hydrogen, "he expected to find an acid as the product of its combustion, and therefore looked for one. It is the undisputed merit of the phlogistonist Cavendish to have proved that water alone is produced by the combustion of hydrogen" (pp. 157–58).

Although he devotes only two chapters to it, it is obvious that it is the main purpose of Prof. von Meyer's work to trace the development of chemistry from the downfall of phlogistonism onwards, and he has therefore only dealt with the earlier periods in order to give the reader a connected view of the growth of the science. This portion of the work is touched with a comparatively light hand, and in some respects compares unfavourably with the rest. Although at times there are graphic sketches—as, for example, in the account of Palissy's work, and in the estimate of Bergmann's services to analytical chemistry, and in the story of that strange compound of truculent charlatanry, gross mysticism, and strong common-sense, who called himself Philippus Aureolus Paracelsus Theophrastus Bombastus—the general impression is not wholly satisfactory, and to trace the historical connection of the several epochs presupposes more knowledge than Prof. von Meyer imparts. It is hardly possible to do justice to the age of alchemy in 40 pages, or to the history of the iatro-chemical period, which includes the work not only of Paracelsus and his school, but also that of Van Helmont, George Agricola, Palissy, and Glauber, in 30 pages. But with the "Geschichte der Chemie" before him, Prof. von Meyer may well have hesitated to plough with the patient heifer of Hermann Kopp.

In his fourth chapter, where he deals with the period of the phlogiston theory, the author begins to expand somewhat, but occasionally, we venture to think, at the expense of strict historical accuracy. Thus it is not strictly true to say that Kunkel laboured "for years" to discover the secret of the preparation of phosphorus (p. 141), or that Cavendish defended the phlogistic theory "with all his might" (p. 118). That singularly austere and passionless person—that "cold clear Intelligence," as Wilson calls him—was utterly incapable of entering the lists as the champion of any theory. He let his Irish friend Kirwan, to whom it was more congenial, do all the fighting. It is hardly correct to describe the calm and philosophic Priestley as "eccentric and of a restless fiery nature." No man gave and got harder knocks in his time than did the kind-hearted, even-tempered old philosopher; he, too, did his fighting "all in the way of business," hitting straight and above belt, and with no malice in his blow; but to call him "eccentric," or "restless and fiery," reveals an entire misconception of his disposition and character. The occasion of Lavoisier's admission into the French Academy is only partially stated, and it is not wholly true to say that amongst all his numerous friends and admirers only one chemist, Loysel, had the courage to protest against his execution (p. 153).

A word in conclusion as to the manner in which Dr. McGowan has done the work of translation. His aim, he tells us, has been to reproduce clearly the sense of the German original, and in this he has, no doubt, succeeded admirably. But a purist might object that, in his efforts to preserve the sense, he has too carefully retained the idiom. To say that "the absorption of medicine in chemistry, the fusion of both together, was the watchword which emanated from Paracelsus" (p. 3) is scarcely a happy method of expression. Nor is this paragraph much better:—

"Spirit of wine—the *aqua vitæ* of the alchemists—continued to grow in importance during the iatro-chemical age, as it had done in the alchemistic. This applied to it not merely from a theoretical point of view, as being a product of various fermentation processes to which much attention was paid, but also from a practical, since Paracelsus and his disciples used it largely in the preparation of essences and tinctures" (p. 95).

On p. 101, Boyle's manor in Dorsetshire is erroneously called "Stolbridge," and on p. 185 "Dalton" is incorrectly printed for "Davy." Such terms as "centrepoint" and "fire-stuff" are not current English. Dr. McGowan's duty as a translator doubtless required him to say that "the nobility and poetry of his [Davy's] nature are shown both in the journals which he kept during his extensive travels in France, Germany, and Italy, and in his beautiful relations to Faraday" (p. 187); but the veracious historian, familiar with the annals of the Royal Institution, would probably have expressed himself differently.

T. E. Thorpe.

PROGRESS IN ELEMENTARY BIOLOGY.

Lessons in Elementary Biology. By T. Jeffery Parker, B.Sc., F.R.S., Professor of Biology in the University of Otago, New Zealand. (London: Macmillan and Co., 1891.)

PROF. JEFFERY PARKER is to be congratulated on having produced an extremely well-written, well-considered, and original class-book. The teaching of so-called "elementary biology" has, in consequence of the coercion of examination schedules and the multiplication of little cram-books dealing with the selected and protected "types," become in this country a very poor thing. The practical work in the laboratory with frog, fern, rabbit, and worm, which was, when first introduced, a step in advance, has become, like so many other things which were good in their origin, a tyranny and an impediment to knowledge. Students have resolutely shut their eyes to all facts but those presented by the schedule types, and teachers of a certain class have seen the easiest way to secure "examination results" in ignoring the generalizations of biology, and in plying their pupils with the regulation details as to the few animals and plants scheduled for dissection. Prof. Parker's book should help to remedy this state of things. His aim has been, he states, to supply the connected narrative which would be out of place in a practical hand-book. I agree with him that the main object of teaching biology as part of a liberal education is to familiarize the student not so much with the facts as with the ideas of science. In this little book the student will find many of the most important conceptions of biological science set forth and illustrated, not by reference merely to the types which he dissects or examines with greatest ease in the elementary course in a laboratory, but by the use of a larger area of well-chosen examples, both of plants and animals. Original woodcuts, often of exceptional merit, are freely introduced in the text.

Whilst the plan of Prof. Parker's book is excellent, I cannot help feeling some regret that he has not carried it out on a somewhat larger scale, so as to make his volume represent for the biology of to-day what the classical "Comparative Physiology" of Dr. Carpenter did for the biology of forty years ago. The defect just alluded to—if it be a defect—is one which can very well be remedied hereafter, since the author will undoubtedly have an opportunity of expanding his book in every direction in a later edition.

Nearly half the book is devoted to the consideration of the phenomena of life as exhibited by unicellular organisms—the Protozoa and Protophyta. There can hardly be any doubt that this is by no means an undue proportion, since it is unquestionable that in these simplest forms the fundamental problems of biology present themselves in the clearest light. We have well-illustrated chapters on Amœba, on Hæmatococcus, on Heteromita, on Euglena, on the Mycetozoa, and then a comparison of the foregoing organisms with certain constituent parts of the higher animals and plants, viz. cells. The minute structure and division of cells and nuclei are fully treated and well illustrated. Then follow separate chapters on yeast, on bacteria, on biogenesis and abiogenesis, and on the more complicated unicellular animals—the Ciliata, from among which are chosen Paramœcium, Stylonichia, Oxytricha, Opalina, Vorticella, and Zoothamnium. A chapter on species and their origin, and the principles of classification, comes next, the illustrative examples being chosen from among the Protozoa already described. The Foraminifera, Radiolaria, and the Diatomaceæ are then brought under consideration. In every chapter the organism or group of organisms treated is made to serve as the concrete basis of a gradually expanding and connected narrative. Thus, in passing to the consideration of such forms as Mucor, Vaucheria, and Caulerpa, the author says:—

> "The five preceding lessons have shown us how complex a cell may become, either by internal differentiation of its protoplasm or by differentiation of its cell-wall. In this and the following lessons we shall see how a considerable degree of specialization may be attained by the elongation of cells into filaments."

A pause is now made, and a brief but thoroughly up-to-date chapter is inserted on "the distinctive characters of animals and plants." Prof. Parker thinks there is a great deal to be said in favour of Haeckel's third organic kingdom—the Protista. I do not agree with him in thinking that it is probable that the earliest organisms were "protists," and that from them animals and plants were evolved along divergent lines of descent.

If we approach this question, not with the attempt to define plants and animals verbally, but with the object of indicating probable lines of descent, the groups sometimes considered as doubtful, and therefore "protist," take rank with great probability either in the animal or the vegetable series. The Mycetozoa and the Volvocineæ fit quite naturally in the animal series; they would be isolated among the Protophyta, and, conversely, the Bacteriaceæ are inseparable from the Oscillatoriæ and other filamentous green plants.

Prof. Parker next proceeds to deal with plants of increasing complexity of structure and function—Penicillium, Agaricus, Ulva, Laminaria, and Nitella; and, as a parallel to these in the animal series, we have two chapters, with excellent woodcuts, on Hydra and on the Hydroid polyps, their colony-building and their alternation of generations. The extremely important facts and theories of spermatogenesis and oogenesis and of fertilization are next set forth, briefly but clearly, and in sufficient detail for the general purposes of the book. In connection with the early development of the fertilized egg-cell of the Metazoon from its unicellular phase to the condition of the diblastula, the question is considered as to how we are to suppose that the passage took place historically from Protozoa to Metazoa or Enterozoa. It is pointed out that there is a break here in the series of living animals known to us, whilst there is no corresponding break in the series of plants: there we pass by insensible gradations from unicellular forms to linear aggregates of cells, and from these to superficial and to solid aggregates.

The *Magosphæra planula* described by Haeckel in 1870 is cited as an animal tending to bridge over the gap in the animal series, but a footnote informs the reader that "unfortunately nobody has since seen this organism." Prof. Parker probably is aware that this is also true of Haeckel's *Protomyxa aurantiaca*, which he figures and describes in an earlier chapter. It certainly is to be regretted that neither of these interesting organisms has been observed again since they were described by Haeckel. However, *Volvox globator* is always with us, and Prof. Parker gives an excellent set of figures and a description of it, and proceeds to show how a two-cell-layered sac—the ancestral gastrula or diblastula—might have been derived from such a colony. He also shows how a primitive diploblastic form might have developed from a multi-nucleate Protozoon, such as Opalina or Oxytricha.

In the laboratory it is convenient to take the Earthworm as an example of that central type of structure which is found under various modifications in all the Cœlomate animals. Prof. Parker, rightly separating himself from the ties of laboratory work, prefers the marine worm Polygordius for his illustration of this grade of structure, choosing it partly on account of its greater simplicity, partly on account of its extremely interesting and well-studied developmental history. As the author contends, a student who reads the two chapters here devoted to the anatomy, physiology, and development of Polygordius, will have an immense advantage either in his subsequent study of the Earthworm, or in reverting to his notes of a previous dissection of that worthy beast. The principle of the comparative method will be revealed to him, and he will learn to distinguish things essential from things non-essential.

Next, with a rush, having scaled the long ladder leading to Polygordius, Prof. Parker takes his reader in one chapter of seventeen pages through the anatomy and morphology of the starfish, the crayfish, the mussel, and the dogfish. This seems and is rather rapid, but the rapidity is intentional and justifiable. By the aid of *this* book the student is intended only to gain a general view of the structure of those animals as comparable to that of Polygordius. For further details he must go on to the special study of animal morphology, physiology, and embryology; or having studied these subjects more or less, he may, by aid of Prof. Parker's clever schematic woodcuts, gain a vivid impression of the unity of organization and the divergence in minor points of structure of the higher animals when compared one with another. Perhaps, however, in that enlarged edition of this book which will at no distant date appear, Prof. Parker will treat the higher animals less unceremoniously; this he might do, and yet retain that conciseness and regard for the essential which form an admirable characteristic of his method.

Mosses and Ferns are treated as the parallel among plants of Polygordius in the animal series; and in a single chapter Equisetum, Salvinia, Selaginella, Gymnosperms, and Angiosperms are surveyed (and excellently illustrated by finished woodcuts) in such a way as to give the student an accurate and highly effective survey of the great features of vegetable morphology and physiology.

Such is the outline of these "Lessons." Their merit, however, consists not merely in the general plan, but in the fact that the author is an experienced teacher and an accomplished investigator, who has developed to a high degree the art of lucid statement—one who is thoroughly familiar with the latest researches in the wide field of which he treats, and is able, whilst setting before his reader the most important generalizations of his science, to avoid redundancy, and to give a fresh and original handling to the oft-told story of the structure and functions of living things.

E. RAY LANKESTER.

CEREBRAL LOCALIZATION.

The Croonian Lectures on Cerebral Localization. By David Ferrier, M.D., LL.D., F.R.S., &c. With Illustrations. (London: Smith, Elder, and Co., 1890.)

IN these valuable lectures, Dr. Ferrier reviews the subject of cerebral localization, so far as the representation of movement and of special sense is concerned. After referring categorically, in the first of the series, to the historical experiments on the subject, arranged in order of chronological sequence, he points out the fundamental principles embodied in the term cerebral localization. Leaving the discussion of motor representation, he devotes the remaining five lectures to the consideration of the cortical representation of the special senses, beginning with that of sight.

The representation of sight is, according to all observers, mainly restricted to a definite area of the cortex. The differentiation of that area and its topographical subdivision are points of the highest interest, and naturally do not escape discussion. We are rather surprised, however, to find that Dr. Ferrier is not prepared to admit that Munk and Schäfer's experiments, besides those of other observers, establish visual representation to be situated in the occipital lobe, but is inclined to believe that the angular gyrus is the centre for clear vision mainly for the eye of the opposite side. Upon this we would only remark that it does not appear to us that the mass of evidence relating to crossed hemianopsia, whether of experimental or clinical nature, can be put aside as easily as Dr. Ferrier would seem to consider possible, but those interested in the subject will find many of the facts bearing on this question referred to in his treatment of the points at issue.

So, too, with the representation of audition, while all (save Schäfer's and Sanger Brown's) observations support Dr. Ferrier's views of the seat of representation of hearing, it would undoubtedly have been better that the rebutting evidence brought against the exceptional facts referred to should have consisted of a number of experiments, and not of a single one, even although that seems to have been a very conclusive observation.

After disposing of the centre of audition, the tactile centre receives attention, and is preceded by a discussion of the paths along which afferent impressions travel in the spinal cord to the higher centres. Of course, this subject has been very actively investigated by various observers for many years, but it has always appeared to us that sufficient attention has never been given to the simple consideration whether or not the *lower* centres are engaged in the transmission of such impulses. In the limited space at Dr. Ferrier's disposal he has evidently not been able to give this matter full discussion, and is therefore led to assume that Brown Séquard's dictum respecting the passage of afferent (tactile, not painful) impulses up the opposite side of the cord holds good. This question is now being reinvestigated, and the preliminary observations published by Mott and others throw very grave doubt on the validity of this assumption, which has so long been accepted as final.

As regards the representation of common tactile sensation in the cortex cerebri, Dr. Ferrier discovered that it was probably represented in the hippocampal region, and he reviews the results of his experiments, as well as those of Schäfer and Horsley, which tended to show that the gyrus fornicatus, as well as the hippocampus, were the seat of tactile perception, and he concludes that possibly the whole limbic lobe is concerned with this representation.

As regards, however, the representation of sensation in the excitable or motor part of the cortex, he will "have none of it." Here, again, we are afraid that the considerations of time and space, which always handicap subjects treated in lecture form, account for the fact that the critical examination of this question is not so complete as perhaps it might have been made.

On the whole, these lectures well maintain the author's high reputation as a keen observer, and an indefatigable student, gifted with singular clearness and distinctness of expression, and they will well repay perusal by all who wish to follow the progress of knowledge of cerebral localization and its most important bearings.

OUR BOOK SHELF.

Education and Heredity. By J. M. Guyau. (London: Walter Scott, 1891.)

THIS small and excellently-translated work is a posthumous publication, written by a Frenchman who died four years ago at the early age of thirty-three. He was a fluent and prolific writer, the author of no less than fourteen other publications, and is described in the introduction as a philosopher and poet. It would seem from this book that the latter temperament was his prevalent characteristic. Its prevalent literary style and the originality both of metaphor and of handling will commend itself, and so will the account of recent hypnotic investigations, and the use made of them in the argument. Interesting and appropriate quotations are inserted from numerous authors of fame and notoriety, as from Plato, Descartes, Leibnitz, and Spencer, down to Tolstoi. But when, after reading right through the book, one asks oneself what has been the net gain, what new ideas it has given, or what valuable facts it has brought together, and what are its solid and original arguments, it is rather difficult to give a satisfactory reply. The book chiefly consists of well-phrased "talkee-talkee," so that some readers may feel a little grateful to so fluent and prolific a writer that he stopped his nimble pen even as soon as he did. One has become nowadays rather satiated with *a priori* deductions.

As for the "Heredity" in the title, it is nowhere in the book, except at the end of one chapter, where neither the author in the text nor the translator in the footnotes has shown any misgiving concerning the truth of the old supposition of the free inheritance of acquired faculties, which greatly affects the argument of the work. Undoubtedly some few men of high authority still entertain the older view, but the majority of students of heredity now regard it as unproved, and at the best, that the inheritance is very slightly efficient.

The following paragraph will serve as an example of what is least good in the author's style and method:—

"Why then should not the representation of man, by hereditary tendency, excite in man himself a peculiar pleasure, and an inclination no longer of flight, but to approach, speak, be helped, to put others in his place? When a child falls under the wheels of a carriage, we precipitate ourselves to its rescue by an almost instinctive movement, just as we should start aside from a precipice. The image of others is thus substituted for the image of ourselves. In the scales of the inner balance, *I*, *thou*, are constantly interchanged. This delicate mechanism is partly produced by heredity. Man is thus domesticated, made gentler, and more civilized; now he is partially savage, partially civilized or civilizable. The result of education through the ages is thus fixed in heredity itself, and this is one of the proofs of the power possessed by education, if not always for the present, at least for the future."

Life is short, there is much to learn, and economy of time is important. It is questionable whether it is worth the while of a person who has some acquaintance with the subject of this book to spend half a working day in reading it, for he might not find it as nourishing as he would wish. Still it is not unlikely that those to whom the subject is unfamiliar would gain instruction from the book and would consider it throughout to be interesting.

F. G.

The Soul of Man: an Investigation of the Facts of Physiological and Experimental Psychology. By Dr. Paul Carus. (London, Edward Arnold.)

IT is in vain that a puzzled reader seeks to discover the aim of this book. It is entitled "The Soul of Man," but no explanation is given as to what is meant by the title; and at the end of forty-six rambling and discursive chapters on things in general, the reader finds himself no wiser. It is called "an Investigation of the Facts of Physiological and Experimental Psychology," but there is no investigation of facts in the book. The rudiments of anatomy, of embryology, of neurology, &c., are set forth, much in the form in which they can be found in elementary text-books on the subjects, but the facts thus presented are not investigated; they are presented in no new light, no new conclusions are drawn from them, and the object of their presentation does not appear. Here and there, indeed, the author states a belief for which in the preface he claims originality; he considers, for instance, that consciousness (which he calls a concentrated or intensified feeling—an additional element that sometimes is, and sometimes is not, attached to mental operations) is "produced" in the corpus striatum. It does not appear, however, that this hypothesis leads to anything, or has any appreciable bearing on the "problem of the human soul," whatever that may be. Dr. Carus thinks, too, that man has two souls, a central soul and a peripheral soul; and it is thus that he explains the familiar fact that certain purposive actions are unattended with consciousness; but we cannot say that this explanation makes the matter any clearer. As a contribution to science, the book cannot be commended. Whether it has a theological value, we must leave to others to say.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Recent Earthquakes in Italy.

WITH reference to the letter which appears in your issue of July 23 (p. 272), on the earthquakes having occurred at Vesuvius on June 7, and on the same day in Southern Australia, I would ask leave to point out that the localities mentioned lie in the vicinity of a great circle which I call the "south-west coast of Australia great circle" (that is, the coast-line between Cape Hamlin and Cape Chatham). Melbourne would be about 370 miles north of its direction, and it cuts Italy in the neighbourhood of Catanzaro, leaving Vesuvius about 65 miles to the north. This great circle is one of maximum compression on the earth's surface—that is, it lies for the most part on the ocean surface; its greatest extent on land being in traversing Arabia, which it crosses in a north-west, south-east direction.

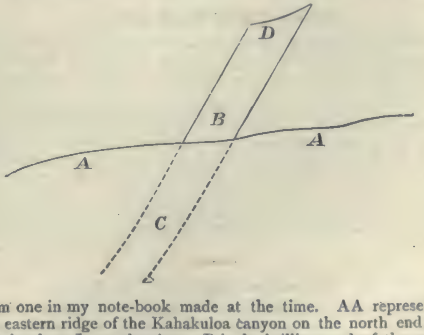
It is also worth noting that, while you cite in the same issue two shocks as having occurred in the Æolian Islands on June 24 (of these, Stromboli lies about 40 miles south of the direction of this great circle), there was recorded on that day, in the newspapers, an earthquake shock as having taken place on the 23rd (midnight) at Charleston, South Carolina, which lies about 650 miles to the north-west of the direction of the great circle in question at this point, and therefore approximatively in the vicinity.

J. P. O'REILLY.

Royal College of Science for Ireland,
Stephen's Green, Dublin, July 24.

The Great Comet of 1882.

IN your issue of May 28 (p. 82) is a communication about the comet of 1882 as seen in the act of passing close to the sun. As attention has thus been called to that comet, I desire to report a remarkable peculiarity of the tail as observed by myself, October 3, 1882, about daybreak. It was my first view of this glorious comet. Other persons on the east sides of the islands had seen it several days earlier. The peculiarity noted was the *abrupt ending* of the tail, which was cut off sharply at an oblique angle, on an incurved line. The following representation is copied from one in my note-book made at the time. AA represents the eastern ridge of the Kahakuloa canyon on the north end of Maui, where I was sleeping. B is the brilliant end of the vast tail like a scimitar blade, fully as bright as the moon. C is



copied from my note-book. It was evidently meant to indicate the continuation of the tail towards the nucleus, as seen on subsequent mornings, when farther from the sun. D is the terminal edge of the tail, as sharp as the outer limb of the moon, and of fullest strength of lustre. Altogether it formed a rather appalling apparition. Clouds soon obscured it. No farther view was obtained for two or three days, when the end of the tail had assumed the usual misty, indefinite outline.

The conclusion forced upon my mind was that the comet, having parted with its tail in its rapid turn at the perihelion, was seen in the act of forcing out a new one ahead of itself, in a solid bank of vapour, the front of which might be compared to the wall of water that heads a freshet in a stream. Another resemblance suggested was that of the solid-looking outline of an up-rolling cumulus cloud.

I will add hereto a statement made to me at the time by the Rev. Hiram Bingham, a distinguished pioneer missionary to the Gilbert Islands. He saw the comet about a week earlier than myself, from Kaneohe, on the east side of Oahu. Both he and his wife observed *waves of prismatic colours running outward* along the brilliant tail. Mr. Bingham is a highly cultivated person, and having commanded the missionary ship for part of two years, is accustomed to lunar and stellar observation. I was led at the time to believe that there was no optical illusion in what he saw.

SERENO E. BISHOP.

Honolulu, June 30.

Copepoda as an Article of Food.

PROF. HERDMAN'S practical demonstration at the North Cape confirms a theory I have long held, that the Copepoda, which abound in every ocean, sea, and lake, might be largely and advantageously made available for human food. It is well known that the species *Calanus finmarchicus*, so abundant in our northern seas, forms the chief food of the Greenland whale. Our own immediate coasts abound in this and other equally edible species. During a recent dredging cruise round the Isle of Man, each pull of the tow-net contained thousands of another and larger species of Copepod, *Anomalocera patersonii*; and Dr. John Murray has found that a still larger species, *Euchæta norvegica*, is plentiful in the lower depths of several Scotch lochs.

A number of finely-meshed trawls, used off the west coast of Ireland, would, I am convinced, furnish excellent food for starving multitudes in time of need.

A propos of the distribution of Copepoda, my attention was called a few days ago by the Mayor of Bootle to the filter-beds of the town salt-water baths, which he said were swarming with Entomostraca. The water is supplied direct from the river, and examination showed the presence of Copepoda in enormous quantities, the bulk of them being *Eurytemora hirundo*, a species only once before taken in Britain, and then in near proximity to Bootle. Probably other filter-beds are equally prolific, and may prove valuable hunting-grounds, the Copepoda undoubtedly acting as scavengers in keeping the water pure from putrefaction.

I. C. THOMPSON.

Liverpool, July 24.

Meteorological Phenomenon.

I HAVE received in a letter from a friend residing in Boraston, Shropshire, the following account of a remarkably interesting meteorological phenomenon, which is well worth putting on record:—

"We had a curious sight from this house yesterday [July 26]. It was a dead calm, but in a field just below the garden, with only one hedge between us and it, the hay was whirled up high into the sky, a column connecting above and below, and in the course of the evening we found great patches of hay raining down all over the surrounding meadows and our garden. It kept falling quite four hours after the affair. There was not a breath of air stirring as far as we could see, except in that one spot."

FRANCIS GALTON.

Refraction through a Prism.

IN such elementary text-books on geometrical optics as I have consulted it has always seemed to me that the writers have found a difficulty in presenting a precise direct proof of the theorem that when a ray is turned out of its course by direct passage through a prism, its deviation is least when its path is symmetrical with regard to the prism.

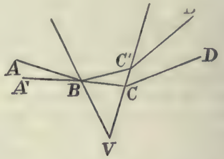
May I ask you to consider the simple proof which I inclose, and may I leave it to your judgment whether it is worth while that it should be presented to the notice of teachers in the pages of NATURE? My knowledge of text-books I cannot suppose to be exhaustive, and the arrangement of the proof which I inclose of course may not be any novelty.

JOHN H. KIRKBY.

Radley College, Abingdon, June 11.

Minimum Deviation.

The problem is to find two rays which, passing directly through a prism near together, have their directions changed by the prism to the same amount—for in the limit, these, when brought into coincidence by change of position of the prism, will mark the course of that ray which suffers minimum deviation (experiment may be appealed to, to show that it is minimum and not maximum). Let ABCD be the course of a ray of light through the prism whose vertex is V. At B make the $\angle VBC' = \angle VCB$, then if the ray BC′ is continued out of the prism on both sides, it is evident that its completion D′C′BA′ meets and leaves the faces of the prism at exactly the same angles as the original ray ABCD, only in the opposite direction. Thus the two rays ABCD, A′BC′D′ suffer equal deviation, and because the Δ's VBC, VC′B are similar,



$$\therefore VB^2 = VC \cdot VC';$$

and when the rays are so close as to practically render C, C′ coincident, we have $VB^2 = VC^2$, or $VB = VC$ when the deviation is a minimum, *i.e.* the deviation is a minimum when the course of the ray makes equal angles with the sides of the prism.

[Oxford men will remember that more than twenty years ago Prof. Clifton gave a somewhat similar proof as follows:—

Since the paths ABCD and D′C′BN are similar, if one is a path of minimum deviation the other must have the same property also. Hence, since light can always travel in the reverse direction along a path, the paths

ABCD and NBC′D′

are both paths of minimum deviation.

But the existence of two such minima is contrary to experiment. Hence the paths must be identical, which can only be the case of the angle VBC = VBC′ = VCB.—ED.]

Further Notes on the Anatomy of the Heloderma.

SINCE I published in NATURE (vol. xliii. p. 514), "The Poison Apparatus of the Heloderma," there has appeared from the pen of Mr. Boulenger another notable contribution to the anatomy of that genus of reptiles, entitled "Notes on the Osteology of *Heloderma horridum* and *H. suspectum*, with Remarks on the Systematic Position of the *Helodermatidæ* and on the Vertebræ of the Lacertilia," (P.Z.S., January 20, 1891). That paper is especially useful, inasmuch as it critically compares the vertebral columns of the two species of Lizards under consideration—a comparison which, up to the time of the appearance of Mr. Boulenger's paper, had not been made. To briefly recapitulate his points, Boulenger finds differences in the form of the premaxillaries of the two species, and in the number of teeth supported by those bones. He finds palatine and pterygoid teeth constantly absent in *H. suspectum* but present in *H. horridum*—a very remarkable fact. A small azygous ossification was found in the cartilage of the mandibular symphysis of *H. horridum*, "apparently the homologue of the symphysial (mento-meckelian) bones of most tailless Batrachians." This last discovery has important bearings in other directions. In the vertebral column there appear to be a total of 76 vertebræ in the case of *H. horridum* to but 63 or 65 in the spine of *H. suspectum*. And, in conclusion, this distinguished herpetologist remarks that "A short rib is present on the third cervical in *H. horridum*, which is absent in *H. suspectum*; the neural spines are more elevated in the middle and posterior portion of the dorsal region in *H. horridum*, specimens of the same sex, of course, being compared. The neural spines are much more developed in the male" (p. 116). Boulenger still thinks the place of the *Helodermatidæ* between the *Anguidæ* and the *Varanidæ*, which he assigned to them in 1884.

In concluding this notice I am led to pass some observations upon certain strictures Mr. Boulenger has made in his paper upon my memoir on the anatomy of *H. suspectum* which appeared in the Proc. of the Zool. Soc. of London for 1890. His criticism of my description of the atlas of *H. suspectum* is well taken, as I have satisfied myself of by an examination of better material since. That bone is found to be in *three* pieces, and not in *five* as stated by me. He is also quite correct when he comes to point out certain errors in my figures of *manus* and *pes* of that reptile, and I thank him for having called my attention to them. With respect, however, to the error he believes me to be guilty of in my description of the teeth in the premaxillary bones of *H. suspectum*, I can in no way agree with him. He observes: "Eight or nine præmaxillary teeth are present in *H. horridum*, and only six in *H. suspectum*. Dr. Shufeldt, however, represents eight teeth in the latter species; but his figure, showing all the teeth as of the same size, looks very diagrammatic." In his figures illustrating these remarks Mr. Boulenger gives *H. horridum* but *six* teeth, and *H. suspectum* but *four*, and the drawings of the bones look, indeed, very diagrammatic. I cannot conceive of any lizard normally having but "nine" teeth in its premaxillary bone; it should at least be an even number. Now the mounted specimen of *H. suspectum* in the collections of the U.S. National Museum, has *eight* teeth in its premaxillary, and it was from that specimen that I drew my figure which appeared in the Proceedings. Normally, that is the number, but those teeth are often broken out in the Heloderms, and they become irregular by subsequent growth. The outer ones are always the longer, when the skull is perfect. In so far as the *form* of the premaxillary is concerned, as touched upon by Mr. Boulenger, I believe no little allowance must be made for *individual variation*, which is often quite considerable among lizards as it is among Vertebrates higher in the scale. Other figures illustrating the work under consideration are excellent.

It would appear that it is to be the fate of the *Helodermatoidea* to have their morphology more thoroughly worked out than most, or even any other, lizards up to the present time; and I am given to understand that Prof. Garman, of Harvard College, has it in mind to review, in the near future, the entire structure of *H. suspectum*.

R. W. Shufeldt.

Smithsonian Institution, July 8.

THE DISCOVERY OF THE STANDARDS OF 1758.

THE discovery by the Clerk of the Journals of the House of Commons, referred to in NATURE last week (p. 280), of the original standards of length, which were in 1758 deposited in the custody of the Clerk of the House, has attracted some attention to the history of these Parliamentary standards. As some misapprehension as to the effect of such discovery appears to have arisen, and as it is to eminent men of science that we are mainly indebted for our standards of length, the following explanatory notes may interest many of our readers.

The standards of length above referred to were made under the directions of a Committee of the House of Commons, of which Lord Carysfort was Chairman, appointed on May 26, 1758, "to inquire into the original standards of weights and measures in this kingdom." The Committee reported that in 1742 several members of the Royal Society were at great pains in taking an exact measure of the ancient Exchequer standards of length (of Henry VII. and Queen Elizabeth), then condemned by the Committee as being coarsely made and "bad standards"; that such measure was made by "very curious instruments prepared by the late ingenious Mr. Graham"; and that the Royal Society had lent to the Committee a brass rod made pursuant to these experiments, which rod had been compared by Mr. Harris, of the Royal Mint, with the Exchequer standards. Mr. Harris advised the Committee that the Royal Society's standard was made so accurately, and by persons so skilful and exact, that he did not think it easy to obtain a better standard; and accordingly the Committee then had two rods made by Mr. Bird, an optician, according to Mr. Harris's proposal; which "rods" were laid before the House. The rod marked "Standard Yard, 1758," was to be taken as the proper standard; it was stated by the Committee to be made of brass, to be about 38 or 39 inches in length and about one inch thick; near to each end of the rod a fine point and line being drawn on a gold stud, the distance between the points on the gold studs being the "true standard length of a yard," or 36 inches. The second rod was made in the same manner as the first rod, excepting that it had "two upright cheeks" instead of points or lines; so that any other yard rod might be measured by being placed between the cheeks. Both these rods (together with three standard troy pounds marked "$\overset{\text{lb}}{\underset{1758}{\text{T}}}$," with a crown and "G.2," and a set of troy standards from 2 pounds to 32 pounds, made and adjusted by Mr. Harris "with very curious and exact scales of his at the Mint") were stated by the Committee to be then deposited with the Clerk of the House of Commons.

In 1838 the attention of the Government was directed to the necessity of determining a new standard weight and measure to replace the above standards of 1758, which were stated by the Chancellor of the Exchequer—in a letter to Mr. G. Airy, the Astronomer-Royal—to have been "destroyed by the burning of the Houses of Parliament," and a Commission was appointed to restore the standards. The Commission included F. Baily, J. E. D. Bethune, Davies Gilbert, J. S. Lefevre, J. W. Lubbock, Geo. Peacock, R. Sheepshanks, J. Herschel, and G. B. Airy. Their report of 1841 gives a precise description of the condition of the standards at the Journal Office immediately after the fire. The Committee reported that the legal standard of one yard was "so far injured that it was impossible to ascertain from it with the most moderate accuracy the statutable length of one yard"; and also that the "legal standard of one troy pound was missing." New Parliamentary standards of length and weight were accordingly made under the directions of the Committee, and were legalized by an Act of Parliament passed in 1855. These new Imperial standards are now deposited with the Board of Trade, but legal "Parliamentary copies" of them are stated to have been immured, in 1853, in the House of Commons, and further copies were then deposited at the Royal Mint, the Royal Observatory, and with the Royal Society. These latter Parliamentary copies are legally required to be compared with each other once in every ten years, but those deposited at the House of Commons are excepted from any such comparison. It would appear, however, that the House of Commons standards are sometimes examined, as is shown by some printed correspondence on this subject which was laid before the House of Lords in 1872, in which year the standards were examined, and after their examination were again immured in a wall near the lower waiting hall of the House of Commons; a certificate of the deposit of the standards being given as follows:—

"It is hereby certified that this day, in the presence of the undersigned, the oaken box containing the Parliamentary Copy No. 4 of the Imperial Standard Yard, and the Imperial Copy No. 4 of the Imperial Standard Pound," . . . has been "deposited within the wall on the right-hand side of the second landing of the public staircase leading from the lower waiting hall up to the Commons Committee Rooms, a brass plate having been fixed upon the wall bearing the following inscription in Elizabethan or church text:—Within this wall are deposited standards of the British Yard Measure and the British Pound Weight, 1853." The certificate is signed by G. B. Airy (Astronomer-Royal), John George Shaw Lefevre (Clerk of the Parliaments), W. H. Miller, C. P. Fortescue (President of the Board of Trade), H. W. Chisholm, and H. J. Chaney; and is dated March 7, 1872.

It hardly appears, therefore, that the old standards of 1758, which appear to have remained unnoticed for the past fifty years, are now of any importance for the purposes of measurement.

MAXWELL'S ELECTRO-MAGNETIC THEORIES.[1]

AN account of Maxwell's electric theories from the pen of Prof. Poincaré could not but be full of interest. The volume before us is the first of two on the views and conclusions set forth in the "Electricity and Magnetism" regarding electro-static and electro-magnetic action, and their verification by Hertz and others; and we must of course wait for the completion of the work before we can form any adequate idea of its scope and character, and fully understand the results of the critical analysis which it contains. But in spite of the fact that the treatise is in the somewhat disadvantageous form of an edited course of lectures, it is a contribution of great value to the literature of the subject. Whether or not it is possible always to agree with the physical views expressed regarding matters which are not yet outside the region of speculation, it is impossible not to admire its style and methods. Here are to be found exemplified that order and harmony which render the work of the best French mathematical writers so exquisitely clear, and that artistic charm which is so seldom seen in the writings of scientific men of other nationalities. It has been remarked by competent critics that Maxwell's work, though essentially that of an artist and man of genius, is obscured here and there by a certain vagueness and want of logical coherence and completeness, which has tried the patience and strength of many a devoted disciple. This was of course to a great extent inevitable. He sought out new fields of speculation for himself, and his greatest and most successful generalizations were, one cannot help feeling, the results rather of unerring intuition than of any completely systematic process of reasoning. Those who follow in his footsteps therefore are glad of the help of any friendly guide who is able by his experience and strength to point out the dangers and diminish the difficulties which attend their progress.

In his introduction Prof. Poincaré gives a critical estimate of Maxwell's theories which strikes one at first sight as somewhat inappreciative. Thus he says:—

"La première fois qu'un lecteur français ouvre le livre de Maxwell, un sentiment de malaise, et souvent même de défiance se mêle d'abord à son admiration. Ce n'est qu'après un commerce prolongé et au prix de beaucoup d'efforts, que ce sentiment se dissipe. Quelques esprits éminents le conservent même toujours. . . . Ainsi en ouvrant Maxwell un Français s'attend à y trouver un ensemble théorique aussi logique et aussi précis que l'optique physique fondée sur l'hypothèse de l'éther; il se prépare ainsi une déception que je voudrais éviter au lecteur en l'avertissant tout de suite de ce qu'il doit chercher dans Maxwell et de ce qu'il n'y saurait trouver.

"Maxwell ne donne pas une explication mécanique de l'électricité et du magnétisme; il se borne à démontrer que cette explication est possible.

"Il montre également que les phénomènes optiques ne sont qu'un cas particulier des phénomènes électromagnétiques. De toute théorie de l'électricité on pourra donc déduire immédiatement une théorie de la lumière.

"La réciproque n'est malheureusement pas vraie; d'une explication complète de la lumière, il n'est pas toujours aisé de tirer une explication complète des phénomènes électriques."

The author, however, shows throughout his exposition that he is not only impressed with the extraordinary importance of Maxwell's work, but also thoroughly appreciates and admires, if occasionally under protest and with longing after the more ancient classic models, its somewhat wild and native beauty.

An important part of the introduction is an exposition of the theoretical basis of what Prof. Poincaré rightly regards as the fundamental idea of Maxwell's treatment of electro-magnetism—that is, the application of the general processes of dynamics to any system of current-carrying conductors. No doubt almost all the work which had been done previously had been more or less of this nature, but we refer here to the attempt which Maxwell made with very considerable success to correlate electro-magnetic phenomena by means of Lagrange's general dynamical equations.

In the Lagrangian method the physical state of a system is defined by means of certain parameters $q_1, q_2, \ldots q_n$, n in number; and a dynamical explanation is obtained, or proved to be possible, when the values of these parameters are found in terms of, or proved to be related to the positions and motions of a system of connected particles, either of ordinary matter, or of some hypothetical fluid.

If $m_1, m_2, \ldots m_p$ be the masses of these particles, x_i, y_i, z_i the Cartesian co-ordinates of the particle of mass m_i, and if the system have potential energy V, a function of the $3p$ co-ordinates of type x_i, y_i, z_i, there are $3p$ equations of motion of the form

$$\left.\begin{matrix} m_i \ddot{x}_i + dV/dx_i = 0 \\ \text{\&c.} \qquad \text{\&c.} \end{matrix}\right\} \quad \ldots\ldots \quad (1)$$

The kinetic energy T is

$$\tfrac{1}{2}\Sigma m\,(\dot{x}^2 + \dot{y}^2 + \dot{z}^2),$$

and the principle of conservation of energy gives $T + V = \text{constant}$.

Now we know V, and can express the co-ordinates of each particle or molecule in terms of the n parameters $q_1, q_2, \ldots q_n$. The celebrated Lagrangian equations in terms of the parameters can then be obtained by direct transformation of (1), and are of the type

$$\frac{d}{dt}\frac{\partial T}{\partial \dot{q}_k} - \frac{\partial T}{\partial q_k} + \frac{\partial V}{\partial q_k} = 0.$$

Here T and V are homogeneous quadratic functions, the first of the quantities of type $\dot{q}$, with coefficients which are functions of the parameters themselves, the latter of the parameters only.

If we have reason to believe that the system we are dealing with is a dynamical system, for which the values of T and V (or, more properly, those parts of the total kinetic and potential energies which are concerned in the special phenomenon treated), can be obtained by observation of parameters of type q, we can use these equations in our discussions of results, whether or not we can actually express the parameters in terms of co-ordinates of particles of the system. The justification of this process is the agreement of the results with experiment.

If now we imagine a system of particles (whether of actual or hypothetical matter) say p in number, which has the required values of T and V, and which further gives the same relations of the parameters q, we have obtained a dynamical explanation of the phenomenon. Prof. Poincaré remarks with respect to this process that no dynamical solution of the problem obtained in this way can be unique, and that in fact it must be possible to obtain in this way an infinite number of different solutions, or to quote his own words:—

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[1] "Électricité et Optique." I. Les Théories de Maxwell et la Théorie Électromagnétique de la Lumière. Par H. Poincaré, Membre de l'Institut. (Paris: Georges Carré, 1890.)

"If any phenomenon admits of a complete mechanical explanation it will admit of an infinite number of others which equally well account for all the results of experiment."

This, as he reminds us, is confirmed by the history of physical inquiry. Theories inconsistent with one another are elaborated by different persons, and explain the known facts so well that there is hardly anything left to decide which is right. For example, according to Fresnel the direction of vibration in a ray of plane polarized light is perpendicular to the plane of polarization, according to Neumann and MacCullagh it is *in* the plane of polarization. It can hardly be said that any perfectly absolute *experimentum crucis* has yet been found to decide between these two theories, although the balance of evidence seems decidedly in favour of the view of Fresnel.

It is, however, to be remembered that while we can find different mechanical theories to explain the facts, the theories are not necessarily distinct; the mechanism proposed performs functions which must be performed by the actual mechanism whatever that may be. There always is, as the above cited case well illustrates, a unity connecting the different explanations and a consequent element of similarity among them; and each satisfactory theory elaborated must tend to progress by suggesting modes of deciding in what respects it is redundant or inadequate.

The difficulty then as to real mechanical explanations of phenomena does not prevent us from making progress in our knowledge of matter. The Lagrangian method, and this is its remarkable merit, enables us to use the parameters instead of the co-ordinates of actual particles, and thereby to predict the existence of further properties of matter capable of throwing light on those already observed. In this way may be lightened the task, happily not likely to be soon relinquished by the human intellect, of inquiring into the actual constitution of matter and the mutual actions of its parts.

There seems, however, no doubt that Prof. Poincaré is correct in his view that the central idea of Maxwell's treatise is to prove the existence of a mechanical explanation of electrical phenomena, not indeed actually finding it, but by showing that the Lagrangian method, which presupposes such an explanation, is applicable, and leads to consistent results.

Coming now to the detailed exposition of Maxwell's theories, the first thing that calls for notice is the theory of electric displacement. This has always been a subject of considerable difficulty. What is electricity? is it the ether or something in the ether? in what consists its displacement? are questions which the anxious inquirer is continually putting, and putting in vain. Maxwell's electric displacement and electric force remain simply analogues to the strain and stress in an elastic solid, and it can hardly be said that anyone has yet brought them out of the category of abstractions. No doubt the mechanical analogues suggested by Maxwell himself and by others are helpful in fixing the ideas and enabling the mind to form some concrete conception of what takes place in the medium; but they may easily be carried too far, and prove the means of leading to error. It is almost better in some respects to remain content, if possible, with abstractions, until further light as to the properties of the ether is obtained by experiment and observation; and perhaps it is on this account that Maxwell has abstained from giving such illustrations in his treatise. On the other hand, some notion corresponding to that of electric displacement is necessary for any theory of electrical action regarded as propagated through a medium surrounding the electrified bodies, whose charges become thus the surface manifestation of the state of constraint set up in the dielectric by the electrification.

Prof. Poincaré distinguishes between two fluids—one which he calls *electricity*, and the other the *fluide inducteur*. Both fluids are incompressible, the latter fills all dielectric space, the former is capable of being produced at or placed at any given place or on any given surface. If, then, within a closed space a quantity of electricity is introduced, as, for example, when a charge is placed on the surface of a conductor, an equal quantity of the *fluide inducteur* is forced out across the bounding surface. When all the conductors of a system are in the neutral state, the *fluide inducteur* is in normal equilibrium; when, on the other hand, the conductors are electrified, the equilibrium ceases to be normal and the state becomes one of constraint.

There is some advantage in thus distinguishing between the fluid constituting the electrification and that filling the surrounding space, as it avoids some difficulties of explanation and treatment which arise when only one fluid is considered as producing the phenomena.

After a rather lengthy but in many points critical exposition of the theory of dielectrics, founded on Poisson's notion of *couches de glissement*, we come to an interesting discussion of Maxwell's theory of stresses in a dielectric field. By a somewhat different process from that used by Maxwell, the stresses are found for an isotropic field to be a tension along and a pressure across the lines of force of numerical amount $KF^2/8\pi$, where K is the specific inductive capacity, and F is the electric force at the point considered.

On this result Prof. Poincaré remarks that, although it agrees very well with the observed attractions and repulsions between electrified bodies, yet if these attractions and repulsions are to be considered as due to the existence of such stresses in an elastic medium, the laws of elasticity for that medium must be very different from those for ordinary substances. The ideas of electric displacement and electromotive force at a point correspond to the strain and stress in an elastic solid; but, for correspondence to stresses of the value $F^2/8\pi$, it is necessary to find some different forms of displacement or strain than any that have yet been imagined.

A difficulty here arises to which Poincaré attaches considerable importance. The potential energy in the medium is, if f, g, h be the component electric displacements, given by the equation

$$W = \int \frac{2\pi}{K}(f^2 + g^2 + h^2)dv,$$

where dv is an element of volume and the integral is extended through all space. According to Maxwell's hypothesis as to the localization of the energy of the field, the amount contained in an element dv at which the displacements are f, g, h, is

$$\frac{2\pi}{K}(f^2 + g^2 + h^2)dv,$$

or $KF^2dv/8\pi$. Consequently, if F be increased to $F + dF$, there will be an increase in the potential energy of amount $2KFdFdv/8\pi$. If now the stresses act in the medium as ordinary stresses, they must produce corresponding strains in each element of volume. Hence if the element dv be a rectangular parallelepiped of edges δx, δy, δz when the field is free from electric stress, these dimensions will become, when an electromotive force F is produced at the element, respectively $\delta x\,(1 + e_1)$, $\delta y\,(1 + e_2)$, $\delta z\,(1 + e_3)$. Hence, if when F is increased to F + dF, e_1, e_2, e_3 become $e_1 + de_1$, $e_2 + de_2$, $e_3 + de_3$, the work done by the stresses will, neglecting small quantities of the second order, be

$$\frac{KF^2}{8\pi}dv(de_1 - de_2 - de_3);$$

and if the increase of potential energy in the element take place in consequence of the work done against the stresses we get the equation

$$\frac{F^2}{8\pi}dv(de_1 - de_2 - de_3) = \frac{2FdF}{8\pi}dv,$$

or

$$de_1 - de_2 - de_3 = \frac{2dF}{F},$$

which gives by integration

$$e_1 - e_2 - e_3 = 2 \log F + \text{const.}$$

This result is inadmissible, since when F is zero, we must have $e_1 = e_2 = e_3 = 0$, while if this equation holds either e_2 or e_3 is infinite.

A solution of the difficulty is simply that the energy is not really potential but kinetic. It is certainly not easy to see why the electro-magnetic energy should be regarded as kinetic and the electro-static as potential, and it seems more natural to conclude, as all progress in knowledge of matter seems to indicate, that the properties of the medium are wholly due to motion.

After a short sketch of purely magnetic theory, Poincaré proceeds to what must be regarded as the most important part of his account of Maxwell's work—the theory of electro-magnetism. His investigation of the magnetic potentials of circuits is somewhat different from that usually given. Maxwell takes as his starting point here the equivalence of a current-carrying circuit of small dimensions and a magnet. Poincaré bases his method directly on the following three results of experiment: (1) that two parallel currents of equal intensity and of opposite directions in two close conductors exert no action on a magnetic pole at some distance; (2) if one of these currents have small sinuosities, its action on the magnetic pole is still equal and opposite to that of the straight current; and (3) that the magnetic action is proportional to the quantity of electricity which traverses a cross-section of the conductor in the unit of time.

With the assumption that the components of the force acting on a magnetic pole are obtained by partial differentiation of a function which depends only on the relative positions of the pole and the circuit, the usual theorems are obtained in the following elegant manner. First of all it is shown that the potential of a closed plane circuit at any point in its plane is zero. This is first proved for a circuit symmetrical about a line on its own plane and a point on the axis of symmetry. Then by using the first fundamental proposition to introduce across the circuit straight conductors each carrying two equal and opposite currents equal to the current in the circuit, a circuit of any form is divided into narrow portions each bounded at the ends by elements of the circuit, and at its sides by radial lines passing through the point in question. By using then the second proposition to replace each end-element of the circuit by a circular arc passing through the centre of the element and described from the given point as centre, each strip is turned into a complete circuit, symmetrical about a line through the given point. Since, then, the theorem is true for every such circuit, it is true for the whole given circuit which they build up. Next it is easily shown that when a circuit is situated on the surface of a cone but does not surround the axis—that is, is such that a generating line meets the circuit in an even number of points—the potential of the circuit at the vertex of the cone is zero. For, by means of conductors introduced along generating lines, and carrying equal and opposite currents as before, it is possible with the aid of the second result stated above to replace the circuit by a number of narrow plane circuits each carrying the given current, and symmetrical about a generating line of the cone. Hence each element produces zero potential at the vertex, and therefore so also does the given circuit.

Then it is proved that two circuits on the surface of a cone, each passing round the axis, produce equal and opposite potentials at the vertex, if the currents are equal and flow in opposite directions round the cone. For by means of hypothetical conductors introduced as before along the generating lines, and the second fundamental result, these circuits can be converted into narrow plane circuits, each carrying a current and symmetrical about a generating line. Thus the arrangement of two circuits produces no potential at the vertex. It is to be observed that the two circuits subtend equal solid angles at the vertex of the cone, and that the potentials must still be equal and opposite if the circuits surround distinct superposable cones.

Considering now any closed circuit, we can draw a cone from any chosen point as vertex, so that the generators pass through the circuit. Then this cone can be divided into an infinite number of infinitely small superposable cones of equal solid angle, each having a current flowing round it in the same direction as that round the given circuit, and the total potential at the common vertex is the sum of the equal potentials produced by three small circuits—that is, the potential is proportional to the solid angle subtended at the point by the circuit.

The equations connecting the components u, v, w, of currents with the components of magnetic force and magnetic induction, the relations connecting the magnetic force and magnetic induction, those connecting the magnetic force with the vector potential (which Poincaré calls the *moment électromagnétique*), and the value of the components of the latter quantity for a linear circuit with their application to the proof of Neumann's expression for the "electrodynamic potential" (the mutual intrinsic energy) of two linear current-carrying circuits, and the corresponding expressions for the "electrodynamic potentials" (electrokinetic energies) of the circuits themselves, are dealt with in the next two chapters.

In chapter ix. we come to the most important part of the book, the theory of induction, and the treatment of this part of the subject is instructive. It is a result of experiment that if the currents γ_1, γ_2, in two fixed circuits C_1, C_2, respectively, are varied, electromotive forces $Ad\gamma_1/dt + Bd\gamma_2/dt$, $Bd\gamma_1/dt + Cd\gamma_2/dt$ are produced, where B is a coefficient depending on the relative positions of the circuits, A a coefficient depending on C_1 alone, and C a coefficient depending on C_2 alone. Thus if the circuits are deformed or relatively displaced, electromotive forces of amounts $\gamma_1 dA/dt + \gamma_2 dB/dt$, $\gamma_1 dB/dt + \gamma_2 dC/dt$, are produced in C_1 and C_2, so that the total electromotive forces are respectively $d(A\gamma_1 + B\gamma_2)/dt$, and $d(B\gamma_1 + C\gamma_2)dt$. Now by the circuits, in which are supposed to act impressed electromotive forces E_1, E_2, the energy furnished in time dt is $E_1\gamma_1 dt + E_2\gamma_2 dt$. This must be expended in heating the conductors, and in doing *all* the work which is done in the displacement or deformation of the conductors. This latter work is of two parts, (1) that which is done in consequence of the geometrical alteration of the circuits, (2) that which is done in virtue of the change of the current strengths. But the "electrodynamic potential" of the system (Maxwell's electrokinetic energy) is

$$T = \tfrac{1}{2}(L_1\gamma_1^2 + 2M\gamma_1\gamma_2 + L_2\gamma_2^2),$$

so that the former work is

$$\partial T = \tfrac{1}{2}(\gamma_1^2 dL_1 + 2\gamma_1\gamma_2 dM + \gamma_2^2 dL_2).$$

Thus the work dW done in virtue of the changes of the currents is the difference between this and the excess of the energy given out by the batteries over that spent in heat. Thus

$$dW = E_1\gamma_1 dt + E_2\gamma_2 dt - R_1\gamma_1^2 dt - R_2\gamma_2^2 dt - \partial T;$$

and this is the work done in virtue of changes of the currents. This quantity must be a perfect differential, since its integral vanishes for a closed cycle of changes. The condition which must hold for this enables the values of A, B, C to be identified with $-L_1$, $-M$, $-L_2$.

Maxwell's introduction of Lagrange's dynamical method into electro-magnetism is, as has been already stated, regarded by Poincaré as of great importance, and as he says "nous touchons ici à la vraie pensée de Maxwell." After finding by this method the inductive electromotive forces, and the electro-magnetic forces, he proceeds to discuss Maxwell's theorems of the electro-magnetic field, and their crowning generalization, the electro-magnetic theory of light. Except here and there, the treatment differs only in points of detail from that of Maxwell.

With regard to the equations of currents,

$$u = CP + \frac{K}{4\pi}\frac{\partial P}{\partial t},$$
$$\&c., \qquad \&c.,$$

a difficulty is pointed out as to the specific inductive capacity of a conducting substance. For such a substance the first term must preponderate, and so K must be small; whereas K is generally regarded as very great in the case of a conductor. It is worth noticing that this is really only a conventional means of explaining the impossibility of charging a condenser the space between the plates of which is filled with conducting substance; the true explanation is, no doubt, very different.

The discussion of the experimental verifications of the electro-magnetic theory of light contains references to several lately-established experimental facts (apart from Hertz's experiments, which are reserved for special treatment) which bear on the theory. For example, it has been shown by Curie that dielectrics, when tabulated in the order of increasing conductivity, are on the whole arranged (as obviously they should be) in the order of diminishing diathermancy. Further, ebonite, which is opaque to light, is very permeable to dark radiations of longer period, which agrees with its high transparency to electrical waves.

Again, it is remarked that the results of the electro-magnetic theory with regard to reflections from the surface of glass and of metals lend a general support to the theory, while the disagreement in the values of the numerical constants as regards the want of magnetic permeability is referred to the frequency of the vibrations and the fact that the magnetization of the medium is not instantaneously produced.

A marked feature of M. Poincaré's treatise is the chapter on rotatory polarization, in which he discusses the phenomena of rotation of the plane of polarized light by the action of a magnetic field. Although the essential difference between this effect and the apparently similar action of quartz, sugar solutions, &c., is pointed out, the author does not appear to lay stress on it as throwing light on the difference between their causes. For example, after giving Airy's differential equations, for the propagation of the two rectangular component displacements, ξ, η, of a circularly polarized wave travelling along the axis of z, in the form

$$\rho\frac{\partial^2\xi}{\partial t^2} = \frac{\partial^2\xi}{\partial z^2} + a\frac{\partial^3\eta}{\partial z^2\partial t},$$
$$\rho\frac{\partial^2\eta}{\partial t^2} = \frac{\partial^2\eta}{\partial z^2} - a\frac{\partial^3\xi}{\partial z^2\partial t},$$

from which a formula for the rotation of the plane of polarization of plane-polarized light in a magnetic field can be obtained, which agrees with experiment; and after comparing the results of these equations with those of other proposed equations, he says:—

"Mais si le concordance de la formule avec l'expérience justifie l'introduction des derivées $+\,\partial^3\eta/\partial z^2\partial t$, $-\,\partial^3\xi/\partial z^2\partial t$ dans les secondes membres des équations du mouvement d'une molécule d'éther, aucune considération théorique ne préside au choix de ces dérivées à l'exclusion des autres; on ne possédait donc pas encore de théorie de la polarisation rotatoire magnétique."

This certainly seems rather too strong a statement in the face of Thomson's dynamical theory outlined in his "Electrostatics and Magnetism," and further elaborated in Maxwell's treatise.

Thomson's views on this subject are of the most fundamental importance, as they point to motion of, or in, the medium occupying the magnetic field as the cause of the magneto-optic effect discovered by Faraday, and to a certain structure of the substance as producing the phenomena shown by quartz, syrup, &c. One of the most interesting passages of his lectures on molecular dynamics, delivered at Baltimore in 1885, is that in which he accounts for the observed results by the presence of rotating particles, "gyrostatic molecules," in the medium.

It is obviously suggested by the gyrostatic investigation that it ought to be possible to explain the magneto-optic rotation in the electro-magnetic theory of light as a consequence of the presence of small magnets embedded in the vibrating medium with their axes in the direction of the ray; and therefore producing a component of magnetization in that direction. It is stated by M. Poincaré that a theory of this kind has been proposed by M. Potier, and published in the *Comptes Rendus*. The theory itself is not given, but the differential equations obtained are quoted, and are of the required form, and lead to the known experimental result.[1]

Maxwell's molecular vortices theory is, however, given, and certain difficulties which it involves discussed. The theoretical results of Hall's experiment are also given in this connection, and Kerr's experiment proving the production of elliptic polarization by the reflection of plane-polarized light from the pole of a magnet is cited, but without any statement of the theory of the effect which has been worked out, principally by Fitzgerald. With regard to the explanation of the Hall effect by strain of the conducting film produced by the magnetic field, it has always seemed to me that it ought to be possible with proper appliances to decide the question, by experimenting with a sufficiently powerful and uniform magnetic field.

The work, it ought to be stated, concludes with an interesting chapter by the editor, M. Blondin, on experimental verifications of the theories of Maxwell. This comprises the chief determinations of specific inductive capacity, Kerr's classical researches, and lastly, the interesting investigation made by M. Röntgen of the electro-magnetic action of currents of displacement.

Of Prof. Poincaré's second treatise on the experiments of Hertz, &c., I hope shortly to give an account as a sequel to the present article.

A. GRAY.

THE ORIGIN OF THE FLORA OF GREENLAND.

HOW the present flora of Greenland originated, is a question of great interest to British botanists and geologists, for the answer will probably help to solve the difficult problem, What was the origin of the recent flora of Britain? The flora of Greenland is so poor in species and has been so well studied that its relationship to the floras of Europe and America ought not to admit of much debate; yet we find that an active discussion is now going on among Scandinavian botanists as to its eastern or western affinities. Sir J. D. Hooker, in his "Outlines of the Distribution of Arctic Plants,"[1] made a careful analysis of the species found in Greenland, and came to the conclusion that the relationship was more European than American, and this view seems to have been generally adopted by botanists. In a recent official report, contained in the valuable series of memoirs published by the Commission for the Exploration of Greenland,[2] Prof. E. Warming, however, has tried to show that the flora is American; and as this author has had access to fuller materials than were formerly available, his opinion will carry considerable weight. Prof. A. G. Nathorst, a botanist especially competent to speak on questions relating to the botany of the Arctic regions and on the relation of the recent Arctic flora to the Glacial epoch, objects altogether to Prof. Warming's conclusions, and, although dealing with the same materials, maintains the accuracy of the generally accepted view as to the European relationship of the vegetation.[3] He also critically examines the flora in a way that has never been done before, and points to its dependence on bygone conditions. To certain of Prof. Nathorst's observations and conclusions I should like to draw attention.

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[1] M. Poincaré's reference has suggested to me a mode of investigating the action of these magnets on the electro-magnetic theory. This is discussed in a separate article, which contains for the sake of comparison an account of the gyrostatic theory.

The principal result arrived at by Prof. Warming was that the boundary between the American and the European provinces is formed by the Denmark Strait (the strait between Greenland and America), and not by Davis Strait as botanists have generally thought. This conclusion Prof. Nathorst critically examines, and so many curious and suggestive facts relating to geographical distribution come out in this examination that I may be excused for referring to certain of them somewhat in detail. The flowering plants of Greenland include 386 species, none of which are confined to that country. Leaving out of account circumpolar forms, Prof. Warming finds in the list 36 characteristic western against 42 eastern species, but suggests that when the flora of Arctic America is better known the balance will probably be in favour of the western forms. Prof. Warming, however, includes among the eastern plants only those now living in Europe, the Asiatic-American species being classed as western on the ground that they must have entered Greenland from the west rather than from the east—a somewhat unsafe line of reasoning when we take into account former changes of climate and the local extinction of many plants.

Prof. Nathorst analyzes the list differently, and gives most suggestive tables and a map of the local distribution of the eastern and western plants in Greenland. From these we find that the coast nearest to Iceland contains European forms alone, the southern extremity contains European forms in a majority, while the part of the west coast nearest to America yields principally western species; but taking Greenland as a whole the flora is more European than American. Another curious fact noticed by Prof. Nathorst is that the American element of the flora of Greenland is not entirely cut off by the Denmark Strait, but extends eastward as far as Iceland.

Prof. Warming considers that the nucleus of the present flora of Greenland represents part of the original flora, which was able to live through the Glacial epoch on the non-glaciated areas; but Prof. Nathorst points out that the few non-glaciated mountain-tops must have been far too high for any phanerogams to exist on them, and all the lowlands were then covered with ice and snow. We must therefore consider that both eastern and western elements of the present flora of Greenland entered the country in post-glacial times. The tables of distribution show at what points a large number of the plants entered—they came from the nearest land, whether European or American. Whether in post-glacial times there was any complete land-connection between Greenland and either North America or Iceland is very doubtful, but the straits may well have been narrower. The ice-foot, also, which collects in winter beneath the sea-cliffs is placed in the best possible position to receive any seeds or masses of soil which may fall during the winter. This shore-ice is drifted away in the spring, and may easily discharge its burden on some far-distant shore uninjured, and the seeds just ready to germinate. Winds, migrating birds, and migrating mammals would all help to transport seeds across the straits.

Turning now to the British Isles, we know that a prolific temperate flora inhabited this country in pre-glacial times. We know also that this flora disappeared and was replaced by a thoroughly Arctic one, at least as far south as Norfolk, where its relics are found beneath the moraines. Then came a period when Britain north of the Thames was covered with ice and snow, and only an occasional hill-top—or "*nunatak*," as it would be called in Greenland—rose above. When the ice retreated, the Arctic phanerogams again spread over the country, for we find *Salix polaris*, *S. herbacea*, *S. reticulata*, *Betula nana*, and *Loiseleuria procumbens* in lacustrine deposits immediately above the boulder clay near Edinburgh; we have also a similar flora, with *Salix polaris*, *S. myrsinites*, and *Betula nana*, in Suffolk; and even in Devonshire the dwarf birch has been found. This stage, though its flora is still imperfectly known, apparently corresponds closely with the present condition of Greenland.

In Britain, however, we have now reached a later stage in the amelioration of the climate and re-settlement of the country, for the Arctic plants have either disappeared entirely or have retreated to our mountain-tops, and in their place on the lowlands we find a temperate flora now living. The British flora, like that of Greenland, varies according to the botanical character of the nearest land, though, as with Greenland, there is no reason, except the supposed impossibility of the migration of the animals and plants without a bridge, to imagine that during post-glacial times there has been any direct connection with the Continent, save perhaps at the Straits of Dover. The distribution of plants in Britain is so peculiar that I may be forgiven for pointing out to non-botanical readers that we have a southern flora opposite France, a Germanic flora on the east coast, a Lusitanian flora in the south-west, and on the extreme west there are two American plants unknown elsewhere in Europe. In the Britain of the present day I believe that we may study the re-peopling of a country over which everything has been exterminated; and until we have fuller direct evidence of the stages of the process, we may safely accept Greenland and Britain as illustrating the way in which Nature works to fill gaps in the fauna and flora, whether these are caused by changes of climate, by volcanic agency, or the submergence and reappearance of islands.

CLEMENT REID.

THE SUN'S CORONA.

SOME little time ago Dr. Schaeberle, of the Lick Observatory, was good enough to send me the following letter:—

> Allow me to call your special attention to a note of mine in the forthcoming number of the A.S.P. Publications, entitled "Some Physical Phenomena involved in the Mechanical Theory of the Corona." I wish to say that, as far as the connection of this theory with the sun-spot period is concerned, there was not, at any time, any effort on my part to make an agreement with other theories, but the conclusions reached are the legitimate and inevitable results of tracing certain observed phenomena to unexpected explanations. As you will see, the logical outcome

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[1] Trans. Linn. Soc. vol. xxiii., pp. 251-348 (1861); partly reprinted (with additions) in the "Manual of the Natural History . . . of Greenland," &c. (1875).

[2] "Om Grφnlands Vegetation: Meddelelser om Grφnland," Part 12 (1888).

[3] *Engler's botanischen Jahrbüch*, 1891, p. 183.

of the whole matter is that, unconsciously, I have actually furnished important evidence in favour of your meteoric hypothesis.

Sincerely yours,
J. M. Schaeberle.

Some time after the arrival of the letter I received the number of the Publications of the Astronomical Society of the Pacific which contained the article referred to, which I have read with the greatest interest. It has been known for some time that Dr. Schaeberle has been able to reproduce the general appearance presented by the corona by means of mechanical contrivances, and that even the polar rays, which were such a noticeable feature of the eclipse of 1878, as I saw it at Separation, can be, in this way, satisfactorily accounted for.

The point of newest interest, however, is that referred to in Dr. Schaeberle's letter.

Assuming *eruptions* most active in the sun-spot zones, and an initial velocity of 380 miles a second, he obtains the following results:—

(1) All parts of a given unperturbed stream will be in a heliocentric latitude nearly equal to the latitude of the point of ejection.

(2) For a constant ejective force the periodic time t will be the same for all parts of the stream.

(3) The chance of collision of a returning with an outgoing stream varies inversely as the square of the distance of the point of collision from the sun.

(4) Near the sun, therefore, collisions must occur which tend to retard or stop the outgoing streams, resulting in a temporary increase in the heat of the combined colliding masses (causing a consequent increase in the brightness of the corona at such places, and at the same time rendering the coronal detail more confused). This heat will tend to be largely dissipated before such masses fall back into the sun, which they will then reach with comparatively small velocity and low temperature. Unretarded returning streams on striking the sun will tend to greatly raise the temperature at the points of impact: perturbed returning streams could, of course, strike all parts of the sun's surface. Unperturbed returning streams will always fall within the limits of the sun-spot zones.

(5) So long as the incoming streams are very numerous, the outgoing ones will, in a great measure, be stopped, so that, after the interval t, there will be comparatively few returning streams: a direct result of this state of things is to allow free passage for the outgoing streams, which, since there are now but few collisions, results in (1) an apparent diminution in the brightness of the corona, (2) more regular and sharply defined detail, and (3) in general a more uniformly illuminated solar surface might be expected, when there are but few or no returning streams. The periodic character of this intermittent motion can be well illustrated by means of a fine vertical jet of water. The vertical vibratory motion of a light ball, often to be seen in water fountains, is also a good illustration.

(6) If the ejective force is such as to make t about five years, a complete cycle of changes will take place in the time $2t$, and after the same manner as is observed in the sun-spot cycle. It is rather remarkable that the aphelion distance of the streams corresponding to this value of t is nearly the same as Jupiter's distance from the sun; so that the perturbations produced by this planet may have more to do with the regularity of the period than the assumed constant force of ejection. The initial velocity required to just carry a particle from the sun to Jupiter is but little less than a parabolic velocity. For an initial parabolic velocity, Saturn, alone considered, would, on the same hypothesis, cause a complete cycle of less marked changes in twenty years, Uranus in sixty years, and Neptune in one hundred and twenty years. The comparatively insignificant planets inside of the orbit of Jupiter would cause minor variations, corresponding to cycles, which, even for Mars, would be of less than two years' duration.

(7) The chance of the earth passing through one of these outgoing streams, which have a mean latitude of 15°, is less than it is for an incoming perturbed stream.

(8) A phenomenon similar to the observed zodiacal light would result from the projection of many such streams in space, and the observed extent of this light proves that the matter which causes this illumination extends to greater distances from the sun than the earth's distance.

It is evident from the foregoing that the complete statement which is to appear shortly will be looked forward to with interest.

For myself, I am glad to think that the views I put forward in the concluding chapter of my "Chemistry of the Sun" will now be looked at from a new point of view. Time will show what the "falls" which take the first place in my scheme, and the second in Dr. Schaeberle's, really are. Certainly I have seen no cause lately to alter the view I expressed in 1887, that the primary cause of solar disturbance is the *descent* of matter on to the photosphere.

J. Norman Lockyer.

NOTES.

On Monday the Prince of Wales presented the Albert Medal of the Society of Arts to Mr. W. H. Perkin, "for his discovery of the method of obtaining colouring matter from coal tar, a discovery which led to the establishment of a new and important industry, and to the utilization of large quantities of a previously worthless material"; and to Sir Frederick Abel, "in recognition of the manner in which he has promoted several important classes of the arts and manufactures by the application of chemical science, and especially by his researches in the manufacture of iron and steel, and also in acknowledgment of the great services he has rendered to the State in the provision of improved war material and as Chemist of the War Department." The medal awarded to Mr. Perkin was for the year 1890; that to Sir Frederick Abel was for the present year.

We are glad to hear that in consequence of the deputation which waited upon Sir Michael Hicks-Beach on June 5, the Board of Trade have registered the British Institute of Preventive Medicine as a limited liability company, with the omission of the word "limited."

It seems as if the introduction of large engineering views may soon produce a very marked effect upon the future of Egypt. Mr. Willcocks, one of the Inspectors of Irrigation, has communicated an interesting letter to the *Times*, from which we select the following remarks on the engineering importance of Dongola:—"The summer supply of the Nile is lamentably deficient for the existing cotton and sugar-cane crops of Egypt, so that all extensions of these valuable crops are out of the question under existing conditions. The Nile Valley in Nubia is eminently suited for storage of water, but up to the present all projects for storing the muddy flood waters of the Nile below the junctions of the Blue Nile and the Atbara have been condemned, as the construction of solid dams would have resulted in the silting up of the reservoirs themselves. This difficulty has disappeared now that it has been discovered that open dams can be constructed which will allow the muddy flood waters to flow through, and store the clear winter supply for use in summer. The construction of these dams has been rendered possible by the great success of Stoney's patent roller-gates, which can be worked under heads of 70 feet of water on a scale sufficient to pass the full flood supply of the Nile. At any time now Egypt can construct a reservoir in its own territory by building an open dam at the head of the Assouan Cataract. If, however, Egypt were allowed to occupy the Nile Valley as far as Dongola, the reach of the river above the Wady Halfa Cataract would provide the necessary reservoir, and the Philæ immersion difficulty would be at an end. So far the summer supply needed for Egypt proper. If the Soudan itself is to be developed, it will only be necessary to construct solid dams at the heads of the Ripon Falls and Fola Rapids, and thus secure the Victoria and Albert Nyanza Lakes as magnificent reservoirs. These reservoirs would not only secure Egypt and the Soudan from drought, but would also, if provided with open dams, secure Egypt from excessive floods. The White Nile as it leaves the two lakes is a clear stream, so that the silting up of the reservoirs would be out of the question, leaving alone their great size."

WE very cordially congratulate Sir G. B. Airy (the ex-Astronomer-Royal), on the completion of his ninetieth year. A distinguished company assembled at the White House, Greenwich Park, on Saturday last, in honour of the occasion.

PROF. ADALBERT KRUEGER, Director of the Observatory of Kiel, has been appointed Prof. Schönfeld's successor at Bonn.

DR. FELIX has been appointed professor in the University of Leipzig.

THE Council of the Yorkshire College, Leeds, have appointed Mr. V. Perronet Sells, New College, Oxford, to be Extension Lecturer in Science.

A PROJECT is in the air for the erection of an Observatory on Mont Blanc. M. Janssen made an appeal last year for support in this undertaking, and on Monday at the Academy of Sciences he announced that his appeal had been heard. He has obtained the support of M. Bischoffsheim, Prince Roland Bonaparte, Baron Alfred de Rothschild, member of the Academy of Fine Arts, and M. Eiffel.

THE annual meeting of the Institution of Mechanical Engineers was opened on Tuesday at Liverpool.

SANITARY science has, during the last month, lost one of its pioneers, in the person of Dr. John Sutherland, whose record of work in the domain of sanitation since 1848 has been of a marvellous character. In 1848 he entered the public service under the first Board of Health, and continued to be employed under the Home and Foreign Offices till the year 1855. During this time he conducted several special inquiries—notably one into the cholera epidemic of 1848–49, which is even now frequently referred to. He was the head of a commission sent to various foreign countries to inquire into the law and practice of burial. He represented the Foreign Office at the International Conference, held at Paris in 1851–52, for regulating quarantine law. In 1855 he was engaged at the Home Office in bringing into operation the Act for abolishing intramural interments, a task which he had undertaken at the request of Mr. Walpole. He was also doing duty in the reorganized General Board of Health, under the presidency of Sir Benjamin Hall, when, at the request of Lord Palmerston and Lord Panmure, he became the head of the commission sent out to inquire into the sanitary condition of our troops engaged in the Crimean War. He found in Miss Florence Nightingale a devoted coadjutor in regard to the hospitals. Dr. Sutherland took an active part in the preparation of the report of the Royal Commission (of which he was a member) on the sanitary state of the Army, dated 1858, and also of the report of the Royal Commission on the sanitary state of the Army in India, dated May 19, 1863. Both of these were of vast importance to the welfare of our soldiers, and most of the recommendations contained therein have been carried out. One of these was the appointment of the Barrack and Hospital Improvement Commission, with Mr. Sidney Herbert, M.P., as President, and Captain (now Sir Douglas) Galton, Dr. Burrell, of the Army Medical Department, and Dr. Sutherland as members. By this committee every barrack and hospital in the United Kingdom was visited, and its sanitary condition reported upon. Defects were brought to light and remedied, and the health of the troops consequently much improved. Subsequently Dr. Sutherland and Captain Galton visited and made similar reports on the Mediterranean Stations, which at that time included the Ionian Islands. All these reports were presented to Parliament, and a reference to them will show the vastness of the work undertaken. In 1862 the Barrack and Hospital Improvement Commission was reconstituted, and all sanitary reports were submitted to the committee and reviewed by them, and suggestions for improving Indian stations prepared. This continued up to the time of Dr. Sutherland's retirement, on June 30, 1888. In 1865 he again visited Gibraltar and Malta, and made an independent and special report on the outbreak of epidemic cholera at those places. In 1866, Dr. Sutherland, in conjunction with Mr. R. S. Ellis, of the Indian Civil Service, Dr. Joshua Paynter, of the Army Medical Department, and Major (now Lieutenant-General, C.B.) Ewart, R.E., visited Algeria, and reported on the causes of reduced mortality in the French army serving in that country, with a view to seeing what of the conditions in force there would be applicable to Her Majesty's troops serving in India and other warm climates. The value of the recommendations made by him and his colleagues will be better understood by a comparison between the vital statistics of the army prior to the time of the Crimean War and those of the present date than in any other way.

MR. WILLOUGHBY SMITH, who had played an important part in connection with submarine telegraphy, died on July 17. He was born in 1828, and in 1848 entered the service of the Gutta-Percha Company, and superintended the manufacture and laying of the first submarine cable. The *Times* gives the following account of his subsequent career. In 1864 the Gutta-Percha Company became merged in the Telegraph Construction and Maintenance Company, and Mr. Smith remained with the company as chief electrician and manager of the gutta-percha works until his retirement through failing health in 1887. In 1866 he was electrician-in-charge, being on board the *Great Eastern* during the laying of the first successful Atlantic cable, and the recovery and completion of the cable that had been lost the year before. Mr. Smith was President of the Institution of Electrical Engineers in 1883, before which Society, as well as before the Royal Institution, he read many interesting and valuable papers. Amongst these was one on his discovery of the effect of light on the electrical quality of selenium, and another on his researches in volta and magneto-electric induction.

MR. DANIEL MACKINTOSH, F.G.S., died at Birkenhead last week at an advanced age. He was the author of a work on "The Scenery and Geology of England and Wales," and his researches on certain traces of the glacial epoch were well known to geologists. In recognition of his services to geological science, the Geological Society presented him in 1886 with a grant from the Lyell Fund.

MR. EDWARD STANFORD has published a pamphlet on "The Spread of Influenza: its Supposed Relations to Atmospheric Conditions," by the Hon. R. Russell. The following are some of the author's conclusions as to the conditions which give rise to influenza, and permit it to be spread. Influenza is a disease caused by exceedingly minute microbes, arising from extensive areas of marsh or sodden land in Central Asia, China, or Siberian. The minuteness of the microbes or their spores is shown by their easy transmissibility, and the large number of persons capable of being infected by a single case in a large room, most persons probably requiring many virulent organisms to be inhaled in a short time before the resistant power of the blood is overcome. This microbe, like that of cholera, multiplies with great rapidity, and probably soon produces sufficient poison to terminate its career in the body, but not before multitudes of spores or microbes have been given off by the breath. Given the original conditions of rainfall, soil, and high temperature, the certain result is the development of inconceivable multitudes of microbes and spores; one species of these is capable of planting itself and living in the tissue and blood of man, of which the temperature is probably near that to which it has been accustomed under the summer sun in wet and drying ground. The somewhat rare and occasional visitations of influenza may be due to at least two or three causes—first, the occurrence of unusual rainfall and favourable summers; second, the prevalence of air-currents from the drying area towards inhabited places; third, adequate communication between these infected places and the towns of Russia, whence progress is rapid towards Western Europe. The wind has no influence that can be verified in the transportation of influenza. As for the means of prevention, Mr. Russell thinks that measures of disinfection and isolation of the earliest cases, and rules at ports and landing places similar to those employed against cholera, would probably prove of the greatest service. Inland, every locality should isolate and disinfect its first cases.

PROF. LANGLEY, the Director of the Smithsonian Institution, is now in this country. *A propos* of his recent researches, referred to in our last number, we learn that Mr. Maxim is building a "flying machine," with which a series of experiments is contemplated; it is now being constructed at Crayford, and is nearly ready for launching. It will be propelled by a light screw making 2500 revolutions a minute. The motive power (it is reported) is supplied by a petroleum condensing engine weighing eighteen hundred pounds, and capable of raising a forty thousand pound load. The real suspending power will lie in an enormous kite measuring 110 feet long and 40 feet wide.

THE following passage occurs in the Report of the Medical Officer of Health of the parish of St. George, Hanover Square, for the five weeks ending July 4, 1891:—"I have calculated the death-rate of the parish for the past month on the census population of 1881, and not on that of 1891, for the following reasons:—The census population of the parish in 1871 was 89,758, and that in 1881 was 89,573; I have no reason to believe that there was any serious inaccuracy in either of these enumerations, so that the population of the parish was practically stationary during the ten years from 1871 to 1881. The enumerated population in 1891 was only 78,362, showing an apparent decrease of 11,211 (or one-eighth of the population) since 1881. I know of no reason whatever for any such decrease, and do not believe it has taken place. The census was taken of the persons sleeping in the parish on the night of Sunday, April 5, a day which had two serious disadvantages, the first being that it was a Sunday, a day on which many people in this parish are out of town, and the second that it was the Sunday after Easter, and that large numbers of people had not returned to town from their Easter holidays. *I therefore consider that the enumeration of the population of the parish this year is of no value for statistical purposes*, and in estimating the birth-rates and death-rates, shall continue to use the census population of 1881, until a fresh and more correct enumeration shall have been made, which will, I hope, be in 1896." This is rather serious. What have our census authorities to say on the matter?

AN earthquake was experienced at Evansville, Indiana, on the 26th inst. The shock was so great as to create a panic in several places of worship. Considerable damage was done to property. The direction of the oscillations was from north to south.

THE weather prospects in the North-West Provinces seem to be improving. Beneficial rains have commenced to fall, and a famine is therefore less probable than it was. The distress among the ryots is, however, great, and the Government of India has voted a grant of £10,000 for their relief. The following telegram was read by Sir J. Gorst, on Tuesday night, in the House of Commons:—"There is an improvement in agricultural prospects and development of monsoon season. There has been good general rainfall throughout the country, except in part of Madras, the Carnatic, and Upper Burmah, in consequence of which there is no present cause of anxiety in Northern India. Strong monsoon blowing West Coast. More rain imminent in Punjab and Rajpootana, where fodder famine has been arrested by rain. Crop operations in Northern India generally progressing satisfactorily, and there is no present cause for anxiety in North-West Provinces and Oude."

THE Technical Education Committee of the Kent County Council has placed £3000 at the disposal of the South-Eastern Counties Association for the Extension of University Teaching, for courses of lectures suited to agricultural and rural populations in small towns and villages throughout the country.

THE Accademia delle Scienze dell' Istituto di Bologna offers a gold medal of 1000 lire value (about £40), the Aldini Prize, "to the author of a memoir which, based on certain data of chemistry, or physics, or applied mechanics, shall indicate new and really practical systems or new apparatus for prevention or extinction of fires." The memoirs may be manuscripts in Italian, Latin, or French (with inclosed name and motto), or printed matter published between May 11, 1890, and May 10, 1892. In the latter case, the memoir may be in another language than those named, but an Italian translation must be added. The date-limit is May 10, 1892.

THE most recent addition to Prof. Flower's excellent series of specimens illustrative of zoological structure placed in the entrance-hall of the Natural History Museum is a set of nineteen dissections prepared by Mr. G. Ridewood to illustrate the variations in the deep plantar tendons of the bird's foot. With the help of these preparations, the student will have little difficulty in understanding the mysteries of the *flexor longus hallucis* and the *flexor perforans digitorum*, upon which two muscles, as has been shown by Sundeval, Garrod, and Forbes, so much depends in the classification of birds.

IT would seem that the present interest in agricultural instruction comes none too soon. The *Agricultural Gazette* of New South Wales gives an account of a new industry—the export of butter to this country, and adds that the Minister of Mines and Agriculture has approved of the establishment of a travelling dairy to impart instruction to the settlers in relation to it.

THE same number contains articles on the grasses and weeds of the colony, and notes on economic plants and weeds, besides information of what some people consider as of a more "practical" character, touching profitable cows and pigs.

THE utilization of waste products is the order of the day. An interesting article on this subject, in relation to breweries, in the *Brewers' Guardian*, calls attention to the utilization of the carbonic acid gas produced in the fermentation of sugar. "On an average, English beer may be considered to contain 5 per cent. of alcohol, and as, in the fermentation of sugar, the weight of carbonic acid produced is almost the same as that of alcohol (the exact proportions being 48·9 of carbonic acid to 51·1 of alcohol), there must have been 500,000,000 pounds of carbonic acid produced in our breweries. The specific gravity of carbonic acid is 0·1524, and therefore a simple calculation shows that the above weight is equal to 25,000,000,000 gallons —a volume it is almost impossible to realize; such a volume would require a space one mile square and forty yards high to contain it. It is now proposed to utilize the greater portion of this large quantity of carbonic acid. The process by which this is to be done has been tried for some little time past in St. James's Gate (Guinness's) Brewery, Dublin; and Sir Charles A. Cameron has reported very favourably on it. The following are the conclusions at which he arrives after a most careful examination of the process:—(1) An immense quantity of carbonic acid is produced in breweries, and is at present wasted; (2) a large proportion of this gas could be condensed to liquid at a cost not exceeding ½*d.* per pound, but probably less than ¼*d.* per pound; (3) the process of liquefying the gas is successfully carried on at Guinness's Brewery, Dublin; (4) the liquefied gas prepared at Guinness's Brewery is perfectly free from any peculiarity of flavour or odour; (5) the carbonic acid produced at soda-water works costs about 4*d.* per pound; (6) it is safer, and in every way more desirable, to use in beverages carbonic acid derived from a food substance, such as grain, than from mineral sources; (7) the uses of liquid carbonic acid are numerous, important, and increasing."

AMONG the plants shown at the meeting of the Royal Botanic Society on Saturday last was a museum specimen of one which had lately died in the Gardens—a victim to the late severe winter. This was one of several specimens of the East Indian or white mangrove, *Avicennia nivea*, sent to the Gardens by the late Duke of Buckingham when Governor of Madras. For some years past these plants had flourished amazingly, thanks to the near approximation to their natural condition attained by keeping them in a very wet state and watering only with sea-water. Under these circumstances they threw up from the roots a number of offsets, or upright adventitious roots, of from 10 inches to 12 inches high, and half an inch thick. In a space of 2 feet square as many as eighty appeared, looking like so many rakes standing up out of the water, and keeping as near as possible the same height above the surface. The only explanation, so far, has been that offered by the Secretary, Mr. Sowerby. In its native state the trees form a fringe along the sea-shore and estuaries of great tropical rivers, lining the banks with a dense and impenetrable mass of vegetation, pushing itself further and further into the river or sea, and leaving behind the dry land it has reclaimed. In such a position these curious rootlets must be an immense advantage to the plant, enabling it to retain all the *débris* washed to the sides, and at the same time preventing the soil between the roots from being carried away by floods, &c. The plants of this species now growing in the Gardens are the only ones alive in this country.

A MOST interesting report of a journey taken along the frontier of the British Protectorate of Nyassaland by Mr. J. Buchanan, C.M.G., Acting Consul at Nyassa, appears in the *Kew Bulletin* for July.

FROM the *Meteorological Observations at Sydney* for January 1891, just received, we learn that the temperature was 2° higher, the humidity 2·4 less, and the rainfall 0·87 inch greater than that of the same month on an average of the preceding thirty-two years.

THE Indian Government has just issued a "Contents and Index of the first twenty volumes of the Records of the Geological Survey of India, 1868–87." Considering the important work done by this Survey, the index will be of great value to geologists. It consists of 118 pages.

THE pamphlet entitled "A Summary of the Darwinian Theory," which was noticed in a recent issue (July 16, p. 247), has been printed for private distribution. The author, Mr. Pascoe, will supply a copy to any person interested in the subject on application to him at 1 Burlington Road, W.

A NEW and cheaper edition of the translation of vol. i. of Weismann's "Essays upon Heredity and Kindred Biological Problems" is announced for immediate publication by the Clarendon Press; and we understand that vol. ii. is in the press, and will consist of four additional essays, and a preface by Prof. Weismann.

Petermann's Mitteilungen for July contains an article on Zante, with an original map, based on the English Admiralty chart, by Prof. Partsch.

AN official notice has been issued concerning the charitable foundation instituted by the Sisters Froelich at Vienna for subsidizing persons distinguished in science, art, or literature. Pensions and donations are to be granted to duly approved applicants. Applications should be addressed to the Trustees (das Curatorium), and transmitted to the President's office of the Common Council of the City of Vienna (an das Präsidialbüreau des Wiener Gemeinderathes Neues Rathaus) before August 31, 1891, through the I. and K. Austro-Hungarian Embassy in London, 18 Belgrave Square, S.W., where particulars of the terms and conditions of the foundation deeds, &c., can be obtained.

FURTHER details concerning the new volatile compound, iron carbonyl, $Fe(CO)_4$, are published by Messrs. Mond and Quincke in the current number of the *Berichte*. It appears that as early as November last year they succeeded in volatilizing small quantities of iron in a stream of carbon monoxide, and recovering it again in the form of a metallic mirror by passing the gaseous product thro heated tube. The best results are given when the iron is obtained by reduction of ferrous oxalate in a stream of hydrogen at as low a temperature as possible, very little exceeding 400° C., and allowing to cool in the stream of hydrogen to 80°. When carbon monoxide is led over the finely divided iron thus obtained, the issuing vapours are found to colour a Bunsen burner pale yellow; and if they are passed through a glass tube heated to a temperature between 200° and 350°, a mirror of metallic iron is deposited. If the tube is heated to a temperature superior to 350°, instead of a mirror a black flocculent deposit is obtained, containing carbon in addition to iron. The metallic mirror dissolves readily in dilute acids, and the solutions give all the reactions of iron. A quantitative analysis was made of one such mirror, and yielded almost theoretical numbers for pure iron. The black flocculent deposit was found in two cases to contain 79·30 and 52·78 per cent. of carbon respectively. The reaction, however, proceeds only very slowly. To give some idea of this, Messrs. Mond and Quincke state that after six weeks continued treatment of twelve grams of iron with carbon monoxide only about two grams had been volatilized. As the action becomes very slight indeed after treatment for some hours, the operation was interrupted at the end of every five or six hours, and the iron reheated to 400° in a stream of hydrogen, after which the reaction proceeded again as at first. It is calculated that the average amount volatilized was about two cubic centimetres per litre of carbon monoxide. This great dilution has of course rendered it very difficult to ascertain the composition and properties of the substance. Its composition has, however, been determined by absorbing the vapour obtained during eight to sixteen hours in mineral oil of boiling-point 250°–300°, which after numerous experiments has been found to be the best solvent for it, and heating the solution thus obtained to 180°, when it becomes black owing to the separation of metallic iron, and carbon monoxide is evolved. Determinations of the amount of separated iron and the volume of carbon monoxide obtained in five such experiments gave for the proportion of molecules of CO to one atom of iron the numbers 4·14, 4·03, 4·15, 4·26, and 4·04 respectively. Hence there can be very little doubt that the compound is represented by the formula $Fe(CO)_4$, analogous to the nickel compound obtained last year, $Ni(CO)_4$. As regards the relation of the compound to the processes of iron and cementation steel manufacture, the authors are of opinion that, although they have been unable to prepare it at temperatures between 150° and 750°, still it is quite possible that it may be momentarily formed at such temperatures, but again immediately dissociated.

The additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. G. Stevenson Macfarlane; a White-fronted Capuchin (*Cebus albifrons*) from South America, presented by the Earl of Carnarvon; a Silver-backed Fox (*Canis chama* ♂) from South Africa, presented by Mr. Max Michaelis; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Mr. J. Smalman Smith; two Rough Foxes (*Canis rudis*) from British Guiana, presented by Mr. G. H. Hawtayne, C.M.Z.S.; two Pennsylvanian Buzzards (*Buteo pennsylvanicus*) from North America, presented by Sir Walter Hely Hutchinson; a Barn Owl (*Strix flammea*), British, presented by Mr. E. Hart, F.Z.S.; a Tigrine Cat (*Felis tigrina*), two Spotted Cavies (*Cœlogenys paca*), a White-lipped Peccary (*Dicotyles labiatus*), a Red and Yellow Macaw (*Ara chloroptera*), a Blue and Yellow Macaw (*Ara ararauna*), two Orange-winged Amazons (*Chrysotis amazonica*), two West Indian Rails (*Aramides cayennensis*), a Martinique Gallinule (*Ionornis martinicus*) from South America, a Golden Agouti (*Dasyprocta aguti*), three Crested Curassows (*Crax alector*) from Guiana, a Hawk-headed Parrot (*Deroptyus accipitrinus*), a Common Trumpeter (*Psophia crepitans*) from Demerara, deposited; an Azara's Agouti (*Dasyprocta azaræ*) from South Brazil, purchased.

OUR ASTRONOMICAL COLUMN.

Observations of Sun-spots and Faculæ.—*Comptes rendus* for July 13 contains the results of observations of sun-spots and faculæ, made by M. Marchand, at Lyons Observatory, during the first six months of this year. The following table expresses, in millionths of the sun's visible hemisphere, the surface covered by spots and faculæ during this period.

1891.	Surface covered by spots.	Surface covered by faculæ.
January ...	385 ...	12·5
February ...	503 ...	20·7
March ...	265 ...	15·9
April ...	726 ...	25·4
May ...	670 ...	22·1
June ...	968 ...	29·7
Total ...	3517	126·3

These figures demonstrate the increase in solar activity which must have been noted by all observers. The total spotted area of 3517 millionths is made up by 65 groups. During the whole of 1890 the spotted area given by 43 groups was only 3760 millionths. Since the end of March not a single day has passed without a spot being seen on the sun. With regard to distribution, 40 groups have appeared in the northern hemisphere as against 25 in the south. These occurred most frequently between the latitudes ± 20° and ± 30°. At the same time 22 groups have had latitudes between 10° and 20° (with four groups below 15°), thus indicating an approach to the equator.

The measures of faculæ give similar results. The two zones from 20° to 30° are the richest, and those from 0° to 10° the poorest. The total numbers are sensibly the same in both hemispheres. There is, however, a slight superiority in relative number in the northern hemisphere, but less marked than during 1890. The total surface covered by faculæ in 1890 was 103·3 millionths of the sun's visible disk, so that the figures now given indicate a considerable augmentation. It is also worthy of note that the results obtained for spots and faculæ show a certain parallelism, secondary minima in March and in May occurring in each case.

Stars having Peculiar Spectra.—In a communication to *Astronomische Nachrichten*, No. 3049, Prof. Pickering notes that a Group II. star situated in Sagittarius (R.A. 19h. 51·8m., Decl. − 42° 7′, 1900), having exhibited bright hydrogen lines in its photographic spectrum, was suspected of the variability of which this appearance is a characteristic. Measures of photographs of the star taken on different dates proved that the supposition was a correct one, and indicated a variation between the magnitudes 9·1 and 13·1.

The photographic spectrum of the star S.D. − 12°·1172 (R.A. 5h. 22·9m., Decl. − 12° 46′), mag. 9·2, appears to be the same as that of a planetary nebula as regards the positions of lines, but it differs in the interesting fact that the H_β hydrogen line (F) is unusually strong in comparison with the nebula line at λ 500.

Two more stars having spectra mainly consisting of bright lines, like the three stars in Cygnus discovered by Wolf and Rayet, have been discovered. They are Cord. G. C., 15·934h. (R.A. 15h. 15·9m., Decl. − 62° 20′, 1900), and a faint star in the position R.A. 13h. 36·3m., Decl. − 66° 55′ (1900). The number of stars of the Wolf-Rayet type is thus brought up to thirty-five.

THE INSTITUTION OF NAVAL ARCHITECTS

The first London summer meeting of the Institution of Naval Architects was held on Thursday, Friday, and Saturday of last week. During the thirty-one years that the Institution has existed, it has only held five summer meetings. The first of these was in Glasgow, and was highly successful, but it was not followed by another summer meeting until the year 1886, when the attractions of the Liverpool Exhibition were sufficient to cause the Council to arrange a second meeting for that year in the second city of the kingdom. The Newcastle and Glasgow Exhibitions followed in the two succeeding years, and the members accordingly were summoned to the banks of the Tyne and Clyde. All these meetings were successful in every respect, not only in adding to the membership of the Institution, but in the valuable papers contributed to the Transactions, and the interest of the various excursions. In spite of this, no summer meeting was held either in 1889 or 1890, in which years there were but the single three days' meeting in the spring. That has been conclusively proved not to be sufficient time for the conduct of the business of the year; and at the last spring meeting it was announced that in future two meetings would be held every year—the first to be the usual spring meeting, which always takes place in London, and the second to be held in the summer, either in London or elsewhere. The success of the meeting just held strongly supports the wisdom of this decision.

There was naturally not so long a list of papers on the programme as there is at the spring meeting, for allowance had to be made for the excursions. With the latter we are compelled to deal very briefly on account of pressure on our space, and we will therefore say a few words upon them at once, before proceeding to notice the papers. On the first day, Thursday, the 23rd inst., the afternoon was devoted to the Royal Naval Exhibition, and in the evening there was a dinner, at which Lord Brassey presided, the absence of the President, Lord Ravensworth, being caused by a domestic sorrow. On the Friday afternoon the excursion was to the shipyard of Samuda Brothers, at Poplar, and to the Thames Ironworks at Blackwall. The P. and O. Company also gave a luncheon, in the Albert Docks, on board the *Carthage*. At Samudas' the two second-class cruisers H.M.SS. *Sappho* and *Scylla* are in course of construction, and give quite a welcome air of bustle and activity to the Poplar yard, not long since a scene of what many thought to be permanent stagnation. These ships are 3400 tons each, and 9000 indicated horse-power. A large amount of armour-plate bending and machinery is now going on in this yard, and the machine tools were examined with much interest by many of those members to whom such work was new. At the Thames Ironworks there are also two ships in progress for the Royal Navy. These are the cruisers *Grafton* and *Theseus*. The latter name brings up stirring memories of another noble ship built in years past at Blackwall. The new steel *Theseus* is, however, a very different craft from Nelson's old flag-ship. She and her sister-vessel the *Grafton* are each of 7350 tons displacement, and have engines which will develop 12,000 indicated horse-power. Saturday was devoted wholly to a single excursion, the members travelling down to Chatham by train, and going over the Dockyard. Mr. Yarrow had kindly arranged to send one of his first-class torpedo boats down to Chatham, so that those who wished to return to London by water were enabled to do so. The three great engineering firms, Penns, Maudslays, and Humphrys, also threw open their works to the inspection of members during the meeting.

We will now proceed to deal briefly with the proceedings at the two morning sittings of Thursday and Friday, during which six papers were read and discussed, of which the following is a list:—Ships of war, by Sir Nathaniel Barnaby, K.C.B.; on the alterations in the types and proportions of mercantile vessels, together with recent improvements in their construction and depth of loading, as affecting their safety at sea, by B. Martell, Chief Surveyor of Lloyd's Register of Shipping; centre and wing ballast tank suctions in double-bottom vessels, by G. R. Brace; some notes on the history, progress, and recent practice in marine engineering, by A. J. Durston, Engineer-in-Chief to the Royal Navy; progress in engineering in the mercantile marine, by A. E. Seaton; on the weak points of steamers carrying oil in bulk, and the type which experience has shown most suitable for this purpose, by George Eldridge.

On the meeting being opened, Lord Ravensworth, the President of the Institution, who occupied the chair, proceeded to deliver a short address, and then presented the gold medal of the Institution to Prof. Lewes for his paper on "Boiler Deposits," read at the last meeting. The gold medal is not given to members of Council, so that some of the papers read at the spring meeting were out of the competition. Sir Nathaniel Barnaby's paper brought forward some of the most salient features in the history of war-ship design during the thirty-one years which have elapsed since the Institution was founded. An interesting fact noticed was that our earliest armour-clad, the *Warrior*, and our latest, the *Ramillies*, were of exactly the same length—380 feet. There, however, the likeness ends, for the modern ship is 14,150 tons displacement as compared with 9210 tons of the *Warrior*. Her horse-power is 13,000 indicated, the *Warrior's* being 5270; her speed is seventeen and a half knots against the *Warrior's* fourteen and a half knots; her armour is 18 inches thick, whilst the *Warrior's* was 4½ inches thick; her coal endurance is 5000 knots as against the *Warrior's* 1210 knots; her weight of broadside is 5500 pounds, as against the *Warrior's* 1918 pounds. These figures well illustrate the progress made in the science of war-ship construction, and the advance also extends to less desirable elements; for the cost of the hull and engines alone of the eight first-class battle-ships of the *Ramillies* class, now in course of completion, is £875,000 apiece, whilst the *Warrior* cost £357,000. It may be of interest to our readers if we add that the cost of a first-class battle-ship at the beginning of the century was about £70,000. The addition of machinery and other improvements brought the cost of the 121-gun screw three-deckers, which followed the Crimean War, up to close upon a quarter of a million. The armour alone of the *Ramillies* has cost exactly the same amount as the Natural History Museum at South Kensington. Bearing these facts in mind, it will be interesting to remember that Lord Brassey has laid down, in the programme of shipbuilding he would propose for the next five years, the number of first class battle-ships as ten; in addition to six armoured coast defence vessels, six armoured rams, forty cruisers of the first class, thirty look-out ships, and fifty torpedo gun-vessels. Nothing is said about the smaller torpedo *boats*, although a first-class torpedo boat costs nearly as much as a forty-gun frigate of Nelson's day. Some of our best naval authorities are, however, not so moderate as Lord Brassey; and Admiral Sir John Hay said, during the discussion on Sir Nathaniel Barnaby's paper, that he would have fourteen line-of-battle ships in place of Lord Brassey's ten. Vast as are the sums involved in the carrying out of such a programme as this, they are not so great, compared to the corresponding expenditure of foreign Powers in terms of the value of the commerce which the ships produced would have to protect. Admiral Sir Edward Freemantle, Lord Brassey, Sir John Hay, Mr. Wigham Richardson, the Director of Naval Construction (Mr. W. H. White), Sir Edward Reed, and others, spoke in the discussion, which was of a long and interesting description.

Mr. Martell's paper described the progress of that part of naval architectural design which bears more particularly on the construction of cargo steamers. The author traced the process of evolution by which the early steamers, naturally modelled after the sailing ships which they succeeded, gave place to later types, which in their turn were displaced by others found to be more suitable to the needs of the time. Mr. Martell dealt largely with the well-decked type upon which so many of the modern "ocean tramps" are modelled. The working of the Load-line Act was also considered by the author. One of the most interesting parts of the paper is the few paragraphs the author devotes to sailing ships. A few years ago it was freely prophesied that the days of masts and sails were past; that, so soon as the then existing vessels were worn out, wind-propelled craft would be confined to the yachtsman's sport. From the number of handsome sailing ships that were lying idle in nearly every port, the prognostication seemed warranted. Even the fishing boats seemed doomed by the multiplication of steam trawlers. Happily for the picturesque aspect of the mariner's craft, these forecasts have not been fulfilled. "Notwithstanding the great economy introduced by the triple-expansion engine," Mr. Martell tells us, "the tonnage of sailing vessels built has yet been well maintained in both 1889 and 1890." Vessels carrying 6000 tons of dead weight, with four masts, both ship and barque rigged, have recently been built; and arrangements have recently been made for the construction of a sailing ship, with five masts, to carry 7000 tons dead weight. This vessel is, however, to have a propelling engine fitted aft, but this engine is to be strictly auxiliary, to be used only in case of calms, and to enable the ship to dispense with the use of tugs. If such an arrangement can be conveniently made, and we see no insuperable difficulties, probably there will be a great future for vessels of this class pending the development of coal supplies in various parts of the world. Probably the boiler will take the form of some water tube type yet to be perfected, as quickness in raising steam is a great desideratum for such purposes. An elaborate table of vessels lost during the last ten years is added as an appendix to the paper. A short discussion followed the reading.

Mr. Brace's paper dealt exclusively with the detail of ship construction set forth in the title. As it took exception to Lloyd's rules, Mr. Martell naturally criticized it with considerable severity.

The sitting of Friday, the 24th inst., commenced with Mr. Durston's paper, which afforded a most interesting contribution to the history of the marine engine. The author takes the engine models in the Naval Exhibition for his text, and on them founds a monograph on the evolution of the marine engine as applied to war-ships from the days of the *Monkey*, the first steam-propelled vessel in the Navy. The *Monkey* was built at Rotherhithe in 1820, and was 210 tons. She was engined in the same year by Boulton and Watt with paddle-wheel engines of 80 nominal horse-power. It would take too much space to follow Mr. Durston in his description of the subsequent development of the branch of the naval service of which he is now the chief; and with which the names of Penn, Maudslay, Rennie, Seaward, Napier, Elder, and others are so intimately woven in the early, and most of them, happily, in later days. There is added to the paper a table giving particulars of 52 ships of the Royal Navy, commencing with the *Acheron*—having beam, paddle-wheel engines, and flue boilers, pressed to 4·5 pounds per square inch, the machinery being by Seaward—and coming down to the present day. The table is of the greatest value, and we cannot refrain from giving some details from it, even at the risk of extending this notice to undue length. The *Acheron*, of 293 actual horse-power, gave 2·2 units of power[1] per ton weight of machinery, the piston speed being 198 feet per minute. It required 10·74 cubic feet of boiler to give one indicated horse-power. The heating surface per indicated horse-power was 5·25 square feet, and the horse-power per square foot of grate was 3·1. The coal consumption is unknown. We will make a jump of 31 years, because that brings us to the first ship in the table of which the coal consumption is recorded. The ship we select is the *Hercules*, built in 1869, and engined by Penn with trunk engines of 8529 indicated horse-power, and, of course, a screw propeller. The boilers here were of the old rectangular or box tubular type, pressed to 30 pounds per square inch. The piston speed had then steadily risen in somewhat the same ratio as the boiler pressure, so that with the *Hercules* it had reached to the respectable figure 643 feet per minute. The indicated horse-power per ton of machinery had also reached 7·5. The capacity of boilers per indicated horse-power was 2·17 cubic feet, the heating surface per indicated horse-power 2·6 square feet, the horse-power per square foot of grate 9·41 units, and the coal consumption per indicated horse-power per hour 2·811 pounds. Looking back over the twenty-two years that have elapsed since the *Hercules* was tried, and remembering the stringent and limiting conditions under which war-ship engines were then designed, one cannot but be struck by the remarkably successful results attained with the engines of the *Hercules*. No doubt this was due to the extraordinary pains taken in the design and manufacture of the engines of Her Majesty's ships in those days. The introduction of more complex machine tools in the workshop has enabled much of this minute care and finish to be dispensed with, and the advances in metallurgical science have put improved materials at the command of the engineer; but nothing has yet exceeded, or, we believe, ever will exceed, the beauty and accuracy of the noble examples of the mechanic's art constructed at the Greenwich shops under the direction of that prince of engineers, the late John Penn. At the same time we gladly acknowledge that the general average of all engines has immensely advanced, and is still advancing, both in design, material, and finish. The whole of these three qualities are due to a wider spread of that knowledge of scientific principles upon which the mechanical arts are founded. The manual skill of the handicraftsman has not increased; on the contrary, it has deteriorated as mechanical contrivances have superseded the old hand operations.

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[1] Unit of power = 1 indicated horse power.

From this digression we will return to the table in Mr. Durston's paper, and take one more example. This shall be the *Royal Oak*, a sister of the *Ramillies* before mentioned, and one of the eight monster line-of-battle ships now in progress—the biggest war-ships ever yet designed. Laird Brothers, of Birkenhead, are the contractors for the *Royal Oak*. She has the vertical triple compound engines and ordinary return tube boilers of the present day. The indicated horse-power is put down at 13,000,[1] but will doubtless be much more, the steam pressure being 155 pounds per square inch, and the piston speed 918 feet per minute. The indicated horse-power per ton of machinery is 11·75 units, the capacity of boilers per indicated horse-power 1·06 cubic feet, the heating surface per indicated horse-power 1·55 square feet, and the horse-power per square foot of grate 18·31 units. The coal consumption remains, until the trials are made, a matter of conjecture, but there is every reason to anticipate it will approximate to that of the best performances recorded for Her Majesty's ships—namely, about 2 pounds of fuel per hour per indicated horse-power developed with natural draught. In taking this figure, however, we are somewhat unfair to the earlier engines, for we have taken the other performances of the *Royal Oak's* engines on forced draught, a condition under which the fuel consumption would be much higher. What may be the fuel consumption of Her Majesty's ships under forced draught we have no means of knowing. It should be remembered that, in the Royal Navy, the steam generated in the main boilers is used for the many auxiliary engines also, but the indicated horse-power of the main engines only is taken. This manifestly puts the engines of Her Majesty's ships at a considerable disadvantage in the matter of fuel economy when comparison is made with mercantile engines. If we had to summarize the lessons taught by Mr. Durston's tables in few words, we should say the stepping-stones to advance in marine engineering have been multi-tube boilers, compound surface-condensing engines, and forced draught. The latter is still in that state of popular disfavour which seems to be the natural condition of all innovations on established practice; but it will yet make its mark, and lead engine-designers to higher results, whilst it will drive them to more perfect work.

Mr. Seaton is well known as one of our best marine engineers, and is, moreover, a skilled writer, with a special talent for communicating his ideas through the medium of the pen. That is well proved by his contributions both in the shape of memoirs to technical Societies and also by his well-known work on the marine engine. Unfortunately for the literary side of his reputation he is the manager of one of the largest shipbuilding and engineering establishments in the country, and there are evidences of this in the paper he contributed to the meeting. It was intended to be a counterpart, from the mercantile point of view, of Mr. Durston's naval paper. Mr. Seaton was doubtless anxious to fulfil his promse to contribute to the proceedings, and has evidently done the best time would allow. His paper is a good illustration that "there is always plenty of room at the top," in the engineering, as in all other professions; but it does not call for any extended notice here. The same thing may be said of Mr. Eldridge's paper, which dealt minutely with technical details. It is, however, a distinctly valuable contribution to the Transactions of the Institution, and may be studied with advantage by all naval architects who may have to design steamers for carrying petroleum in bulk—vessels that are fast growing in importance and numbers.

The meeting terminated with the usual votes of thanks.

SEVENTH INTERNATIONAL CONGRESS OF HYGIENE AND DEMOGRAPHY.

THE arrangements for this Congress—which will be opened by the President, H.R.H. the Prince of Wales, on Monday, August 10, at the first general meeting at St. James's Hall, when short addresses will be given by some eminent foreign hygienists—are now in a very complete state.

We may mention that the previous Congresses were held in Brussels, Paris, Turin, Geneva, The Hague, and Vienna, at the last of which it was resolved, on the invitation of the Sanitary Institute and the Society of Medical Officers of Health, that the next Congress of the series should be held in London in the present year.

Besides the Permanent International Committee, to which a number of additional members have been attached for the purpose of this Congress, the executive consists of an Organizing Committee, with Sir Douglas Galton as Chairman; a Reception Committee, with Sir Spencer Wells as Chairman, and Mr. Malcolm Morris as Honorary Secretary; and a Finance Committee, with Surgeon-General Cornish as Chairman, and Dr. Moline as Secretary. There is also a numerous Indian Committee, with Mr. S. Digby as Honorary Secretary; and an Editing Committee. Prof. Corfield, whose address at The Hague Congress in 1884 was the origin of the present one (see NATURE, vol. xliii. p. 511) is the Honorary Foreign Secretary of the Congress, and Dr. G. V. Poore the Honorary Secretary-General.

The Congress is divided into nine Sections under Hygiene, and one under Demography, which includes Industrial Hygiene, and deals with the life conditions of communities from statistical points of view. The Hygienic Sections will meet in Burlington House and in the University of London. They are as follows:—

(1) Preventive Medicine. President, Sir Joseph Fayrer, K.C.S.I.

(2) Bacteriology. President, Sir Joseph Lister, Bart.

(3) The Relations of the Diseases of Animals to those of Man. President, Sir Nigel Kingscote, K.C.B.

(4) Infancy, Childhood, and School Life. President, Mr. J. R. Diggle, Chairman of the London School Board

(5) Chemistry and Physics in Relation to Hygiene. President, Sir Henry Roscoe, M.P.

(6) Architecture in Relation to Hygiene. President, Sir Arthur W. Blomfield.

(7) Engineering in Relation to Hygiene. President, Sir John Coode, K.C.M.G.

(8) Naval and Military Hygiene. President, Lord Wantage, K.C.B., V.C.

(9) State Hygiene. President, Lord Basing.

The Demographic Division will meet in the theatre of the Royal School of Mines in Jermyn Street, under the presidency of Mr. Francis Galton.

A large number of papers are promised, some on subjects selected by the officers of the Sections, and some on other subjects; indeed, there is such a profusion of papers that it seems very doubtful whether it will be possible to deal with them all during the four days available for the purpose, especially as we are informed that most if not all of the Sections will only sit from 10 a.m. to 2 p.m.

A vast number of delegates have been appointed from institutions and public bodies in this country. Delegates have been appointed by the Governments of all the European and several other countries, and also by many foreign Universities, cities, public institutions, and scientific societies. There are also a number of delegates from India and the colonies.

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[1] The indicated horse-power of the *Sardegnea*, the big Italian war-vessel, is estimated to be 22,000. This is the largest power yet designed for any ship. There are four sets of engines, two for each propeller.

An Honorary Foreign Council, including the names of most of the best known foreign hygienists, has been appointed, and also an Honorary Council of the British Empire, with representatives from India and the colonies.

A Bacteriological Museum and Laboratory will be a special feature in connection with the work of the second Section; and an exhibition of drawings of sanitary construction, in connection with the work of the sixth Section, will be arranged in the Library of the University of London, under the direction of Mr. Thomas W. Cutler.

As is usual in gatherings of this kind, a considerable number of entertainments, excursions, &c., have been arranged for, including an entertainment at the Guildhall, *conversaziones* at the Royal Colleges of Physicians and of Surgeons, and a dinner and *fête* at the Crystal Palace.

A Ladies' Committee, under the presidency of Mrs. Priestley, has also been formed for the purpose of holding receptions and of organizing visits to various places of interest for the benefit of the ladies who may take this opportunity of visiting London.

A daily programme will be issued, giving the titles of the papers to be read, and the list of excursions, entertainments, &c., for each day; and besides this, *Public Health*, the journal of the Society of Medical Officers of Health (under the editorship of Mr. A. Wynter Blyth) will issue a special daily number during the Congress, giving abstracts of the more important papers in each Section.

A volume of abstracts of papers will also be issued, and a special hand-book for London is being prepared by Messrs. Cassell and Co. in French and English; this will contain several maps and plans, and will be mainly devoted to those matters which have a special interest for members of a Congress of Hygiene and Demography.

After the Congress a volume of Transactions will be published, to a copy of which each member will be entitled. The subscription is £1, and the offices are at 20 Hanover Square.

THE ORIGIN OF CERTAIN MARBLES.[1]

AMONGST the interesting collection of rocks brought home by Prof. Haddon from Torres Straits are some fragments of wind-blown coral-sand rock from Thursday Island. They have a deceptively oolitic appearance, and the majority of the grains being of a red colour give a prevailing warm tint to the stone, and thus render more conspicuous by contrast a number of dark green, worn, and rounded crystals of augite, which are scattered irregularly through it. The appearance of this handsome rock is sufficiently striking, but it gains greatly in interest from its suggestive resemblance to the famous Tiree marble, wherein likewise green grains of pyroxene are set in a flesh-coloured matrix of altered limestone. The comparison is confirmed and enhanced by an examination of thin slices; in the recent limestone the calcareous grains are found, as so commonly happens with these coral-sand rocks, to consist of rounded fragments of calcareous Algæ, and worn tests of various species of Foraminifera; mingled with these are more or less rounded crystals, not only of green augite, but also of olivine, felspar, and a finely crystalline glassy basalt; in the Tiree marble the green grains of pyroxene (salite) show beautifully rounded outlines, and are sharply separated from the surrounding matrix, into which they show no tendency to pass; crystals of felspar are also present—some fairly fresh, others, and these are the majority, corroded and almost entirely replaced by calcite, only the thin outer skin of the felspar preserving a fresh appearance; in some few cases, fragments of felspar partially penetrated by salite are met with. The calcareous matrix is finely granular, possibly dolomitic, but blotched and spotted by badly defined larger crystalline individuals of calcite, the outlines of which are sometimes obscurely rounded, so that although no trace of organic structure can now be recognized, yet on the whole the appearances are such as might be expected to be presented by a coral-sand rock, which had suffered metamorphic changes. Macculloch, in his detailed account of this rock, refers to its occurrence as an irregular mass, completely surrounded by gneiss; another white limestone occurs in the island, similarly disposed.

It is interesting to speculate on the final result of pressure metamorphism, acting on volcanic islands surrounded by their reefs. Thus, were the ancient granite masses of Queensland and New Guinea to approach one another, moving towards the line of weakness which now forms Torres Straits, we may conceive that basic schists in great variety would arise from the rolling out of the cores and superficial deposits of the intervening volcanoes; while the associated coral reefs would be converted into irregular masses of structureless limestone, and becoming involved in the surrounding schists would be irregularly dispersed through them, so as to occur in unexpected and anomalous positions.

In conclusion we would call attention to an important paper, read in 1876, by Mr. W. L. Green, Minister of Foreign Affairs to the King of the Sandwich Islands (footnote, Journ. Roy. Geol. Soc. Ireland, vol. iv. p. 140, 1877). *Inter alia*, he says:—

"The Hawaiian Islands are more or less surrounded by coral reefs, the island of Hawaii less so than the others, for one reason, because the lava has kept pouring into the sea along most parts of the coast during past centuries, and has not given the coral an opportunity to form to so large an extent as in the other islands. Now it is a fact that wherever the lava runs into the sea, or wherever the waves have an opportunity of breaking against [it], . . . a large quantity of olivine sand is formed. The felspar, the other material of which this lava is mainly composed, gets ground up to powder and disappears—indeed, it is almost always in the minutest grains to begin with; whilst the olivine, a much heavier mineral, and in grains from the size of a bean to a pea downwards, forms the main component of the sand of the seashore wherever the sea meets the lava, or else the olivine-sand gets more or less mixed up with the coral-sand, where the two classes of rock are in close proximity. A great deal of the olivine-sand is of the finest possible quality; indeed, it is often so fine that although a much heavier mineral than carbonate of lime, it will often, where both are washed by the waves, settle on the top of the coral-sand, and I have often scraped the almost pure fine olivine-sand from the top of a coral-sand beach. This mixture of the two sands is common over the group, extending 400 miles from Hawaii to Bird Island." Again, " . . . there is every grade of mixture from all coral to all olivine. Very often the olivine-sand rock will be found to run in streaks amongst the coral-sand rock, so that in the course of time, when the coral-sand rock comes to be metamorphosed into a limestone or a marble, the olivine-sand rock would probably suffer the change which that mineral is well known to experience—namely, into serpentine."

These views will certainly commend themselves to many of those who have come to regard Eozoon as a mineral structure. With the presumption in its calcareous composition of an organic origin, there has always existed a suspicion that some such explanation as this might eventually be found. It is interesting to note that the streakiness which Mr. Green expressly mentions as characterizing the interlamination of the olivine and coral sand, is so frequently an accompaniment of "Eozoonal" and serpentinous limestone.

IS THE MARINER'S COMPASS A CHINESE INVENTION?

A WRITER in the *North China Herald* of Shanghai devotes a learned article to detailing and discussing the facts regarding the claim of the Chinese to have invented the mariner's compass. They did not learn the properties of the magnetized needle from any other country. They found it out for themselves, though it is impossible to point to the man by name who first observed that a magnetized needle points north and south. He suggests that it came about in this way. The Chinese have in their country boundless tracts of ironstone, and among these no small portion is magnetic. Every woman needs a needle, and iron early took the place of the old stone needles, and were commonly used before the time of Ch'in Shih-huang—that is, more than twenty-one centuries ago. Whenever a needle happened to be made of magnetic iron, it might reveal its quality by falling into a cup of water, when it happened to be attached to a splinter of wood, for example. It came in some such way to be known commonly that certain needles had this quality. The great producing centre for magnetic iron is T'szchou, in Southern Chihli. This city was very early called the City of Mercy, and the magnetic stone produced there came to be known as the stone of T'szchou, and so *t'szshih* became the ordinary name for a magnet. Later, the Chinese began to speak of the City as the "City of the Magnet," instead of calling it the "City of

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[1] A Suggestion: by Profs. Sollas and Cole.

Mercy." The polarity of the magnetic needle would become known to the Chinese of that city and its neighbourhood first. The first who noticed the polarity would be some intelligent person who communicated the fact as an unaccountable peculiarity in an age when omens and portents were diligently sought for in every natural object and phenomenon.

The earliest author who mentions the "south-pointing needle" lived in the fourth century B.C. There can be no reasonable doubt that the polarity of the needle was known at that time. The discovery of the fact must have preceded the invention of any myth embracing it. As to the discovery, there is no reason to suppose it was in any way foreign, because the Chinese use an enormous number of needles, and have an inexhaustible supply of ironstone. But though the polarity was known, it was not turned to a practical use till the Tsin dynasty, when landscapes began to be studied by the professors of *fengshui*, or geomancy. There was at that time a general belief in the magical powers of natural objects. This was a Buddhist doctrine, and it took firm hold on the Chinese mind of that age. The Chinese philosophers of those times taught that indications of good and ill luck are to be seen all through Nature. The polarity of the needle would take its place in this category of thought. Though it is not distinctly mentioned by writers of the fourth century, yet to their disciples it became an essential part of the landscape compass which the professors of *fengshui* all use. Kwo Pu, the founder of this system, died A.D. 324, and it was not till four centuries later that the *fengshui* compass began to assume its present form. The compass used by the professors of geomancy for marking landscape indications was first made about the eighth century. It was of hard wood about a foot wide, and it had in the centre a small well in which a magnetized needle floated on water. On the compass were inscribed several concentric circles, as on the wooden horizon of our globes. They embrace the twelve double hours, the ten denary symbols, eight diagrams, and other marks. This compass was used in preparing a geomantic report of any spot where a house or tomb was to be constructed, so that the construction might not be upon an unlucky site or planned in an unlucky manner. At the same time there was living a Chinese who had studied Hindoo astronomy, and was the Imperial astronomer, and also a Buddhist priest. He noticed that the needle did not point exactly north, and that there was a variation of 2° 95′. This variation went on increasing till a century later—that is, till the ninth century. A professor of geomancy then added a new circle to the compass. On this improved compass the first of the twelve hours begins on the new circle at 7½° east of north.

The compass, it will be observed, grew out of the old astrological report or nativity paper, calculated from the position of the stars, and prepared in the Han dynasty by astrologers as a regular part of social life, especially when marriages were about to be solemnized. Some of the old astronomical circles are preserved in the new geomantic chart. This was the compass used when Shen-kwa wrote on the south-pointing needle in the eleventh century. This author mentions that any iron needle acquires polarity by rubbing it on a piece of loadstone. He alludes to the variation as a fact which he himself had observed, and speaks of the south-pointing needle as an implement used by the professors of geomancy. By them it was employed in the form of a float upon water. After this, in 1122, an ambassador to Corea describes the use of the floating needle on board ship while he made the voyage. This is the first instance, the earliest by more than a century, of the use of the mariner's compass on board ship, found as yet in any book, native or foreign. The existence of the book in which this is recorded settles the question of the first use of the mariner's compass at sea in favour of the Chinese. At that time the needle floated on water supported on a piece of wood, but in the Ming dynasty some Japanese junks engaged in piracy were captured by the Chinese, and the compass in use on board was found to have the needle dry and raised on a pivot, while still pointing southward. The Japanese had learned from the Portuguese navigators to make a compass of this kind, and probably the needles they used were brought from Europe. From this time, the Chinese adopted the principle of a pivot, and made their compasses without a well of water in the middle to float the needle in. Charts were probably used of a very rough kind, but how far is not known. What is known is that the junk-master was aware of the direction in which the needle must point to reach the port to which he was going. In the Sung dynasty, embracing part of the tenth, as well as the eleventh, twelfth, and part of the thirteenth centuries, Chinese junks went to Persia and India. The Arabs trading to China directly would learn at that time the use of the compass, and would apply it on board their dhows. From them the Europeans learned this useful invention.

The credit of the discovery, both of the polarity of a magnetized needle and its suitability for use by mariners at sea must therefore, according to this writer, be given to the Chinese. It was China also that has the credit of having first noticed that any iron needle may be polarized by rubbing it with a magnet. In the thirteenth century the Arabs used a floating compass on their dhows. The needle was made to float on the water by attaching it crosswise to a cornstick or splinter of wood. A magnet applied to it drew it into a north and south direction. They would use Western notation to mark the quarters and intermediate points on the horizon. When therefore the mariner's compass was adopted from them, the Chinese 24 points were not communicated. In the European compass the notation of 32 points is Western, and rests on the winds and the sun. In the Chinese primitive mariner's compass the notation is that of the professors of geomancy, and rests on the old astrological division of the horizon into twelve double hours. From the Arab account we learn, what the Chinese accounts do not tell us, that the Chinese floated the needle by inserting it in a splinter of wood.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

ROYAL COLLEGE OF SCIENCE.—The following scholarships, prizes, and Associateships have been awarded for the session 1890–91:—First year's scholarships to William Allan, Thomas T. Bedford, Edwin Edser, and Herbert A. Clark; second year's scholarships to John W. Pickles and Sydney Whalley; the Edward Forbes Medal and prize of books for biology to Arthur G. Butler; the Murchison Medal and prize of books for geology to Charles G. Cullis; a Tyndall prize of books for physics, Course I., to William Allan; the De la Beche Medal for mining to James G. Lawn; the Bessemer Medal and prize of books for metallurgy to Joseph Jefferson; the Frank Hatton prizes of books for chemistry to Herbert Grime and Lionel M. Jones. Prizes of books have been given by the Department of Science and Art in the following subjects:—Mechanics—Charles H. Kilby, Charles P. Butler, Herbert A. Clark. Astronomical Physics—Lawrence Parry and Samuel S. Richardson. Practical Chemistry—William A. C. Rogers. Mining—James G. Lawn. Principles of Agriculture and Agricultural Chemistry—Henry Wilkinson. Associateships of the Royal College of Science have been awarded as follows:—Mechanics—1st class, Harold Busbridge and Ernest W. Rees; 2nd class, Angus Leitch. Physics—1st class, Sidney Wood; 2nd class, William Shackleton and Alfred B. Lishman. Chemistry—1st class, Herbert Grime, Lionel M. Jones, Alfred Greeves, William A. C. Rogers, and Morton Ware; 2nd class, John G. Saltmarsh. Biology (Zoology)—1st class, Arthur G. Butler and James Harrison. Geology—1st class, William J. Smeeth. The following Associateships, Royal School of Mines, have also been awarded:—Metallurgy—1st class, Joseph Jefferson, Alfred Stansfield, John Eustice, and William F. P. Tindall; 2nd class, John D. Crabtree, Thomas S. Fraser, Henry T. Bolton, Benjamin Young, Hugh F. Kirkpatrick-Picard, George J. Snelus, James R. Crum, and Stanley H. Ford. Mining—1st class, James G. Lawn, John Yates, Robert Pill, Theodore G. Chambers, Algernon P. Del Mar, Nono Kitto, and George R. Thompson; 2nd class, Reginald Pawle, Charles C. Scott, Henry Cavendish, Gustave Busch, George H. Gough, and Ben Howe.

SCIENTIFIC SERIALS.

THE *American Meteorological Journal* for June contains:—An account of the meeting of the New England Meteorological Society on April 18 last. The subject of discussion was weather predicting. The general methods of predicting in the United States and Europe were first described, and afterwards local and long-range predictions were considered. Papers were read by J. Warren Smith, on the Signal Service weather forecasts; W. M. Davis, on European weather predictions; A. L. Rotch, on the local weather predictions of the Blue Hill Observatory; M. W. Harrington, on weather prediction in the States and its improvement, together with several other similar papers.—The zodiacal light as related to the aurora, by O. T. Sherman. The author gives tables and curves constructed from a large number of observations, showing (1) the relative elongation of the zodiacal light, from observations taken in March, from 1801–86; (2) corrections to the earth's calculated longitude, being that part of the amount by which the observed position varied from the calculated, which is probably due to zodiacal light; (3) Fritz's auroral numbers for Europe south of the polar circle; and (4) his relative numbers for Europe. The conclusions drawn from the tables are that from 1806–27 there was no observation of the zodiacal light, slight and irregular variation of the earth's motion, and slight and irregular auroras. For the next fifty years each period of elongation of the zodiacal light corresponded with a maximum acceleration of the earth's motion, and a minimum in the aurora. And further, that at the time when the zodiacal light was beyond the earth's orbit, the auroras were few and diminished in number.—Farwell's rainfall scheme. This article (which is unsigned) states that Senator Farwell carried a Bill through the last session of Congress, for testing the possibility of the artificial production of rain by means of explosions. The experiments, which are soon to be tried, are intrusted to the Agricultural Department; the officials, however, are said to have little confidence in the success of the experiment. Mr. Fernow, Chief of the Division of Forestry, gives a long report upon the proposal, together with a summary of the literature of the subject.

American Journal of Science, July.—The solar corona, an instance of the Newtonian potential function in the case of repulsion, by Prof. Frank H. Bigelow. This is a continuation of the author's researches into the laws which regulate the development of the various coronal forms.—Newtonite and rectorite, two new minerals of the kaolinite group, by R. N. Brackett and J. Francis Williams. Taking the composition of kaolin as $Al_2O_3,2SiO_2,2H_2O$, the following series of hydrous silicates of alumina may be derived by eliminating or introducing a molecule of water:—

	Percentage Composition.		
	Al_2O_3	SiO_2	H_2O
(1) $Al_2O_3, 2SiO_2, H_2O$ ...	42·52 ...	49·99 ...	7·49
(2) $Al_2O_3, 2SiO_2, 2H_2O$ ...	39·57 ...	46·56 ...	13·93
(3) $Al_2O_3, 2SiO_2, 3H_2O$ ...	36·98 ...	43·47 ...	19·55
(4) $Al_2O_3, 2SiO_2, 4H_2O$ ...	34·72 ...	40·82 ...	24·46

From the facts and considerations stated in the present paper it appears probable that three members are known out of the four in the above series, viz. (1) rectorite, (2) kaolin and members of the kaolinite group, and (4) newtonite.—On the intensity of sound; ii. the energy used by organ-pipes, by Charles K. Wead. From the results of experiments with different organ-stops out, it appears that no exact conclusion can be drawn from the loudness of the sound as to the relative quantity of wind required to blow pipes of different construction; thus, the soft Dulciana stop of the organ upon which the experiments were performed took more than half as much wind as the comparatively loud Open Diapason, whilst the pipes of the Trumpet stop required less energy than any others sounding the same note. The results obtained in the case of different pipes of the same stop indicate that the volume of air used per second, and therefore the energy expended per second, varies as the ¾-power of the wave-length of the note, or inversely as the ¾-power of the vibration-ratio.—New analyses of astrophyllite and tscheffkinite, by L. E. Eakins. The analyses give $R''_4R'_4Si(SiO_4)_4$ as the general formula for astrophyllite. This agrees with that found by Brögger from a discussion of analyses by Bäckström and König. Tscheffkinite does not appear to be a mineral in any strict construction of the word, but merely a mixture.—The minerals in hollow spherulites of rhyolite from Glade Creek, Wyoming, by J. P. Iddings and S. L. Penfield. The authors find that in the rhyolite investigated fayalite occurs in association with abundant quartz of a peculiar development, as the result of the mineralizing action of vapours in the cooling acid lava. In certain hollow spherulites the fayalite is replaced by hornblende and biotite.—Bernardinite: is it a mineral or a fungus?, by Joseph Stanley Brown. From Mr. Brown's examination it appears that the mineral resin from San Bernardino County, California, described by Prof. Stillman in the *American Journal* twelve years ago, is the fungus *Polyporus officinalis*, Fries.—Development of Bilobites, by Dr. Charles E. Beecher.—Gmelinite from Nova Scotia, by Louis V. Pirsson. The optical characters, cleavage, and chemical composition of this mineral have been studied. The result of the crystallographic work points to a distinct difference between it and chabazite, but with regard to twinning and chemical constitution the two appear to be identical. Indeed, gmelinite seems to bear much the same relation to chabazite that enstatite does to hypersthene.—Analyses of *kamacite*, *tænite*, and plessite from the Welland meteoric iron, by John M. Davidson. The conclusion is arrived at that in the Welland siderolite only two distinct nickel-iron alloys occur, viz. *kamacite* and *tænite*, and that the so-called plessite is merely thin alternating lamellæ of the two.

American Journal of Mathematics, vol. xiii., No. 4.—In this number J. Perrott's "Remarque au sujet du théorème d'Euclide sur l'infinité du nombre des nombres premiers" is continued from No. 3, and concluded; the author promising a further article on "L'application du procédé du géomètre grec à d'autres cas de la proposition de Lejeune Dirichlet."—The following papers also appear:—Ether squirts, by Karl Pearson, an attempt to specialize the form of ether motion which forms an atom. The main portion of the paper is devoted to an investigation of inter-atomic and inter-molecular forces.—On the matrix which represents a vector, by C. H. Chapman. The fundamental idea is that the linear and vector function of a vector is simply the matrix of the third order.—Sur une forme nouvelle de l'équation modulaire du huitième dégré, par F. Brioschi.—The index to vol. xiii. is appended to this number, which concludes it.

SOCIETIES AND ACADEMIES.

EDINBURGH.

Royal Society, July 6.—The Hon. Lord McLaren, Vice-President, in the chair.—Mr. John Aitken read a paper on the solid and liquid particles in clouds (see p. 279, July 23).—Prof. Tait communicated a paper by Prof. Chrystal on a demonstration of Lagrange's rule for the solution of the linear partial differential equation, with some historical remarks on defective demonstrations hitherto current. Prof. Chrystal's proof is purely analytical. Prof. Tait remarked that, on quaternionic principles, the problem may be regarded as follows. Let the equation be

$$Pp + Qq = R,$$

where P, Q, and R, are given functions of x, y, and z, and p, q, represent respectively the quantities dz/dx, dz/dy. By the introduction of a new variable, u, this may be put into the form

$$P\frac{du}{dx} + Q\frac{du}{dy} + R\frac{du}{dz} = 0.$$

But du/dx, du/dy, du/dz, are proportional to the direction cosines of the normal to the surface $u = c$, and therefore P, Q, R are proportional to the direction cosines of a tangent line to $u = c$. Hence we deduce, as the equations of a curve which lies wholly on the surface,

$$\frac{dx}{P} = \frac{dy}{Q} = \frac{dz}{R}.$$

The integrals of these equations are known to have the form $v = \alpha$, $w = \beta$, where α and β are arbitrary constants. The intersections of these surfaces fill space with a set of lines, and the problem is to find a single general set of surfaces upon which these lines will lie. Their equation is $v = f(w)$, where f is an arbitrary function. It is therefore the integral of the given differential equation.—Prof. Tait read the fifth part of his paper on the foundations of the kinetic theory of gases. He has applied his expressson for the isothermals of a liquid and its vapour to the case of ethyl oxide. The results are in remarkable accordance with the direct observations of Drs. Ramsay and Young. He has also applied the virial method to systems of doublets, triplets, &c. The close correspondence of the results calculated from his formula with Andrews's and Amagat's observations on carbonic acid was somewhat surprising when it was considered that the theoretical results were deduced on the assumption of smooth, hard, spherical molecules, while the molecule of carbonic acid is very complex. In the present part of his paper, Prof. Tait shows that, from the manner in which the (approximate) virial equation is formed, no term depending on internal actions in molecules themselves can appear in it when the number of molecules is sufficiently large. He also discusses the mechanism of equilibrium between liquid and saturated vapour. He has reduced the difficulties of the problem to the evaluation of certain definite integrals.—Dr. John Murray communicated a paper by Mr. J. W. Gregory, of the British Museum, on the Maltese fossil Echinoidea, and their evidence on the correlation of the Maltese rocks. In this paper the fossil Echinoidea of Malta are revised, and many additions to the fauna made by the description of material recently collected. Several genera new to Malta are recorded, and also some species previously known only in Italy. Some changes in nomenclature are advocated: thus, as the author accepts the zoological use of the generic name *Echinanthus*, a new one—*Breynella*—is proposed for the genus known to palæontologists by the former term. In regard to the age of the Maltese beds, the author agrees with Fuchs as to the Lower Coralline limestone being clearly Oligocene; the overlying Globigerina limestone is assigned partly to the Aquitanian and partly to the Langhien: as no sharp line of division can be drawn between these two series, the exact limits of the Oligocene and the Miocene in Malta cannot be precisely determined. The blue clay appears also to belong to the Langhien, and to be hardly entitled to separation from the underlying Globigerina limestone; the greensand is referred to the Helvetian, and the Upper Coralline limestone to the Tortonian. The relations of Echinoid faunas of the different horizons to those of the corresponding beds in other parts of the Mediterranean are considered, and it is argued that deep-sea conditions prevailed in different areas at different times: hence they show merely a series of local subsidences, instead of one great regional depression.—Prof. Ewart communicated the first part of a paper on the lateral sense-organs of *Læmargus* and *Acanthias*, in which he dealt specially with the sensory canals.—Prof. Tait communicated a paper, by Prof. C. G. Knott, on the electric resistance of cobalt at high temperatures. The cobalt on which Prof. Knott experimented was in the form of a thin strip cut from a sheet in the possession of Prof. Tait. The metal was very pure—containing possibly 1 per cent. of carbon, 0·15 per cent. of silicon, 0·73 per cent. of iron, a very small percentage of manganese, and perhaps 0·1 per cent. of an undetermined metal. The formula $r = a\epsilon^{kt}$, where r is the resistance and t is the temperature, closely represents the results at temperatures above 100° C. This law is identical with that which holds in the case of nickel, but the rate of variation is not so great in cobalt as it is in nickel. When first heated to a very high temperature, profound changes take place in the metal as regards its change of resistance with temperature. The metal resembles nickel and iron in that the rate of variation of its resistance increases rapidly as the temperature rises. But, in nickel and iron, at a still higher temperature, this is followed by a distinct decrease. No such effect is observed in cobalt.—Prof. Tait also read a paper, by the same author, on the thermo-electric positions of cobalt and bismuth. A triple junction of cobalt, bismuth, and palladium was used. A rod of bismuth was formed by breaking the metal into small pieces, and packing them into a siphon-shaped glass tube. Gentle heating fused the pieces, and so a solid rod was formed. The other wires were fused into its ends. The line of this specimen of cobalt, on the thermo-electric diagram, lay, at ordinary temperatures, above that of the specimen of nickel which Prof. Tait used in the construction of the diagram, but a neutral point existed at 100°, because of the greater steepness of the cobalt line. The slope of the line is the greatest which has yet been observed, with the exception of that of the upwardly-sloping portion of the line of nickel. The thermo-electric power of bismuth does not alter in strong magnetic fields, although Righi has shown that its resistance alters in such fields.

Sydney.

Royal Society of New South Wales, May 13.—Civil Engineering Section Meeting.—Mr. C. W. Darley in the chair.—The inaugural address was delivered by the Chairman; and a paper read on researches in iron and steel, and working stresses in structures, by Prof. Warren.

June 3.—Mr. W. A. Dixon, Vice-President, in the chair.—Six new members were elected.—The following paper was read:—Notes on the large death-rate among Australian sheep in country affected with Cumberland disease or splenic fever, by M. Adrien Loir, Director of the Pasteur Institute of Australia.—Prof. Anderson-Stuart exhibited his new instrument for demonstrating the manner in which sound-waves are propagated; and Lovibond's tintometer was shown by the Chairman.

Paris.

Academy of Sciences, July 20.—M. Duchartre in the chair.—The life and works of the late Prof. W. Weber, by M. Mascart.—Observations of minor planets, made with the great meridian instrument of Paris Observatory during the second half of 1890 and the first quarter of 1891, by Admiral Mouchez. The asteroids which have been observed for position are:

㊸, (78), (68), (53), ㊾, ㉔, (134), ⑩, ⑪, ⑥, ㉘,
(58), (118).

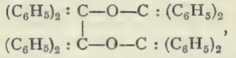
—The third meeting of the International Committee of the map of the heavens: presentation of the Proceedings, by the same author.—Elements of the elliptic comets Swift (1889 VI.) and Spitalier (1890 VII.), by Dr. J. R. Hind.—Evidences that Europe and America have been united during recent times, by M. Emile Blanchard. The evidences given in the author's memoir are derived from a discussion of the fauna and flora of the two continents.—On the glycolysis of circulating blood in living tissues, by MM. R. Lépine and Barral. The authors' method of studying the glycolysis of blood in circulation in an isolated member appears to be more exact than that of studying it *in vitro*. They have used it to prove the diminution of hæmatic glycolysis that occurs in experimental diabetes.—Apparent total disappearance of Jupiter's satellites, by M. C. Flammarion. On July 15, M. Flammarion observed Jupiter when three of his satellites were passing across his disk and one behind it. This rare phenomenon occurs every twenty-three years, a period which contains 523 revolutions of the fourth satellite, 1220 of the third, 2488 of the second, and 4934 of the first. It was first put on record by Galileo in 1611, and M. Flammarion gives a list of seven other observers who have noted it.—Experiments on weirs, by M. H. Bazin.—Vibration of a wire along which an electric current is passing, by M. D. Hurmuzescu. A metallic wire stretched between two supports and traversed by an electric current sets itself in vibration. The amplitude of the vibrations steadily increases and reaches a maximum, which is maintained so long as the current is passing and no changes occur in the conditions of the surrounding medium. For a given tension, the amplitude appears to depend on the difference of temperature of the wire and the medium in which it vibrates; hence it varies as the intensity of the current.—The absorption and photography of colours, by M. Labatut. Using M. Lippmann's method for the photography of the spectrum in its colours, the author has investigated the absorbing action of screens coloured with dyes, such as cyanin, &c., in relation to the parts of the spectrum impressed on the prepared plate and the interference colours produced.—On the composition of atmospheric air: new gravimetric method, by M. A. Leduc. The following represents the results obtained in two experiments:—

	Grms.	Grms.
Weight of air analysed	3·4237	... 3·5551
Weight of oxygen which combined with phosphorus	0·7958	... 0·8249
Percentage proportion of oxygen	23·244	... 23·203

The mean of these values is 23·224, or, roughly, 23·23, which may therefore be taken to represent the percentage of oxygen in purified air. The composition by volume is stated as: nitrogen 78·98 per cent., and oxygen 21·02 per cent.—On silicon selenide, by M. Paul Sabatier. This body has been prepared by passing a current of dry hydrogen selenide over crystallized silicon at a red heat. The selenide obtained is a hard substance, having a semi-metallic appearance, and apparently not volatile at the temperature of the experiment. Its composition, verified by several analyses, is represented by the formula $SiSe_2$.—Melting-point of certain organic binary systems (hydrocarbons), by M. Léo Vignon.—Study of the solid products resulting from the oxidation of drying oils, by M. A. Livache.—On a new method of testing for phenol, by M. L. Carré.—On ozone considered from a physiological and therapeutical point of view, by MM. D. Labbé and Oudin.—On the mode of action of the butyric ferment in the transformation of starch into dextrine, by M. A. Villiers.—On a toxalbumin secreted by a microbe from blennorhagic pus, by MM. Hugounenq and Eraud.—Oscillations of the retina, by M. A. Charpentier. The author has studied experimentally certain phenomena which appear to demonstrate the production of oscillations in the visual organ under the influence of luminous excitations. These movements are apparently due to a reaction of the retina at the moment when light strikes it.—On the innervation of the stomach of Batrachians, by M. Ch. Contejean.—On the development of the mesoderm of Crustaceæ, and on that of its derived organs, by M. Louis Roule.—On the homology of the pedal and cephalic appendices of Annelidæ, by M. A. Malaquin.—On the muscardine of the white worm, by MM. Prillieux and Delacroix.

Brussels.

Academy of Sciences, May 5.—M. Plateau in the chair.—*Linamarine*, a new glucoside from *Linum Usitatissimum*, yielding hydrogen cyanide on hydrolysis, by A. Jorissen and E. Hairs. The method of preparation found to give the best yield is described. This glucoside presents some points of resemblance with amygdalin, but the table of properties discloses many important differences, notably the solubility of the new body in cold water, its melting at 134° *without decomposition*, and the absence of benzaldehyde from the products of its hydrolysis. The elementary analysis of the new glucoside gives the following figures: C, 47·88; H, 6·68; N, 5·55; O, 39·89.—On the pinacone of desoxybenzoïn, by M. Delacre. The author shows that there are two bodies of the formula $C_{28}H_{26}O_2$ obtained by the reduction of desoxybenzoïn, one consisting of glassy needles melting at 210°, and the other obtained in large crystals melting at 163°. He explains the discordance of the results of MM. Limpricht and Schwanert and M. Zagumenny as being due to the former having obtained the mixed bodies, and hence determined the melting point at 156°.—On the constitution of α-benzopinacoline, by M. Delacre. The author gives a complete chemical and physical study of the properties of this body; he concludes that α-benzopinacoline is not a pinacoline but the ether of benzopinacone, and that its constitution would be expressed by the formula



$$\begin{array}{l}(C_6H_5)_2 : C—O—C : (C_6H_5)_2 \\ \qquad\qquad\;\; | \\ (C_6H_5)_2 : C—O—C : (C_6H_5)_2\end{array},$$

thus making its molecular weight double that he previously assigned to β-benzopinacoline. The data given in the paper for the determination of the molecular weight of the α-benzopinacoline by the cryoscopic and vapour tension methods would lead to the adoption of the same molecular weight as in the case of the β-benzopinacoline.—On the rate of formation of compound ethers, by N. Menschutkin. A study of the velocity of etherification of some thirty-two alcoholic derivatives, comprising primary and secondary saturated alcohols, tertiary alcohols, primary unsaturated alcohols, alkyl chlorides, alkyl cyanides, and ethers. Acetic anhydride was employed as etherifying agent, as by its use no water was produced, and thus the complication of the problem by the introduction of reversible reactions was avoided. The velocity of etherification of methyl alcohol is the greatest; the substitution of any element or group of elements for hydrogen in the molecule CH_3OH invariably decreases the velocity of the reaction.—Theorems on the curvature of algebraical curves, by Prof. Cl. Servais.—On the "attractive spheres" in some vegetable cells, by E. de Wildeman.—Crystallographic note on albite from Revin, by M. A. Franck.

Cracow.

Academy of Sciences, May.—On the expansion and compressibility of atmospheric air, by A. W. Witkowski. The author has made experiments with air between the temperatures 100° and − 145° C., and at pressures up to 130 atmospheres. The coefficient of expansion (α) has been found at the constant temperatures 100°, 16°, − 35°, − 78°·5, − 103°·5, − 130°, − 135°, − 140°, and − 145°, by varying the pressure. The values obtained for these nine isothermals are tabulated and represented graphically. From the isothermal curves it appears that the coefficient of expansion increases up to a maximum in each case, and then diminishes. The increase is most rapid near the liquefaction points. All the curves tend towards a point the co-ordinates of which are $p = 1$ atmosphere, and $\alpha = 0·00367$. The values expressing the compressibility of air have been calculated from the expansion coefficient.—An electrical thermometer for low temperatures, by the same author. The fact utilized in the construction of the instrument is the variation of the resistance of a platinum wire at different temperatures. From the experiments it appears that this is about 2 ohms per degree. It is therefore easy to obtain a sensibility of $\frac{1}{40}$ of a Centigrade degree. The relation between the temperature and the electrical resistance is subject to slight variations if the thermometer is employed for widely different temperatures. This fact has been noted by previous experimenters.—On derivatives of *m*-methyl-*o*-uramidobenzoyls, by S. Niementowski.—On the critical pressure of hydrogens, by K. Olszewski.—Mathematical notions and methods, by S. Dickstein.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Réunion du Comité International Permanent pour l'Exécution de la Carte Photographique du Ciel (Paris, Gauthier-Villars).—Solutions of the Examples in Charles Smith's Elementary Algebra: A. G. Cracknell (Macmillan and Co.).—The Right Hand: Left-Handedness: Sir D. Wilson (Macmillan and Co.).—The Positive Theory of Capital: E. V. Böhm-Bawerk, translated by W. Smart (Macmillan and Co.).—Outside the Class-room: Thoughts for Young Engineers: W. H. Bailey (Manchester, Cornish).—The Skeleton of the Irish Giant, Cornelius Magrath; D. J. Cunningham (Williams and Norgate).—Die Jährliche Parallaxe des Sterns Oeltzen 11677: Dr. J. Franz (Königsberg).—The Photochronograph and its Application to Star Transits (Washington).—The Geological and Natural History Survey of Minnesota, Eighteenth Annual Report, 1889: N. H. Winchell (Minn.).—The Iron Ores of Minnesota: N. H. Winchell and H. V. Winchell (Minn.).—Thirty-eighth Report of the Department of Science and Art (Eyre and Spottiswoode).—Abbildungen zur Deutschen Flora: H. Karsten's (Berlin, Friedländer).—Anatomie des Hundes: Dr. W. Ellenberger and Dr. H. Baum (Berlin, P. Parey).—The Telescope: J. W. Williams (Sonnenschein).—Les Engrais Chimiques: Tome Premier, Les Principes et la Théorie: M. G. Ville (Paris).—An Explanation of the Constitution of the Ether, of the Constitution of Matter, and of the Cause of Universal Gravitation: J. G. Vine (Reeves).—Peabody Institute of the City of Baltimore, Twenty-Fourth Annual Report, June 4, 1891 (Baltimore.)—Proceedings of the Boston Society of Natural History, vol. xxv., Part 1 (Boston).—Notes from the Leyden Museum, vol. xiii., No. 2 (Leyden, Brill).—Contributions from the U.S. National Herbarium, vol. i., No. 4 (Washington).

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THURSDAY, AUGUST 6, 1891.

A PHYSICIST ON COLOUR-VISION.

Colour-Measurement and Mixture. By Captain Abney. (London: The Society for the Promotion of Christian Knowledge, 1891).

THIS interesting little book extends over only 200 pages, but is full of careful and important observations. It is, in fact, a summary of the results arrived at by the author during his careful and laborious investigation of the properties of the spectrum. It forms one of the "Romance of Science" series published by the S.P.C.K., a series intended "to show that science has for the masses as great an interest as, and more edification than, the romances of the day." Now, though the earlier portion of this book could be understood by anyone, we venture to think that the second half is for the most part so technical that the full meaning and value could only be appreciated by those who are more or less conversant with the methods of experimenting on colour. To those who are familiar to even a slight extent with the technicalities of colour experiments, the characteristic of the book is its extreme lucidity. We are carried on from point to point, until, when we look back on the closed book, we find we have travelled over the greater number of the problems of colour-vision almost without effort. It is a book which will not appeal to the masses, but should be read by every physiologist and physicist interested in colour-vision.

There is yet another reason for the interest which attaches to this work, necessitating a fuller notice than if it were simply a popular disquisition on colour. It is the record of a careful series of experiments by an eminent physicist, firmly convinced of the truth of the Young-Helmholtz theory of colour-vision. The voluminous work of Hering and his pupils is not once mentioned throughout the whole book, although König's later publications receive due notice. In fact, if space permitted, we cannot imagine a book more calculated to form the basis of a fruitful discussion on the merits of the rival theories than that now before us. For both the problems of colour-vision, and their solution according to the Young-Helmholtz theory, are definitely and clearly stated.

The book opens with a description of the methods used to obtain a spectrum, and a consideration of its properties with especial reference to the ultra-red and ultra-violet rays. The apparatus used by the author to investigate the three fundamental properties of colour—hue, luminosity, and purity—are described in detail. Absorption and interference are then touched upon in their relation to colour, and experiments are given to show that the colour of a body is due to its refusal to transmit or reflect certain rays of the spectrum. This is followed by an interesting chapter on scattered light, with especial reference to atmospheric effects, and a pretty lecture-room experiment is described to show that the change in the colour of the sun when on the horizon is produced by small particles in the air.

The author then passes on to consider the second property of colour—luminosity; and the luminosity of the spectral colours is measured as follows. The light from a certain portion of the spectrum passes through a slit which cuts off the remainder of the spectrum. A portion of the same white beam which was decomposed by the prisms is reflected on to the same screen as the monochromatic beam, and an upright rod is interposed. This rod throws two shadows, whose intensity is compared after the manner of a photometer. The luminosity of the whole reflected beam is greater than that of the coloured beam, and a rotating diaphragm, with variable sectors, is therefore interposed in its course. By altering the size of the sectors, the intensity of the white light is diminished, until the luminosity of the shadow it casts is equal to that cast by the monochromatic beam. The luminosity is then read off in terms of the segment of the circular diaphragm which remains open when the luminosity of the two shadows is equalized. The luminosity of all the principal points in the spectrum is measured on this plan. Subsequently the luminosity of a combination of red and green is shown to be equal to the sum of the luminosities of the same red and green determined separately. Three colours, A, B, and C, are chosen, which, when combined, make white of a certain intensity, W; and the author shows that if the luminosity of the combined light A + B be subtracted from the luminosity of the white light, W, the remainder exactly equals the luminosity of the third factor, C.

A curve of luminosity can be constructed in this manner for the whole spectrum, and its maximum is found to lie on the yellow side of the D line. A similar luminosity curve is given for an observer who was what is ordinarily called red-blind. On this curve the red end of the spectrum is shortened, and the maximum luminosity falls nearer the green than on the curve constructed for a person with normal colour-vision. These facts are explained as follows. To the red-blind observer red is invisible, and therefore the luminosity of red is abolished; the luminosity of yellow, which is composed of red and green, is also diminished, and thus the maximum of the curve moves towards the green.

This question of luminosity is intimately associated with the theory of the value of white in the system of colour. The author discusses later on in the book the abolition of colour by white light, and examines the extent to which white light can be added to a colour without being perceived. He finds that both depend on the luminosity of the colour, and formulates the law that "the extinction of every colour is effected by white light that is 75 times brighter than the colour." Again, he finds that a large proportion of white light can be mixed with yellow without being perceived, whilst a very small proportion of white added to blue is at once apparent.

An attempt is made to explain these facts on the Young-Helmholtz theory; but the work done by Hillebrand,[1] under Hering's guidance, makes the explanation offered very improbable. Hillebrand used an apparatus in which one half of the field could be illuminated by a monochromatic spectral colour, whilst the other half was illuminated by white light. The observer shielded one eye from the light for a considerable time, so that it was ultimately brought into a condition of complete rest. Now if he looked at a field filled with monochromatic light of moderate intensity with the rested eye, it appeared to him colourless; and by suitable adjustment he could make an absolute match between the half of the field illuminated by monochromatic light and the other half illuminated by white light from the same source. Thus, as the whole spectrum appeared colourless, he was able to construct a curve of luminosity for the spectrum by matching it with the white light in the other half of the field. The maximum of this curve lay in the green. A glance with the unshaded eye at once brought the colour into view, although the field was unaltered. But as soon as the colour came into view, he noticed that the luminosity of the coloured half no longer matched that of the colourless half of the field. If yellow or red were the colour chosen, the luminosity of the coloured half of the field appeared to exceed that of the colourless half, whilst if green or blue were selected the exact opposite was observed. Moreover, as soon as the colours of the spectrum were appreciated, the maximum luminosity shifted into the yellow, and the curve he then obtained closely resembled that constructed by Captain Abney and other observers. Thus we must conclude that every part of the spectrum is capable of exciting the sensation of white apart from its specific colour, and that the maximum sensation is produced by a certain point in the green. As soon, however, as the colour becomes apparent, this sensation of white is either increased or decreased by the specific luminosity of the colour. The luminosity of the spectrum, as determined by Captain Abney, is the algebraic sum of two factors. Firstly, the power which every part of the spectrum possesses of exciting the sensation of white; and secondly, the specific luminosity of the colour sensation itself, which is a positive quantity on the red and yellow side and a negative quantity in the blue and green.

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[1] "Ueber die specifische Helligkeit der Farben," *Sitzb. d. k. Akad. d. Wissenschaft. in Wien*, February 1889.

If this explanation for the difference in the two curves be correct, a person who was completely deficient in colour-sense would construct a luminosity curve for the spectrum differing considerably in the position of its maximum from that given by Captain Abney in his book. The curve obtained by König[1] from a man to whom yellow, blue, green, and red were invisible, to whom the whole spectrum appeared in varying shades of white, shows that this is the case. The maximum luminosity lies in the green, over the line *b*. A comparison of this curve with that given by Hillebrand for the normal eye at rest reveals their almost absolute identity. The existence of this form of colour-blindness can only be explained with extreme difficulty on the Young-Helmholtz theory; whereas Hering's hypothesis, that white and black form a colour pair analogous to red and green, yellow and blue, not only renders the existence of such a condition probable, but also easily explains Hillebrand's results.

The author passes on to show that white can be produced from the mixture of three spectral colours, and ultimately defines a primary colour as one which cannot be formed by the mixture of any other colours. The three primary colours he selects are red, green, and violet; for yellow is formed by a mixture of red and green, blue by a mixture of green and violet. But he warns us from assuming that the three primary colour sensations "are of necessity the same sensations as are given by the three primary colours" (p. 138). On p. 150, red (between C and the lithium line), violet (close to G), are selected as furnishing two primary sensations, whilst "all three fundamental sensations" are excited by the green, except at a point where the green is mixed with white only.

Now, to say that spectral green excites the sensations of red and violet seems to us radically false. For when speaking of sensations we leave the realm of physics, and the sole test of the sensations excited by a portion of the spectrum is the colour which we perceive when light from that part impinges on the retina. No one who examines spectral green will say that it gives him the sensation of red or violet, but rather that the greater part of spectral green appears to be mixed with either yellow or blue. Again, a primary sensation must be one which gives us the sensation of one colour only. Now every eye sees in violet both blue and red. Thus, whether violet be a primary colour from the physical point of view, physiologically speaking it is anything but a primary sensation.

Though violet fails to answer the test of a primary colour sensation, a point can be found both in the yellow and the blue of the spectrum, from which the sensation of one colour only is obtained. But throughout the book we find repeated mention of the formation of yellow by the mixture of spectral red and spectral green. How can this be reconciled with the acceptance of yellow as a primary sensation?

To most eyes, the red of the spectrum yields to a greater or less extent the secondary sensation of yellow. Take such a red, and gradually add minute quantities of spectral blue. The yellow will gradually disappear, and a red will be produced, which yields the sensation of red only, untinged with either yellow or blue. Take a spectral green, which is also slightly yellow, and treat it in the same way. If we now mix the absolutely pure red with the absolutely pure green, white is produced, not yellow. And now we can understand why spectral red and spectral green can be made to form yellow. For both the red and the green, which, when mixed, form yellow, when separate give the secondary sensation of yellow in addition to that of their principal colour. Thus, when mixed, the pure red annihilates the pure green, and yellow only remains. Measured by this standard, the primary colour sensations fall into two groups, in which each colour is complementary to the other. Firstly, red and green, from which all secondary sensations of yellow and blue are absent; and secondly, yellow and blue, which do not give the secondary sensations of either red or green.

Colour-blindness is brought in to support the Young-Helmholtz theory, but the author has obviously not had the opportunity of investigating many cases of this affection. He speaks of green-blindness, in which the sensations of red and violet are present, but not that of green; and of red-blindness, in which the sensations of green and violet are present, but not that of red; and gives measurements to show that in the latter class of cases the spectrum is shortened.

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[1] "Die Grundempfindungen u. ihre Intensitäts-Vertheilung im Spectrum," *Sitzb. d. k. preus. Akad. d. Wissenschaft. zu Berlin*, xxxix, 1886. Hering has since shown, by investigating a similar case of total colour-blindness, how closely the curve ot luminosity agrees with that given by Hillebrand. The account of this interesting case has not yet been published.

Now, Hering[1] has particularly investigated this portion of the subject, and explains the existence of two forms of colour-blindness as follows. He finds that persons with a normal colour-vision can be divided into two groups. The one class perceive yellow, the other blue, with exceptional ease, probably owing to a difference in the pigmentation of the media of the eye. The difference between the two groups is best seen with spectral green; for a green can be found which appears at the same time yellow-green to the one, blue-green to the other. To an observer with strong yellow vision, almost the whole of spectral red appears to be tinged with yellow, whilst a member of the second group, whose strong sense of blue prevents his seeing the yellow, pronounces the greater part to be pure red. Thus, the pure red and the pure blue are radically different colours for the two groups. Now, it is found that the pure red and the pure green formed for an observer with a strong sense of yellow appear grey to one who is what is called green-blind; whilst, on the other hand, the pure red or the pure green of the observer with a strong blue sense appears colourless to one who is red-blind. A red which is invisible to one who is "red-blind" is evidently coloured to a patient who is green blind, and he speaks of the colour he sees as red. But if a minute proportion of blue is added, the red gradually becomes purer until it becomes free from yellow to those of us who have a strong yellow sense. As the red becomes purer, our green-blind patient complains that the "red" is fading, and when finally the red is quite pure he matches the colour he sees with a grey, and says that the colour has gone. Thus, there is no fundamental difference between the red- and the green-blind. Neither group can perceive red or green. The only difference between them is one which we find amongst normal-sighted persons—namely, a different visual acuity for yellow and blue. The "red" of the green-blind is in reality the secondary sensation of yellow yielded by almost all the reds in nature, differing from the ordinary yellow in its limited power of exciting white. This peculiar yellow he has learnt to associate with what others around him call red, and he only betrays his affliction when all yellow is eliminated from the colour he calls red. Thus, a consideration of colour-blindness again leads us to throw red and green, blue and yellow, together into two groups as primary colour sensations.

Simultaneous contrast is touched on very superficially, and successive contrast is scarcely mentioned, yet the author again grasps at the three-colour theory to explain the few phenomena he mentions. Yet it is notorious that the Young-Helmholtz theory fails to afford any adequate explanation of the phenomena of contrast. It was by an ingenious contrast experiment that Hering produced such a striking confirmation of his views before the Physiological Congress at Basle, and placed the three-colour theory in a dilemma from which its ablest exponents have not yet succeeded in extracting it.

In conclusion, the book before us is an admirable summary of a valuable series of experiments. We can scarcely imagine that it will appeal to the public in general. But it should be read by those who are interested in the phenomenon of colour-vision, and the fact that the author frankly accepts the three-colour theory and ignores the work of Hering does not, in our opinion, detract from its value. For the book thus becomes an admirable statement of the strongest portion of the physical theory of colour by one of the ablest of English physicists.

H. H.

POSITIVE SCIENCE AND THE SPHINX.

Riddles of the Sphinx. A Study in the Philosophy of Evolution. By a Troglodyte. (London: Swan Sonnenschein, 1891.)

THESE be old old riddles that the Sphinx propounds and the Troglodyte attempts to guess, in the volume before us; none other, indeed, than the What, Whence, and Whither of man and of the world. There have been other guesses in the past, there will be other guesses while time lasts; each guesser thinks his own guess nearer the true answer than any other; his neighbours mostly smile, unless his guess chances to be something like their own; and the Sphinx looks on with stony stare, imperturbable, giving no hints.

So soon as man, as man, looked out upon the world, and began dimly to realize the first personal pronoun, the nascent reason, or, if the phrase be preferred, intellectual faculty, demanded, for the first time in the history of the development of consciousness, an explanation. Man, then as now the chief centre of interest to man, must thenceforward not only live and act, but must seek to explain his life, and his activity. Yesterday the tribe-chief went forth a living man, feared by all: to-day his body is brought back, helpless, lifeless, and a hog spurns it with his snout. How account for this? How explain this change? Something there was about the man yesterday which made him totally different from the mere mass of clay that to-day already needs hustling out of sight. That something, call it soul, spirit, energy, life, what you will, has departed. Whither has it gone?

This question, eminently natural, almost inevitable, opened the way for reason's first blunder to enter and to become a fruitful mother of children. Reason, in the exercise of the new-born analytic faculty, distinguished between the mere body and the informing something through which it was a living body; between the material substance and the spirit-energy which was associated with that substance during life. But reason also jumped to the conclusion that what were distinguishable in thought were also capable of separate existence in fact. The matter remains in the corpse, but the something, the spirit-energy, has escaped, to lead a distinct and independent existence. In justification of this conclusion the phenomena of dreams were no doubt adduced as evidence. While the chief's body was lying stark and stiff, his true self, his spirit-energy, appeared by night to more than one of his chosen followers. Thus the dream seemed to support the false conclusion of the nascent reason, which had not yet learnt to distinguish without dividing.

It has cost positive science much labour, and not a few hard blows, to establish, by detailed work in physical science, biology, neurology, and psychology, the illegitimacy of this conclusion. Now we distinguish further, but no longer divide. We distinguish between the material substance of the body and the energy of molecular motion during life; and, further, between the molecular motion of the grey matter of the cerebral hemispheres and the concomitant manifestation of consciousness. But although consciousness is distinguishable from molecular energy (and the distinction is absolute), it is not, so far as positive science can say, divisible therefrom. No physicist holds that the special modes of energy—we mean the particular groupings and interactions of energy—which characterize the functioning of a man's brain, escape from the molecules at death, and henceforward persist divorced from matter. We cannot, however, add that no psychologist holds an analogous doctrine concerning consciousness. But we contend that no psychologist is justified *on positive grounds* in holding such a view. That something called soul or spirit escapes from a man's body at death, and henceforward persists, divorced alike from matter and energy, is a view to which positive science as such gives no support. It is held by those who hold it on quite other grounds. The conclusion to which positive science points (and we include among positive sciences psychology, which deals with consciousness as existent) is that consciousness, though distinguishable from energy, is known only in association with certain forms of energy in organic tissues.

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[1] "Zur Erklärung d. Farbenblindheit" (Prag, 1880); "Ueber Individuelle Verschiedenheiten des Farbensinnes" (Prag, 1885); "Eine Vorrichtung z. diagnose d. Farbenblindheit," "Ueber d. Erklärung d. periphären Farbenblindheit," "Einseitige Störungen d. Farbensinnes," *Archiv f. Ophthalmologie*, xxxvi.

But this is a conclusion which is ignored by the Troglodyte. He professes to give us a "philosophy of evolution" which he himself describes as "the first perhaps which accepts without reserve the data of modern science." His theory of a Transcendental Ego; his suggestion that "matter is an admirably calculated machinery for regulating, limiting, and restraining the consciousness which it encases"; his conception of a graduated immortality, from that of an amœba up to that of man; his attempted rehabilitation of the view that force-atoms are monads "endowed with something like intelligence, and thus enabled to keep their positions with respect to one another"; all this, and much besides, seems to us completely off the lines of modern scientific advance.

But it may be said that such conceptions, though unnecessary for positive science, may be necessary for a philosophy which endeavours to go beyond and get behind science. In reply to this we can only say that we regard such conceptions as not only unnecessary to positive science, but unwarrantable intrusions into her domain. They form part of a different scheme of thought. The muddling together of positive and metaphysical conceptions is provocative of nothing but confusion and bad temper.

The introductory chapters of his first book, in which the author attempts to hound on positive science from agnosticism, through universal scepticism, to a gloomy pessimism, seem to us laboured and inconclusive, though there are incidental positions here and elsewhere with which we are in complete accord. With dogmatic Agnosticism and the Cult of the Unknowable (capital letters indispensable) we have but little sympathy. But this is no necessary part of the attitude of positive science, which seems to us briefly as follows. In the first place its followers take their start from the measurable and verifiable base-line of perceptual experience, from the ordinary facts of daily observation; and they utterly refuse, at this stage of the inquiry, to listen to the metaphysicians who hoot from their cloud-land, "But you haven't yet proved the existence of matter, or explained how it is possible to perceive or know anything at all." Starting, then, from the base-line of perceptual experience, they analyze phenomena, digging down by wise abstraction and the ignoring of unessentials, to deeper and deeper concepts, until they arrive at those universal abstracts which cannot be got rid of in thought without reaching nonentity. Happy they who in this procedure escape the analyst's fallacy—the supposition that the results of abstraction have a fuller reality than the phenomena with which they started. The analyst needs often to be reminded that the perceptual rose, with its delicate scent, its rich colour, its soft petals, is certainly not less real than the vibrating molecules which remain to his thought when, as physicist, he has stripped it of all its own peculiar charms.

Thus positive science in its deepest analysis brings us down to matter, and energy, and consciousness. If a number of metaphysical questions are intruded at all sorts of stages during this process, the result will be such confusion as the Troglodyte unconsciously exemplifies in his chapter on scepticism, a chapter in which some stress is laid on, and some capital made out of, the false psychological conclusion that conceptions cannot be derived from experience. Should the author ever come to grasp that the law of psychogenesis is one and indivisible, and sweeps through perceptual and conceptual processes alike, he will have to rewrite much of the "Riddles of the Sphinx." But, as he himself tells us, "the minds of most men are fortresses impenetrable to the most obvious fact, unless it can open up a correspondence with some of the prejudices within."

When positive science has dug down to basal conceptions, then, and not till then, in logical order (but, of course, far earlier in historical order) arises the question, "But how does it all come about? What is the origin and meaning of it?" We quite agree with the Troglodyte that this question *must* arise in the mind of every man in so far as he is a thinking man. The question, "How does it all come about?" however, presents two faces. It may mean, "How can we explain the fact of *knowing?*" And the solution of this problem is, we agree with Mr. Shadworth Hodgson in maintaining, the true business of philosophy. But even supposing that philosophy explains in some sense the process of knowing, there still remains the question in its further aspect, "But how does it all come about?" To this question, positive science as such answers, or should answer, humbly, and with no parade of capital letters, "I do not know."

And is that the end of the matter? So far as positive science at present goes, Yes. But man, the questioner, still remains; and Reason, true to her first impulse, still demands an explanation. Of the explanation afforded by revelation this is not the place to speak. But, quite apart from the fact of revelation, the explanation said to be revealed still stands as a product of the human mind. And he is a bold man, if not a foolish, who, having regard to the past history of human thought on the question, lightly sets aside the conception of a *Causa causarum* to whom we may attribute *symbolically* all the higher attributes of man; not because personality, wisdom, love (the symbols we employ), can truly describe or define that which passes man's comprehension, but because being man we can no other. Man alone in the organic world is capable of ideals, and for generations the name of God has stood for man's central ideal of power and perfection. And it seems to us that the sum and substance of positive criticism as applied to man's conceptions of that which admittedly lies beyond the reach of positive science comes to this: "You must frankly acknowledge and confess that such conceptions are symbolic and ideal." But if symbolic and ideal we must expect the symbolism to be variable in different ages, among different peoples, and even in different individuals. Hence (apart from revelation) the only indefensible attitude is that of inelastic dogmatism, positive or negative.

In conclusion, we may say that the "Riddles of the Sphinx" are in this work treated with considerable, though frequently misguided, power. The conception of evolution as a tendency towards an ideal of perfect individuals in a perfect society is good, and is in parts well worked out. That many will be found to acquiesce in the author's solutions of the old problems of life we think exceedingly doubtful. Nor do we think that the solutions will prove of lasting value. It is futile to attempt to preserve the new wine of positive science in the old bottles of prescientific metaphysics. The new wine must be preserved in new bottles. In other words, a new metaphysics must be and is being elaborated, in special relation to the newer aspects of scientific thought.

C. LL. M.

ANALYTICAL METHODS OF AGRICULTURAL CHEMISTS.

Proceedings of the Association of Official Agricultural Chemists, 1890. (Washington: United States Department of Agriculture.)

THIS is a Report of the Seventh Annual Convention of the Association, under the Presidentship of Mr. M. A. Scovell, and with Mr. H. W. Wiley as Secretary. The objects of the Association are to secure uniformity and accuracy of methods, results, and modes of statements of analyses of manures, soils, cattle foods, dairy products, and other materials connected with agricultural industry; and to afford opportunity for the discussion of matters of interest to agricultural chemists. In the words of a past President, it aims at laying "a foundation so solid, that every Court in this land must respect its conclusions, and every analytical chemist, whether he lives in this country or elsewhere, must be forced either to practice or admit the advantages and correctness of our system of analyses." A study of the programme and of the proceedings shows that the objects have been most carefully and conscientiously kept in view, and that all the working members have been most thoroughly imbued with the spirit of the Association.

The reports submitted for the consideration of the meetings, all drawn up by experts, and incorporating the work of many members, were as follows: on the determination of nitrogen; on analysis of dairy products; on analysis of potash; on analysis of cattle foods; on analysis of sugar; on analysis of phosphoric acid; on analysis of fermented liquors; and a report of a Committee on foods and feeding-stuffs.

As an example: for the report on the determination of nitrogen in manures, three samples, containing nitrogen in different states of combination, were prepared, and sent to the members for analysis by various official methods. Twenty-two reported the results obtained by Kjeldahl's method on one sample, the same number the results of Kjeldahl's method modified for nitrates on two samples, and a less number gave results by the Ruffle method, the soda-lime method, and Dumas's method on one or more of the samples. The whole of the results are collated, with the remarks of the analysts thereon, so that data are obtained for testing the accuracy of the methods under various conditions, and eliminating personal factors. Various suggestions for the improvement or simplification of the processes are made and discussed, and some of them recommended for systematic trial during next year. Similar good work is done for the other Committees.

The remarks of the Committee on ways and means for securing more thorough chemical study of foods and feeding-stuffs, are particularly worthy of attention, pointing out, as they do, the deficiencies in present methods of analysis, and the absolute necessity of more exact methods and more accurate study of the proximate principles contained in foods, and of their physiological value. As a contribution towards this knowledge, Mr. W. E. Stone sends a paper on the occurrences and estimation of the pentaglucoses in feeding-stuffs, in which he shows that bodies yielding furfurol, and therefore presumably pentaglucoses, are present in grass, straw, linseed meal, and a great many other feeding-stuffs. Among the points which are noticed, and which should be known to all analysts, is the fact that cotton-seed meal, often used in mixed manures in the Southern States, is completely soluble in nitric acid with a little hydrochloric acid, but that the solution does not yield all its phosphoric acid to ammonium molybdate.

Should such a Bill as that introduced by Mr. Channing, for the better prevention of the adulteration of manures and feeding-stuffs in this country, ever become law—and the Government has promised to take up the matter—the formation of such an Association of Official Agricultural and Analytical Chemists in this country would be almost a necessity, and it seems that the Institute of Chemistry is the proper body to arrange the organization of such an Association.

GEOLOGICAL RAMBLES ROUND ABOUT LONDON.

Hand-book of the London Geological Field Class. By Prof. H. G. Seeley, F.R.S. (London: G. Philip and Son, 1891.)

THIS little book is a record of excursions similar in some respects to those collected in the volume of Geological Excursions which was noticed in these columns on June 18 (p. 149). But there are points of difference. This hand-book deals with a more limited area, being practically restricted to the south-east of England; it has a purpose more definitely educational. The latter may be described in a few sentences extracted from the preface:—

> "This Society exists to teach the elements of Physical Geography and Geology direct from Nature without preliminary study from books. . . . The field work has been led up to by short courses of winter lectures given in London, designed to connect together the observations to be made in the succeeding summer, and to connect the geology of the district to be examined with that of other areas."

The excursions are described in the notes written by students in the field; the lectures are reported (from shorthand notes) by Mr. White, one of the class. As regards the former, Prof. Seeley states that "students have been free to report what they saw and what they heard, and they have severally written in their own ways both as to length and language used." The lectures also "were not constructed with a view to being reported, nor were the reports written out with a view to being printed." Prof. Seeley has, however, "read the proof to remove serious inaccuracies." The lectures need no apology, for they are excellent examples of that clear and suggestive method of teaching of which Prof. Seeley is a master. The reports of the excursions also acquire a certain freshness as recording the impressions of novices, and may on that account be even more helpful to beginners than if they had been written by more experienced observers. One or two inaccuracies, however, appear to have escaped the Professor's watchful eye. Is not the statement on p. 18, relating to the presence of *Paludina* and *Unio* in such Wealden Limestones as the Petworth Marble, a little misleading? for it implies that the latter genus is common in these deposits, which, we believe, is not the case. A sentence on p. 29 suggests that "enormous pressure" is requisite to convert a sandstone into a quartzite. Very probably this would be the result, but there are not a few quartzites which show no signs of having been specially subjected to pressure. Also, it is hardly correct to call Lydian stone an altered sandstone. Again, more than once it is intimated that gneiss and crystalline schists occur in Belgium. This, if the terms be used in their ordinary sense, is incorrect; and even the porphyroids and amphibolites, and the abnormal rocks of the Bastogne district, the vague descriptions of which may have given rise to this misconception, are of extremely limited extent. But these are very trifling blemishes, which can be readily removed in a second edition. The book will be of great use to all students, living in or about London, in helping them to use their eyes; and most of all because, to quote Prof. Seeley's words, "It here and there touches upon problems which are not usually presented to beginners." But, as he rightly urges, these problems—namely, the application of stratigraphy to the elucidation of the physical geology of past epochs—"should never be absent from the mind of anyone who considers geological facts in the field."

T. G. B.

OUR BOOK SHELF.

Katalog der Bibliothek der Deutschen Seewarte zu Hamburg. (Hamburg, 1890.)

VARIOUS notices have from time to time appeared in NATURE relating to the German Naval Observatory at Hamburg, describing the building, its equipment of instruments, and the important work which is carried on there chiefly in the interests of the German Imperial and mercantile navies.

As this institution is possessed of a library containing some 10,660 works, it has for some time past been a matter of urgent necessity that an accurate and well-considered form of Catalogue should be printed and published. The required book was completed last year, and is now available.

This Catalogue shows that the library contains a large proportion of works either directly of a naval character, or bearing upon naval matters, whilst several other branches of science are fairly represented.

As might be expected, meteorology holds the first place of importance, and amongst the 2769 works on this subject are a large proportion of Dove's writings. Indeed, it seems worthy of note that Dove's library, which occupied him many years in collecting, may now be found at the German Naval Observatory. Turning to the division of the Catalogue on physics, 1617 works will be found; on magnetism and electricity, 974; whilst other subjects, such as navigation, hydrography, and construction of ships are well cared for.

Although the books and papers mentioned in this Catalogue are generally printed in the language adopted by their authors, a translation into German of several works of interest is also placed side by side with the original.

In conclusion it may be remarked that although there is nothing specially new in the arrangement of this book, it is well worthy of the time and energy which have evidently been spent in bringing the work to its present state.

Scientific Results of the Second Yarkand Mission; based upon the Collections and Notes of the late Ferdinand Stoliczka, Ph.D.—Coleoptera. By H. W. Bates, F.R.S., J. B. Baly, D. Sharp, F.R.S., O. Janson, and F. Bates. Pp. 1–79 and 2 Plates. (Calcutta: Published by order of the Government of India, 1890.)

THIS, the twelfth part issued, all but one of which deal with zoology, contains an enumeration of 207 species of Coleoptera. These species belong to the following families:—Cicindelidæ (4), Carabidæ (60), Longicornia (5), Phytophaga (25), Haliplidæ (1), Dytiscidæ (8), Gyrinidæ (1), Hydrophilidæ (3), Staphylinidæ (9), Scarabæidæ (38), Cetoniidæ (3), and Heteromera (50). Diagnoses or descriptions of all the new genera and species were published more than ten years ago, and the only additional information contained in this part is a list of species, in addition to, in some cases, fuller descriptions of the novelties. In the portions contributed by Mr. H. W. Bates and Dr. Baly, both of whom, however, give some particulars regarding geographical distribution, the references to the published diagnoses are given; but in Dr. Sharp's and Mr. F. Bates's contributions, many of the genera and species are mentioned as new, though diagnoses of the whole of them were published in 1878 or 1879—the former in the Journal of the Asiatic Society of Bengal, xlvii. Part 2 (1878), the latter in *Cistula Entomologica*, ii. (February 1879). The two plates include 44 figures—Carabidæ (17), Longicornia (5), and Heteromera (22). On the cover, and also on p. 37, the name "Hydrophilidæ" is misprinted "Hydroptilidæ." The Hydroptilidæ do not belong to the order Coleoptera at all, but to the Neuroptera! It is to be regretted that a delay, the cause of which is not explained, of more than ten years, has occurred in the publication of the "Part" dealing with the Coleoptera, as works of this kind upon the beetle fauna of little-known districts are always of the highest value, more particularly in the matter of geographical distribution. No systematic work upon the Coleopterous fauna of India has yet been published, and even a fragment like the present, containing a list of the species of a neighbouring region, is a welcome addition to our knowledge. Four other "Parts" have been issued on the Insecta—the "Neuroptera" and "Hymenoptera" (both in 1878), and the "Lepidoptera" and "Rhynchota" (both in 1879); the last Part of the whole series being the "Araneidea" (1885).

Popular Astronomy. By Sir George B. Airy, K.C.B. Seventh Edition. Revised by H. H. Turner, M.A., B.Sc. (London: Macmillan and Co., 1891.)

ALTHOUGH our astronomical knowledge has been enormously extended since the lectures forming the basis of this well-known book were delivered (1848), Mr. Turner has not found it necessary to make any very considerable revision, for the reason that the advances have been chiefly on the chemical and physical sides. Still, in the lapse of time, methods of observation have been improved, and accounts of these find a place in Mr. Turner's notes. Among these are short descriptions of the chronograph and the new "electrical controls" for the driving-clocks of equatorials. One of the most noteworthy points brought out in the new edition, however, is the modern estimate of the value of observations of the transit of Venus as a means of determining the solar parallax. It was formerly supposed that this would be one of the best methods, but the difficulties encountered in 1874 and 1882 prevented observations of the necessary degree of accuracy; and now most astronomers are of opinion that this method can never give more than an approximation to the truth. Numerous minor additions have also been judiciously made.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Force and Determinism.

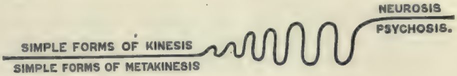
"THE relation between force, which is a mechanical thing, and will or life, or whatever it is, which is a psychological thing"—a relation which, as Dr. Lodge rightly says, "demands investigation"—presents itself to some of us as follows.

When a stimulus received by an organism gives rise to a response, however particular to the individual respondent, there are (1) a number of complex but determinate molecular changes in the organic tissues; and (2), accompanying some of these changes, certain psychological states. Are these psychological states produced by the molecular changes? or are the molecular changes produced or in any way guided by the psychological states? Neither the one nor the other. The molecular changes and the psychological states are different aspects of the same occurrences. In other words, they are distinguishable (and the distinction is absolute), but not divisible.

"The energy displayed by a gang of navvies is not theirs, but their victuals'; they simply direct it." In physiological language it is the outcome of the proper functioning of their cerebral control-centres. Now we believe that, although we can at present by no means adequately explain them, all the molecular occurrences within the organism, forming, as we believe they do, an orderly and determinate sequence between stimulus and response, whether they involve force or energy, are of such a nature as to be explicable in physical and physiological terms. The fact that certain phases of the sequence have also a subjective or psychological aspect does not, it is held, justify us in changing our point of view, and ignoring the distinction between the psychology and the physiology of the process.

Now to say that mind, or will, or consciousness directs the organic energy along a definite path we regard as incorrect, because it ignores a distinction which we hold to be valid and valuable, and conducive to clear thinking on these difficult subjects. But we have no such objection to the statement that the energy is guided by molecular forces which have for their subjective aspect certain states of consciousness. To unscientific folk this may sound mere quibbling; but to physicists, who have done so much to teach us the vital importance of accurate language for clear thinking, we look for support in drawing this distinction, unless the distinction can be shown to be either invalid or useless.

This distinction between force, energy, and the physical series (what I have elsewhere spoken of as *kinesis*) on the one hand, and thought, consciousness, and the psychical series (what I have elsewhere spoken of as *metakinesis*) on the other hand, we hold to be absolute; while at the same time we hold that consciousness is indivisible from particular (neural) modes of kinesis. And this distinction we hold to be especially valuable when questions of the origin and development of consciousness are under consideration. This may, perhaps, best be expressed by a diagram.



Now, looked at from above, this wriggle is supposed to represent the development, from simple forms of molecular transactions, of that complex form of kinesis which we call neurosis. From this point of view, all is force and energy or kinesis, and can become nothing else. Looked at from below, we have the development of consciousness. From what? We must not say from lower forms of energy or kinesis, because that involves jumping across the line, or, in other words, ignoring the distinction. From what, then? From those lower forms of "something-which-is-not-yet-consciousness-but-which-may-develop-into-consciousness," for which I have ventured to coin the term metakinesis.

I have elsewhere endeavoured to show that this view is not open to the objection that, since the kinetic sequence is a continuous and determinate one, consciousness is merely a by-product, and that an unconscious Darwin might have written and influenced the conduct of unconscious Englishmen. For consciousness, though it is distinguishable from, is, according to the hypothesis, no less inseparable from, certain complex modes of the kinetic process. As the world is constituted, such supposed kineses, separated from their metakinetic aspect, would not be the same kineses but something altogether different. In other words, it is with certain molecular transactions which have also a conscious aspect that, in the world of living beings of which we have practical knowledge, we have to deal.

It is essential that physicists and psychologists should work hand in hand. Both are endeavouring to explain the phenomena on positive lines. And if there is anything in the views that I have briefly sketched in the preceding paragraphs which runs counter to the conclusions of physics, it must go by the board, and give place to a more widely-consistent conclusion, to which physics, speaking with the voice of authority in its own special province, can give a cordial assent. C. LLOYD MORGAN.

I AM afraid that, as Prof. Lodge has accepted my "middle paragraph" so easily, he has failed to appreciate its point. For, if that paragraph is correct, the Professor's assertion, "Force is certainly necessary to direct the motion of matter," is only a truism, similar to the important geometrical theorem, "In any right-angled triangle, one angle is equal to 90°." On the other hand, Dr. Croll's assertion, to the effect that guidance is effected by "determinism," and *not* by force, is a contradiction in terms. For, by definition, that which changes motion *is* force. If, therefore, Prof. Lodge's assertion has any real meaning, he must have some independent definition of "force," and I should very much like to know what that is.

Again, Prof. Lodge in no way answers "the crux in my last paragraph." Prof. Lloyd Morgan implies in his last letter that, in the case of the sun altering the direction of motion of the earth, no energy is expended. This is, of course, only approximately true; and even in the case of his twirling his stick round his finger and thumb, as the stick is elastic, its forces of cohesion in reality do some small amount of work. It is indeed true that, if two particles were once connected by an *absolutely* inextensible string, the cohesion of the string would do no work. But what I pointed out was that, in order to bring such a string into action, it would be necessary to wait till two particles were moving on paths with a common normal—an occurrence which must be infinitely rare. When Prof. Lodge says "an infinite mass can absorb any amount of momentum, without receiving a trace of energy, &c.," he forgets that the term "infinite" is only relative, "an infinite mass" being one whose change of velocity (or kinetic energy) consequent on a given change of momentum is negligible *for the purpose in hand*. It would not, I imagine, suit Prof. Lodge's purpose to suppose psychic forces might do a *little* work, so long as it was only a *very little*?

May I remind him of the old paradox, "What would happen if an irresistible force were brought to bear against an immovable post?" EDWARD T. DIXON.

12 Barkston Mansions, South Kensington, July 24.

THE discussion on this topic has gained in clearness by Prof. Lodge's conceding that "the same question—What determines the direction of the transfer of energy?—may doubtless be asked in connection with inanimate activity; . . . but in neither case do I know the answer."

Perhaps some more precision may be attained by expressing the question in other words.

The principle of conservation of energy reigns over the *quantitative* relations of all processes in nature, but it does not give any explanation of the *qualitative* changes of those processes. These changes and their conditions must in every case be found out by special experience. But, nevertheless, they are, in every accessible case, found to be subjected to fixed laws. A given substance undergoes evaporation or chemical transformation—dependent on or necessarily bound up with changes of heat into energy of molecular motion, or into chemical energy, or *vice versâ*—at a distinct degree of temperature, or under distinct conditions of electrical action. Inexplicable as these transformations of quality or form of energy remain for us, there is nothing undetermined in them, neither have we any right to such a supposition for the qualitative changes going on in plants and animals—their quantitative relations being likewise governed by the principle of conservation of energy.

But there is another phase of the question. Some unknown material changes in the brain are connected with phenomena of consciousness. Nothing can be more fallacious than to consider consciousness as a form of energy, and to suppose it in a relation of equivalence to such forms. How it is, that what to our physical conception, or outer sense, are processes in the brain (which, as such, may be more clearly understood in future), are, at the same time, to our psychical conception, or inner sense, phenomena of consciousness, or acts of will, is a question beyond the domain of physical science, and capable of elucidation only by transcendental philosophy. Whoever wishes for more light here, must study the "Kritik der reinen Vernunft," especially the chapters "Von den Paralogismen" and "Die Antinomien."

Schopenhauer, and others after him, have considered our power of will, or our conscious directing of motion, as the key for all qualitative processes in nature, these being considered as, in their essence, acts of will. But this is cutting the knot by means of a metaphysical assumption. D. WETTERHAN.

Freiburg, Badenia, July 27.

IN reading over the remarks of Dr. Lodge and Prof. Morgan upon Dr. Croll's views as to the direction of force, it appears to me that both have missed the point. Dr. Croll did not mean that a force at right angles to another does no work, but simply alters the direction. His view is that the change of direction is not caused by a force. Dr. Lodge says it is, although he acknowledges that the second force does no work. Further, Dr. Croll says, with regard to the first force, that its direction is quite apart from the force. The force cannot direct itself. This is the crucial point before we get to a second force or to a right angle. I fully acknowledge the importance of Dr. Lodge's principle, but it is not simply the indorsement of Dr. Croll's idea.

Prof. Morgan thinks Dr. Croll's view no argument in favour of theism. It does not prove that mind can or does affect matter. Perhaps it does not directly prove this, but, within its range, it seems to me an effective reply to mechanical atheism. We see direction, and if this does not come from force it must come from some other source. We know of no other source but mind. To talk of mind affecting matter denies the essence of mind by which it is distinct from matter, and makes it a mechanical *ab extrâ*. But try to banish it and it will come in somewhere. "Tamen usque recurret."

Dr. Croll's position seems to me to affect the first law of motion. Uniform motion in a straight line is in no way connected essentially with force, if his view is correct.

Dr. Lodge's principle appears to affect the second law of motion, and also the doctrine of impact and transference of force.

Further, it affects gravity. Gravity is always at right angles to the first law of motion, and therefore gravity is not a force; for that can not be a force which never exercises force.

T. TRAVERS SHERLOCK.

Congregational Church, Smethwick, July 25.

Technical Education for Farmers, Farriers, and Engine-Drivers.

KNOWING that you take very great interest in the various questions relating to technical education, I may give you a few particulars of an experiment which the Devon County Agricultural Society recently made at its Exmouth meeting. Being desirous of giving farmers, farriers, and those generally interested in the welfare of horses, some information on the scientific principles which underlie a proper performance of the duties of the farrier, and the correct form and mode of attachment of horses' shoes; and also of giving farmers and engine-drivers some practical and scientific instructions on the working and care of steam-engines, the Society approached the County Council with a view to a grant in aid of their object. The proposal was very warmly taken up by Mr. Lethbridge and other gentlemen who are well known for their active interest in education and other matters important to the welfare of the county, and a grant was obtained.

The Society secured the services of Prof. F. Smith, head of the Army Veterinary School, Aldershot, and of Mr. W. Worby Beaumont, and by these gentlemen lectures were given on each of the three days of the Society's meeting at Exmouth. The weather was very unfavourable on two days, but notwithstanding this the attendance at the lectures was large, and on the second and third days was larger than was expected, and was fully up to the accommodation provided. The audiences were remarkably attentive and appreciative, and in every respect the experiment proved successful. Many who were sceptical before the lectures of their value to working men, became convinced that not only is it possible to give working men information which is useful in an important degree in their daily work, but that the men are themselves quick to appreciate its value. I may mention that on one of the days nearly two hundred shoeing-smiths and a large number of farmers attended the horse-shoeing lectures, and on one day seventy-eight engine-drivers entered for the lecture on the steam-engine, and there were also in attendance a large number of working and gentlemen farmers.

Totnes, July 29. JOHN L. WINTER.

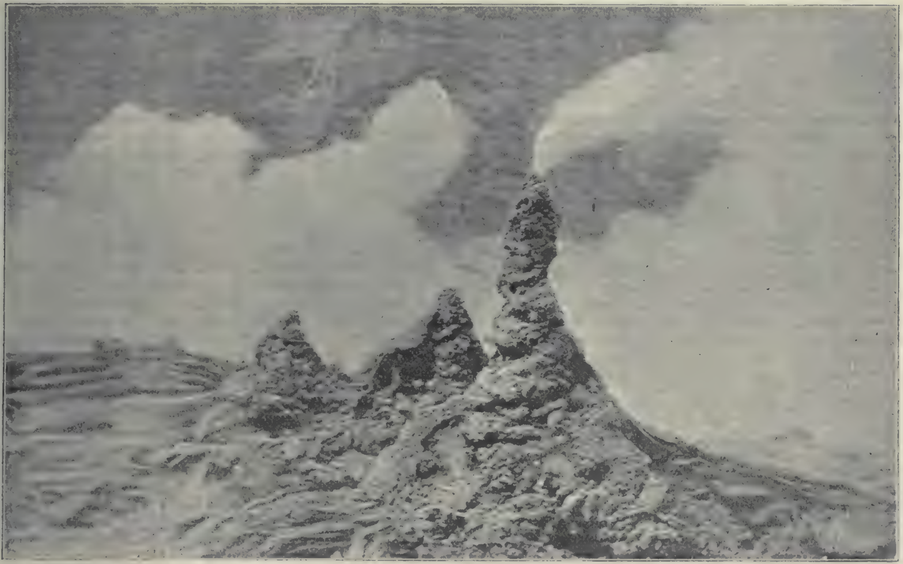
THE ERUPTION OF VESUVIUS OF JUNE 7, 1891.

THE suggestion that I published in several newspapers has been fully confirmed—namely, that the second alternative type of eruptive character would be pursued by the volcano. Now for a period of over a month lava has continued to dribble forth, activity has returned to the central vent, and no great changes have occurred.

The throat of the volcano commenced to be cleared on June 9, the vapour forcing its way up from the crater bottom through the choke of loose materials, and rose above as a column carrying with it much dust; at the same time the powerful vapour blast issuing from the upper extremity of the lateral rift, of which mention is made in my first letter, soon stopped. Each day I was kept informed of the state of the volcano by the kindness of Messrs. Ferber and Treiber, the director and engineer respectively of the Vesuvian Railway.

On June 15 I considered it right to again visit the mountain, and had the good fortune to be accompanied by Messrs. H. Elliot, A. Green, Linden, Newstead, and Treiber, several of whom are excellent photographers, so that with two of my own cameras we were able to make an extensive pictorial record of some very unique formations.

At the point of issue of the lava, at the junction of the foot of the great Vesuvian cone and the Atrio del Cavallo, the first lava had cooled sufficiently to walk over it, but beneath our feet could still be seen in a few holes the flowing lava. At the foot of the great cone, and extending for half way across the Atrio along the radius of the eruptive rent, as if this had continued so far, were a series of driblet cone fumaroles. We counted seven complete and well-formed examples, besides numerous abortive ones. Most were giving out intensely heated vapour, which was liberated from the lava flowing beneath, and which soon carbonized a piece of wood placed in it. Around the lips of the upper opening, hæmatite with fused chlorides of potash, soda, iron, copper, &c., were being condensed from the vapour, and trickling down the outer surface of the fumarole, consolidated as curious vari-coloured stalactites of very deliquescent nature.



The lava had first flowed towards the escarpment of Monte Somma in a fan-like manner, so that the eastern extremity reached that great natural section just beneath the Punta del Nasone. Still following the natural inclination of the ground, it turned to the west, and on June 15 was opposite dyke 16 (as marked on my large geological map just published, and on the dykes themselves), advancing at a very slow rate.

The lava is a vitreous and coarse-grained rock, especially in regard to the included leucite crystals, whilst the surface is, with one exceptional tongue, of the corded or "pahoehoe" type. This is due to the magma being one that has been simmering since January in the chimney of the volcano, so that most of its dissolved H_2O has been boiled off, and so allowing it to cool without the formation of scoria from the vapour that otherwise would escape after its exit. Leucite I have also demonstrated to be formed while the magma is simmering under low pressure with free escape for vapour in the upper part of the volcanic chimney.[1]

At the summit of the great cone the crumbling in of the edges was constantly going on, but the upper extremity of the lateral rift at the foot of the cone of eruption and at the summit of the great Vesuvian cone had nearly ceased to give forth vapour. Along the line of rent on the mountain side no fumaroles or other signs of activity were visible except quite at the foot, where those commence of which I have spoken.

Up till June 26 there was a struggle to clear the upper part of the volcanic chimney of the impeding materials, which were constantly being added to by the slips from the crater's edge; but on that evening a dull red glow was visible in the crater bottom, showing that a fairly clear passage had been temporarily made for the continuous escape of vapour, and also that the lava was at no very great depth from the summit of the volcano. This of course indicates that the lateral opening was insufficient to drain off much of the lava which occupies the chimney above the level of the lateral outlet. Had such evacuation really taken place, the eruption would have assumed enormous proportions, from the actual amount of lava above the tap, but more from frothing up of that below that level in consequence of the relief of pressure that in that case would occur. Of course, during all these days the ejection of dust with the smoke occurred, giving the latter that peculiar dark grey colour. Further destruction of the crater edge took place, so as to partly block the outlet, and it was not till our next visit that it again cleared.

On June 30 I again visited the crater, in company of my friend Mr. A. Green. All the summit of the great cone was covered by a thick coating of dust and sand, upon the surface of which were the usual white and yellowish-green chloride crusts seen on such occasions, so rich in copper as to plate with that metal the iron nails of our boots. The crater had considerably enlarged, the edges were in an extremely unstable state, with often considerable strips marked off by cracks parallel to the free edge, so that, with a slight push by a stick, it was possible to detach large masses of the materials which form the sides of the crater in the recent cone of eruption. So dangerous were the edges, that it was but in two places that my experience indicated as being safe to approach and look over, and that even with several precautions; so that the fatal accident to Señor Silva Jardim, who lost his life here but a few hours after our departure, is not to be wondered at.

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[1] See H. J. J.-L., "Geol. M. Somma and Vesuvius," Q.J.G.S., vol. xl.; and "Relationship of the Structure of Igneous Rocks to the Conditions of their Formation," Scient. Proceed. R. Dublin Soc., vol. v., N.S.

On looking down some 45 to 50 m. beneath us, we could see the glow from a mouth some 2 or 3 m. in diameter. The walls of the crater were concave, so that although overhanging at the top, yet a plumb-line let fall from the edge would strike the bottom of the cliff. The crater bottom was roughly plain, due to the combination of a talus all round, and an attempt at a cone encircling the main vent. It will be thus seen that the crater cavity was of the form of a convex-sided cylinder, or more simply barrel-shaped, with its upper diameter some 50 to 55 m.

With much difficulty we made our way around to the north side of the cone of eruption, which had now lost its usual loose scoria surface, which was buried beneath a thick coat of sand and dust, covered with a thin saline crust on its surface. The upper limit of the radial rift, which we were prevented from examining three weeks previously, on account of its giving out so much vapour as to constitute the temporary escape aperture of the volcano, had now become quiescent, so that we could fully examine it. Only a current of hot air was now issuing from it, but I was able to collect some fine masses of crystallized molysite and kremersite from its edges. Its average breadth was about 0·50 m., where it traversed old compact lava, but of course it disappeared as soon as it reached the looser materials. The real azimuth of its orientation, which we could now determine with greater accuracy than when we were walking over hot rock and enveloped in hot irritating vapours, proves to be, as it radiates away from the axis of Vesuvius, about 15° west of north. It curves then a little to the north, and near the foot of the great cone it again assumes nearly the same azimuth as at starting, an arrangement which is quite evident when the Vesuvian cone is regarded from the Punta del Nasone. From that, the highest point of Somma, the lower extremity of the rift lies a little to the right or west, and faces that part of the Somma ridge which corresponds to the upper extremity of the Vallone Cancherone.

In the forenoon of June 30 much dust had fallen at the lower railway station, of which we collected some bagsful. It is the usual fine sandy material of these eruptions, and consists of the pulverized materials of the cone of eruption.

Having passed the night at the lower railway station, the next day we crossed the Atrio, ascended to the western extremity of the ridge of Somma, and followed it along so as to get a general bird's-eye view of the whole scene of the eruption, and take photographs of the more important points. As one stands on the Punta del Nasone and embraces that magnificent view of Vesuvius and the Atrio del Cavallo, one sees at their feet the new lava-stream in the form of the letter L, the horizontal portion of which is still being prolonged down the Atrio towards the Fossa della Vetrana. In the middle of the ridge we found a thin coating of fine red dust which had reached thus far from the crater. Much of the Atrio was also covered by the same material. Scaling the cliff face just beyond the Cognulo di Ottajano to the Atrio del Cavallo, we again visited the lower point of the outburst. Most of the beautiful fumaroles were in a state of ruin, and lined by good-sized crystals of hæmatite and mixed chloride crusts. Here the lava was quite solid, though at one point was a hole, some 50 m. from the base of the great cone, where we could see the molten rock flowing lazily along about a metre beneath our feet. The lava at the end of the flow was making considerable progress to the westwards, and stood opposite dyke 13.

Since then, few changes have taken place in the mountain: the crater still gets larger, dust is thrown out, and the lava descends. These phenomena are capable of continuing for months if the drainage opening does not enlarge.

As the eruption progresses, I will send you further details. H. J. JOHNSTON-LAVIS.

THE PRODUCTION OF MUSICAL NOTES FROM NON-MUSICAL SANDS.

THAT I have succeeded in producing musical notes from sand that was never before musical, and am also able to produce similar results from certain mute or "killed" musical sands which have been temporarily deprived of their musical properties, has already been announced in the *Chemical News* (vol. lxiv. No. 1650).

It is not necessary now to give the details of the numerous experiments which led up to this discovery; it will be, perhaps, sufficient for present purposes, to state that in November 1888 I published a paper[1] in which I propounded a theory to account for the cause of musical sounds issuing from certain sands. After giving various reasons for my conclusions, I said:—"It occurred to me, then, that the music from sand was simply the result of the *rubbing together* of the surfaces of millions of perfectly clean grains of quartz, free from angularities, roughness, or adherent matter, in the form of clinging fragments investing the grains, and that these microlithic emissions of sound, though individually inaudible, might in combination produce a note sufficiently powerful to be sensible to us."

Having described numerous experiments, and drawn attention to the hopeful results obtained from the "millet-seed" sand, my paper concluded with the following:—"From what I have now told you, I think we may conclude that music may be produced from sand if (1) the grains are rounded, polished, and free from fine fragments; (2) if they have a sufficient amount of 'play' to enable them to slide one against the other; (3) if the grains are perfectly clean; and (4) if they possess a certain degree of uniformity in size, and are within a certain range of size."

On June 20 last I visited Studland Bay for the purpose of carrying out some new experiments. I found that the musical patch emitted tones louder and more pronounced than I had ever heard them there before. The best results were obtained by drawing a thick deal rod, on to the end of which I had fixed a resonator, over the surface of the sand; sounds produced in this way were heard unmistakably for a considerable distance. The patch averaged 7½ yards in width, and ran parallel with the trend of the shore for some hundreds of yards. The sand on the sea side of the patch was fine, and emitted notes of a high pitch; that on the land side was coarse, and emitted notes of a lower pitch. The rod drawn across the patch gave, therefore, a great variety of pitch. Many other interesting facts cannot now be referred to, but it is important to state that some of this sand, when taken off the patch, and struck in a box, gave out notes as it did *in situ*. On trying this sand subsequently at home, the *coarse* emitted distinct notes of a low pitch, but the fine was mute. This was, so far as I know, the first time that the Studland sand had been musical *off the patch*.

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[1] Read before the Bournemouth Society of Natural Science.

According to my theory, if the number of grains with polished surfaces could be increased in this fine sand, the number of vibrations would increase also, and so intensify the note, and cause it to become audible; this could only be done, however, by introducing a certain percentage of grains fulfilling the required conditions. To obtain such grains and to introduce them gradually until the necessary number should have been added, would have been a tedious process; and it occurred to me then that the same result might be obtained if the sand were struck in a vessel with a hard and polished interior. I placed, therefore, this fine sand in a teacup, and on striking it, found that it emitted a high, shrill note (A in *altissimo*), which was far more intense than that given when it formed a part of the patch.

When polished grains of sand are in contact with the sides and bottom of a glazed porcelain vessel, it is obvious that there are numerous points of contact between two polished surfaces—the sand grains and the vessel—and that on striking the surface of the sand, the friction necessary to produce the vibrations of a musical note is induced between these points.

This I *proved* by placing the same sand in various vessels with rough interiors, and by lining these glazed or polished vessels with silk, &c., but in no case would this sand emit notes unless the grains were in direct contact with the glazed or polished surfaces. This peculiarity is not in any way dependent upon the sonorous properties of the vessel used, for it may be "deadened" with impunity, and the note will remain unaltered.

The results of numerous experiments show that musical sand of the Eigg type—*i.e.* sand possessing in great perfection the physical conditions necessary for the production of music—will be musical in receptacles of whatever composition or form, though in some of these it emits notes "under protest" only.[1]

Those sands which are of the Studland Bay type—*i.e.* having the necessary physical conditions less perfectly developed, and are usually mute except *in situ*—will emit music only in vessels possessing hard and glazed interiors, and, as a rule, of a certain form; while some of the more "sulky" types of sand not only need a vessel of hard and glazed interior, and definite form, but also require a box, or small pedestal of wood (which I call a "coaxer"), on which this vessel must stand before the notes emitted become audible. A "sulky" sand was rendered far more musical by being sifted, washed, and boiled, giving out, after this treatment, notes without the aid of the "coaxer."[2]

After discovering what could be done with such simple apparatus, it occurred to me to try, under similar conditions, some of my abandoned sands—those unmusical sands that had been, during a period of four or five years, treated unsuccessfully for music.

One sand (an iron-sand composed of more or less polished grains, quartz, and much dust formed of denser minerals) gave a very hopeful "swish" (explained in my paper of 1888) in a certain porcelain vessel, and from this—by (1) sifting in sieves, to eliminate the fine material, and to insure uniformity in size of grain; (2) rolling down an inclined plane of frosted glass, to separate the rounded grains from the angular quartz; and (3) boiling in dilute hydrochloric acid, to cleanse the surfaces—I succeeded in producing a sand that, in certain glazed vessels, emits musical notes as clear as those emitted from any of my musical sands but that of Eigg. This sand gives F in *altissimo*, but it very soon becomes "killed" because of the fine dust and loss of polish that is the inevitable result of the attrition of the grains. There remains but one thing to be done, and that is to produce a sand which, like that of Eigg, will be musical in almost any receptacle, and I have reason now to think that this will not be very difficult.

It has not been possible here to record more than the merest outline of what has been done, or to give instances of the interesting *capriciousness* of these sands; it should be understood, however, that no *ordinary* beach or cliff sand has the slightest inclination to "sing" under any of the "coaxing" methods at present known to me.

CECIL CARUS-WILSON.

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[1] When musical sands sound "under protest" they give out high, shrill notes. The smallest quantity of musical sand from which I can obtain a true note is a thimbleful of the Eigg sand. Small quantities emit notes of a high pitch.

[2] Many musical sands are quickly "killed" by constant striking, because the harder minerals present abrade the softer as they rub together, and this forms a fine dust.

NOTES.

SIR MICHAEL HICKS-BEACH, who previously gave a negative answer to the request made by the Executive Committee of the British Institute of Preventive Medicine, having reconsidered his decision, has now granted the required license to register the Institution as a Limited Liability Company, with the omission of the word "Limited." The license, however, is not to be construed as expressing approval by the President of the Board of Trade of experiments on animals, or in any way affecting the exercise by the Secretary of State of his discretionary powers to grant a vivisection license to the proposed Institute. The articles of association have been signed, and the Institute is now duly registered. The following gentlemen have already expressed their willingness to serve on the Council: Sir Joseph Lister, Chairman, Sir Charles A. Cameron, Mr. Watson Cheyne, Prof. Michael Foster, Prof. Greenfield, Prof. Victor Horsley, Sir William Roberts, Sir Henry Roscoe, Prof. Roy, Prof. Burdon-Sanderson, Dr. Pye-Smith. Dr. Armand Ruffer, of 19 Iddesleigh Mansions, Westminster, S.W., will act as honorary secretary until the first meeting of the Council.

THE graduation ceremony at the close of the summer session of the University of Edinburgh was held on Monday. Principal Sir William Muir, Vice-Chancellor, presided. Prof. Kirkpatrick presented for the honorary degree of Doctor of Laws Colonel Sir Colin Campbell Scott Moncrieff, K.C.M.G., C.S.I., R.E., remarking that, through his work as chief officer of the irrigation works of the Nile, it could be said that Sir Colin had created a greater and an infinitely freer, happier, and more prosperous Egypt than it was before. As a gallant officer, a distinguished man of science, a statesman of high merit, and, above all, as a benefactor of his fellow-creatures, Sir Colin was pre-eminently worthy of the highest of their academic honours. The honorary degree of Doctor of Law was then conferred *in absentiâ* on Prof. Simon Newcomb, Washington.

SIR JOSEPH FAYRER has been elected a Corresponding Member of the Royal Italian Society of Hygiene. Sir Joseph has also been promoted from the grade of Foreign Corresponding Member to that of Foreign Associate of the French Academy of Medicine.

PROF. DU BOIS-REYMOND, the distinguished physiologist of Berlin, has been awarded the Gold Medal for Science.

MR. J. E. KEELER has been elected Professor of Astrophysics in the Western University of Pennsylvania, and Director of the Allegheny Observatory. Mr. F. W. Very is associated with him as Adjunct Professor of Astronomy. It is expected that the Observatory will continue its researches on important problems in the domain of astro-physics.

IT is stated that Siam, following the example of Japan, is commencing to Europeanize her institutions. The founding of a University has been decided upon, and Prof. Haase, of Königsberg (Germany), has accepted the appointment to the Chair of Physics.

THE last number of the *Rendiconti* of the Reale Accademia dei Lincei contains an account of the annual meeting held on June 9, at which the King of Italy was present. After the opening speech of the President, Brioschi, one of the chief features was an admirable address by Prof. Messedaglia on the Homeric uranology, with special reference to precession.

La Revue Scientifique of the 1st instant contains the address by M. Villemin, the President of the Tuberculosis Congress. It deals with recent researches. The results of the first Congress are also detailed by M. Petit, the General Secretary.

A FINAL meeting of the Committee of the Virchow Testimonial Fund took place on July 16, Sir James Paget, Bart., F.R.S., in the chair. The Treasurer gave an account of the moneys received, which amounted to about £175. It was resolved to send this sum to the General Treasurer of the Fund, and to present Prof. Virchow on the occasion of his birthday with an illuminated address, conveying to him the congratulations of the Committee and subscribers. This the Honorary Secretaries, Dr. Semon and Mr. Horsley, were directed personally to transmit to Berlin on the occasion of the celebration.

THE Essex County Council has appointed an Organizing Joint Committee, consisting of six members of their own body and six members of the Essex Field Club, to form a centre for supplying lecturers and teachers (with apparatus and materials), conducting examinations, and affording help and guidance to local bodies, in connection with the recent grants towards technical instruction. A grant of £900 has been made for these purposes. The members of the Committee are: (representing the County Council) Mr. E. N. Buxton, Mr. E. A. Fitch, Mr. J. H. Burrows, Mr. S. W. Squier, Mr. F. West, and Mr. W. B. Whittingham; (for the Essex Field Club) Prof. Boulger, Mr. F. Chancellor, Prof. R. Meldola, F.R.S., Sir Henry E. Roscoe, M.P., F.R.S., Mr. F. W. Rudler, and Mr. J. C. Shenstone. The Organizing Secretary to the Committee is Mr. W. Cole, 35 New Broad Street, E.C.

THE idea of "a British Museum of Portraits," to be executed by photography, was conceived as long ago as 1864 by Mr. James Glaisher, F.R.S., and brought before a meeting of the Council of the Amateur Photographic Association, of which the Prince of Wales is the President. The suggestion was cordially approved by the meeting, and photographs were taken in *carte de visite* size and deposited at the South Kensington Museum. At first, however, only fading silver prints were made, and these were so unsatisfactory that for some years the undertaking was held in abeyance. By the discovery and perfection of the process of permanent carbon printing, an opportunity has at length been afforded of resuming the prosecution of the work under infinitely more favourable conditions; and, as a result, a collection of excellent portraits is now being made by the Amateur Photographic Association. Already there are nearly 200 large permanent carbon portraits deposited in the Art Department at the South Kensington Museum, and about as many more are ready to be sent. These latter were on exhibition at a private view on Saturday last at 58 Pall Mall, S.W., the studio of Mr. Arthur J. Melhuish (Photographer Royal). They embrace some photographs of men of distinction in science, and are excellent both as likenesses and as specimens of photographic art. The conditions under which they are taken are, in fact, sufficiently exacting to insure the production of a faithful portrait, inasmuch as every portrait must be approved by the sitter and by the Standing Committee previous to its being placed in the South Kensington Museum. The undertaking is on a non-commercial basis, the photographs being taken for the purposes of this collection only, and not for publication, and no expense of any kind being incurred by the sitter. The invitations to sitters are issued under the authority of the Council.

THE Trustees of the Indian Museum, Calcutta, have just issued the second and concluding portion of a Catalogue of the specimens of Mammals contained in that Institution. The first volume of the Catalogue, compiled by Dr. John Anderson, the late Superintendent, was published in 1881. The present volume, which commences with the Rodents, has been prepared by Mr. W. L. Sclater, the present Deputy-Superintendent. The total number of specimens of Mammals contained in the Indian Museum, as is shown in the Catalogue, is 4872. These are referred to 590 species, of which, 276 are found within the limits of the Indian Empire, and the remainder are from elsewhere. As the Indian Museum contains many types of Blyth, Jerdon, and the older Indian authorities, the collection is one of considerable importance, and the Catalogue will be of much use to students of the group of Mammals.

FOR the first time for many years the *Journal für Ornithologie* has actually appeared within the month imprinted on the cover bearing the date of publication. English ornithologists have this year received in July the Heft bearing the date 'Juli, 1891.' *Gott sei dank.* The articles published in the present year appear also to be of a higher class than many of those formerly issued in the *Journal*, and some very important papers by Dr. Reichenow, Dr. A. B. Meyer, Herren Schalow, Hartert, &c., have been published. The chief interest centres round the collections which that greatest of modern naturalist-explorers, Emin Pacha, has sent to Berlin; and the birds obtained by him during his journey from Bagamoyo to Lake Tanganyika are fully described by Dr. Reichenow. The novelties are not many, but are sufficient to show that there is much to be done in German East Africa before our knowledge of its ornithology approaches completion. English naturalists will await with eagerness the zoological work of our Consul in Mozambique, Mr. H. H. Johnston, C.B., for the whole of the district in his sphere of influence is practically unexplored as far as natural history is concerned, and at present our knowledge is almost a blank. To Mr. Johnston and his companions, therefore, English zoologists are now looking for information which shall connect the work of Böhm and Emin with that of Kirk and Livingstone.

IN a recent paper to the Société des Ingénieurs Civils, M. Haubtmann states that in London the cost of the electric "horse hour" is 0·375 francs, that is three times the cost of gas. In Paris it is 0·90 francs, and at Saint Brieuc, the town where, since June 1 last, it is cheapest in France, it is still 0·52 francs. At Fribourg it has the lowest cost in Europe, 0·15 francs, and 0·10 francs for a consumption over 20 horse-power. Such differences, he points out, do not arise from difference in cost of motor force, for, deducting that, the horse-hour still remains in Paris at 0·75 francs, while in Fribourg it is 0·125 francs. They arise from differences in the amounts of capital engaged, and in the systems adopted.

IT is stated that a memorial is about to be presented to the United States Congress asking for the creation of a Government Department of Public Health, with a Cabinet officer at its head, to be known as the Medical Secretary of Public Health.

THE Danish Academy of Sciences has recently offered the following among other prizes:—A gold medal, worth about £17, for an exposition of the theory of electric vibrations in limited and resting bodies in general, with a special application to simple forms of perfect conductors, so that for these cases, the mathematical problem may be explained, and if possible solved. A prize of about £22, for an investigation showing in the case of our four principal cereals, the nature, and as far as possible the proportional quantities, of the chief carbohydrates found at different stages of ripeness. Memoirs to be accompanied with preparations. A prize of about £27 for a complete account, accompanied with preparations, of the *Phytoptœcidia* found in Denmark, and a monographic exposition of the species of the genus *Phytoptus* (in its old and wider sense), which inhabit the various galls, found on a particular plant, with the view especially of showing whether several usually different galls of the same plant species arise from the same *Phytoptus* in different phases of its development. In choosing a plant, preference should be given for one in which these galls have an economic value, as is the case, *e.g.*, with some occurring on the beech. Further, the Academy desires an exposition, as complete as possible, of the development of a particular species of *Phytoptus*. The date for the first is October 31, 1892; for the two others October 31, 1893. Memoirs may be written in Danish, Swedish, English, German, French, or Latin.

THE fürstlich Jablonowsky Gesellschaft, recognizing the fact that the determinations of the secular perturbations of the orbits of the interior planets, in the form in which they have been left by Le Verrier, are not satisfactory, and that probably the anomaly in the motion of the perihelion of Mercury is to be explained by the fact that the differential equations have been treated linearally, offers a prize of 1000 marks for a new determination of the secular perturbations of the orbits of Mercury, Venus, the Earth, and Mars, in which the terms of a higher order are taken into account. Competitors are to send in the results of their investigations before November 1894, observing the usual rules to secure the anonymity of their papers.

THE *Educational Times* states that the Supreme Council of Hygiene of Austria has been engaged in discussing the advantages of erect as compared with slanting writing, and the official Report of Drs. von Reuss and Lorenz points strongly in favour of the former. They point out that the direction of the written characters has a marked influence on the position of the body. In "straight" writing the scholar faces his work, and is spared the twist of the body and neck, which is always observable in those who write slantwise, and one common cause of spinal curvature is thus obviated. The erect method is, therefore, expressly recommended for use in schools, in preference to the ordinary sloping lines.

WE have received the eighteenth Annual Report of the Geological and Natural History Survey of Minnesota. It consists of a summary statement for 1889, report of field observations made in 1888 and 1889, by N. H. Winchell; American opinion on the older rocks, by A. Winchell; additions to the library of the Survey since 1884, and a list of publications of the Survey.

L'Électricité points out that the new electric photophone, which consists of a small glow lamp at the end of an elastic tube used for throwing a strong light for surgical purposes into the mouth, ear, &c., was really suggested by the action of the water jet in the luminous fountains now so common, and that these really owe their origin to a laboratory experiment by M. Becquerel in 1876.

HERR KLENZE, we learn from a German source, has been making inquiry into the digestibility of different kinds of cheese. The most easily digested, he found, were Cheshire and Roquefort; while others are ranked as follows, in ascending order of difficult digestion: Emmenthal, Gorgonzola, Neuchâtel, Ramadour, Rotenburg, Mainz, fromage de Brie, and (most indigestible of all) Swiss cheese.

IN recent numbers of the *American Journal of Science* (February 1891) and *Ciel et Terre* (July 1 and 16, 1891) attention is drawn to the remarkable conclusions arrived at by Dr. Brückner in his work entitled "Klimaschwankungen"—the most complete work extant upon the question of the variation of climate—in which he shows that the climate has not undergone any continuous variation from the earliest historic time, but that it oscillates, and presents alternately periods of heat and cold, and of dryness and humidity, the period being about 35 years, which, it will be observed, is a multiple of the period of frequency of sun-spots (11 to 12 years). M. Penck, the eminent German geographer, has drawn some interesting conclusions as to the probable effects upon the harvests of the world.

PART 34 of Cassell's "New Popular Educator" has just been issued, and contains articles on applied mechanics, algebra, botany, electricity, and comparative anatomy.

MR. G. C. HOFFMANN, of the Geological and Natural History Survey of Canada, has made a microscopical and chemical examination of a peculiar form of metallic iron found on St. Joseph Island, Lake Huron. It appeared in the form of spherules disseminated through a thin deposit of dark reddish-brown limonite which coated certain faces of some surface specimens of quartz. These metallic-looking spherules were found to consist of nuclei of silicon coated with a humus-like substance, which in turn was overlain by a metallic layer containing all the elements most frequently met with in meteoric iron. But the small proportion of nickel present (0·11 per cent.), and the relatively large amount of phosphorus (1·07 per cent.), as also the fact that the spherules contain nuclei apparently of a concrete character, leads Mr. Hoffmann to suggest the possibility of a terrestrial source for the material, upon the assumption that it has resulted from the reduction of an iron-salt by organic matter. The paper, which is accompanied by four coloured plates, appears in the Transactions of the Royal Society of Canada, 1890.

THE preliminary results of some investigations upon the growth of the face are stated by Prof. G. M. West in *Science* for July 3. The values obtained in the case of measurements of the female face point to the existence of three distinct periods of growth, the first ending at about the seventh year, and the third beginning at about the age of fifteen. The abrupt transition from one period to the next is indicated by the very slow growth of some children until the ages of eight or fourteen, when a rapid development often occurs. From the fifth to the tenth year the average growth appears to be about 6·5 mm. During the next four years it is 6·2 mm., and from this time little advance is made, the maximum of 128 mm. being reached at about the age of twenty. The male face is larger than the female face at all ages. Its growth is also more rapid, and continues later in life. The measurements have been on 2500 persons, including both sexes.

PROF. TITO MARTINI, of Venice, contributes to the issue of the *Rivista Scientifico-Industriale* for the end of June, the results of some experiments on the crystallization of thin liquid films. He finds that a strong solution of sodium sulphate, when cooled to near its saturation point, possesses a viscous character which enables it to form a thin film on a metallic ring, as in Mr. Boys's experiments with soap-bubbles. On rapid evaporation such a film crystallizes to an extremely beautiful open lattice-work of minute crystals, which preserve their transparency for some time, and then effloresce and crumble to powder. The experiments succeeded with rings up to thirty-six millimetres diameter. Similar experiments with ammonium chloride and sodium hyposulphite have hitherto proved unsuccessful. With a transparent film of liquid sulphur, however, even more beautiful results have been obtained. The author regards such experiments, besides being eminently suitable for lecture demonstration, as likely to throw light on the nature of molecular arrangement in relation to crystallization.

The same number of the *Rivista* summarizes a somewhat important communication to the Naples Royal Academy of Physical and Mathematical Sciences, in which Prof. Dino Padelletti urges that the usual investigation for the movement of the plane of oscillation of Foucault's pendulum in relation to the earth's rotation is insufficient. The author contends that the problem for latitudes between the pole and equator is more difficult than would appear from the usual simple solution, and cannot be solved by the principle of inertia. He proposes an equation derived from the principle of composition of the rotational forces.

A meteorological journal in the Russian language, the *Meteorologitscheskij Westnik* (Messenger), has lately appeared under the competent editorship of Woeikof, Rykatschew, and Spindler; its general plan seems to be like that of the German *Zeitschrift*. The idea of starting it arose at a meeting of the Russian Naturalists and Physicians at St. Petersburg in the end of 1889. Four graphic tables are given in this journal, showing the course of the meteorological elements during 1889 at the agricultural experimental station of Sapolje, also measurements of ground temperature, &c.

The *Selborne Society's Magazine* for July contains the first of a series of articles on the Kew Museums by Mr. J. R. Jackson; others on the effects of environment on plants, and other interesting matter. Among the correspondence are complaints from Warwickshire that the Wild Birds Preservation Act is a dead letter there, as the "authorities," whoever they may be, take no trouble in the matter. On the other hand, the inhabitants of Shetland are fully alive to it.

The last volume (xxii., 6) of the *Trudy* of the Society of Naturalists of Kazan contains the second part of Mr. Korzchinsky's valuable researches into the northern limits of the black-earth steppe region of East Russia. In the first part published in 1888, the author gave the results of his explorations in the province of Kazan. He now confirms his conclusions by further exploration in Samara, Simbirsk, Perm, and Ufa. He gives the northern limits of the black-earth steppe vegetation, and shows that they depend neither upon climate nor upon the altitudes, but chiefly upon the courses of the rivers.

According to *La Nature*, the telephonic service of Paris, rapidly developing of late, will soon include an immense central telephonic office in the Rue Gutenberg, capable of serving directly 30,000 subscribers, without connection with the other offices of the quarter. The work is being actively pushed forward. Cables are being laid in the sewers, an enlargement of which, at certain points, is rendered necessary. There were 7800 subscribers in Paris last October. Paris has now telephonic communication with Brussels, Marseilles, Lyons (which also communicates with Marseilles), Lille, Havre, Rouen, and London. Twenty-eight towns in France have a telephonic system. There are two in Algeria, in Algiers and Oran. Lille and Roubaix, Lille and Dunkirk are connected by telephone; and, ere long, connection will be formed between Lille, Valenciennes, Calais, and Fourmies, between Lyons and Saint Etienne, between Dieppe and Rouen, between Marseilles and Nice.

The climate of the Greek island Cephalonia has been lately described by Dr. Partsch (*Petermann's Mitt.*). We note the following features. At Argostoli temperature reaches a maximum in July (25°·3 C.), whereas in Corfu and Patras it does so in August. With several days' calm and bright sunshine, in the bay, the air, laden with moisture, becomes unbearably hot and close. Yet the natives go but little to the wooded hills behind, where the temperature goes down sometimes to 15°·5 C. or lower. Mules bring down snow nightly, in summer, from covered pits in the hills, for supply of restaurants, &c. As to rain, there is a sharp contrast between the wet winter-half and the dry summer-half of the year. The annual rainfall (3½ years) was about 35 inches. The autumn rains are ushered in by severe thunderstorms. November and December are the wettest months, but about Christmas there is usually a short time of fine weather. March is extremely variable, and often very cold. With May begin the rainless months, and the drought is sometimes considerably over 100 days. Five months have sometimes passed with but a few slight showers. On this greatly depends the currant cultivation: a brief downpour may spoil the crop. Snow falls seldom in Argostoli, but often on the hills. Dew is plentiful in summer, but its salt precipitate is feared. Wind is greatest in winter, southerly winds prevailing, especially south-east. A hot south wind (the *lambaditta*) blows, rarely, in early summer, and with evil effects to vegetation. The fresh north-west wind (*maestro*) brings cumulus clouds on the hills.

Mr. F. Howard Collins, the author of a useful epitome of Mr. Herbert Spencer's system of philosophy, has written a pamphlet in which he discusses the causes of the diminution of the jaw in the civilized races. In opposition to the views of Weismann, he contends that the phenomenon is due to "disuse"; and the argument, as he presents it, deserves to be seriously considered. Some time ago Mr. Collins sent to Nature a letter in which he gave some account of the ideas which he now expounds more fully. In the preface to his pamphlet he seems to imply that the letter was not inserted because, according to a belief said to be current among certain biologists, the editor of Nature is "more willing to publish letters contending that acquired faculties are not inherited than those contending that they are." Mr. Collins has too readily allowed himself to be influenced by the belief of "certain biologists." If he supposes that it is possible for the editor of Nature to print all the letters sent to the paper for publication, he must have a very inadequate conception of an editor's functions.

To throw light on some physiological processes, Herr Hofmeister recently experimented (*Archiv für experim. Pathol.*) on the swelling of plates of gelatine in various solutions; the plates being taken out from time to time, dried, and weighed. With salt solutions of various concentration, the gain of weight was large in the first days, then gradually fell off, as in former experiments with pure water. The effect varied with the nature of the salt; and even with solutions holding the same number of molecules in 1000 parts water, the swelling varied as much as five-fold. This difference, it is pointed out, is related to attraction of the salt for water; the greater the attraction, the more difficult the entrance of water into the plate. But that this is not the only factor is proved by the swelling in pure water being always much less than that in the solutions. Experimenting with ordinary salt, the gain of weight proved to consist both of water and salt, both dependent (but differently) on concentration. With increase of the latter, the gain of water rises to a maximum (about 13 per cent.), then declines; but the gain of salt goes on always increasing proportionally to the concentration. The remarkable property salts have of increasing the gain of water beyond what occurs in pure water is also shown by indifferent organic substances, as cane-sugar and alcohol. Experiments were further made on swelling of gelatine plates in methyl-violet solutions, and with the result that the concentration of the solution in the plates was always much greater (over 30 times) than that in the solution presented. The colouring-matter is taken up in relatively much greater quantity than the water. Further, gelatine takes up somewhat more colouring-matter relatively from a dilute than from a concentrated solution. The forces concerned in these phenomena, and which are neither purely mechanical nor chemical, Herr Hofmeister brings into analogy with those occurring in absorption of gases by liquids, the reciprocal solution of liquids, adsorption of gases on solid bodies, &c.

The *Photographic News* quotes the following from the *Scientific American*, December 9, 1848:—"*New Electrical Light.*—The inventors of a new electrical light, exhibited at the Western Literary Institution, Leicester Square, London, on its recent reopening under the new auspices, expect, it is said, to apply it generally to shop and street illumination, and they state that, while the conveying will cost no more than gas, the expense of illumination will be one-twelfth the price of the latter light. The current of electricity, in passing through the two pieces of charcoal which form the poles of the circuit, and are excluded from all access of air, gives, in this case, it is said, an intense and beautiful white light, with the effect of daylight, to a much greater extent than the lime does, and having this advantage, that it is sustained and continuous. If Messrs. Staite and Petrie can thus produce a steady and sustained light they have accomplished what has hitherto been the sole preventive to the substitution of galvanism for gas. The *Mechanics' Magazine* states that this one light completely eclipsed ten gas lights and an oxyhydrogen. The gas companies had better look out. The dissatisfaction of the public with their mismanagement may have begotten a rival destined to eclipse many more than merely ten of their gas lights."

With the view of certifying to the efficiency of teachers of public elementary schools to give instruction in woodwork in accordance with the provisions of the Code (1890), the City and Guilds of London Institute is prepared to issue certificates to qualified teachers of public elementary schools on the following conditions:—The candidates will be required to give evidence of having regularly attended during each of two sessions, a course of at least twenty practical wood-working lessons in a school or class certified by, and under an instructor approved by, the Institute. The candidates will further be required to pass an examination at the end of each year's course, to be conducted by examiners appointed by the Institute, and to pay a fee of five shillings for each examination. For the first year, candidates who have attended an advanced course of instruction will be exceptionally admitted to the second year's examination without having passed the first, and will be eligible for the teacher's certificate. The examination fee for such candidates will be ten shillings. The written examination will include questions founded on such subjects as the following:—*Woods.*—Places from which some of the commoner woods are obtained. Their characteristic properties and uses. The general structure of cone-bearing and leafy timber trees. The meaning of seasoning timber. Effects of shrinkage and warping. Identification of specimens of wood. The questions will be limited to oak, ash, elm, beech, mahogany, sycamore, basswood, white deal (spruce), red pine (Scotch fir), yellow pine.

Das Wetter for July reports a curious case of globular lightning which occurred at Berga, near Schlieben, in Germany, between 3 and 4 o'clock on the morning of July 1. The lightning entered the chimney and split into two parts, one portion ran along the rafters of the roof, and the other entered a bed-room occupied by a man with his wife and three children. The man, who was up, on account of the violence of the storm, saw the ball jump on to the bedstead, which it broke, and from there it slowly travelled to the opposite side of the room, and disappeared, with a loud crash, through the wall. None of the occupants were injured, further than being deafened for a short time.

The additions to the Zoological Society's Gardens during the past week include a Banded Ichneumon (*Herpestes fasciatus*) from West Africa, presented by Dr. Arthur Williams; a Black Stork (*Ciconia nigra*), European, presented by Lord Lilford, F.Z.S.; two Nilotic Crocodiles (*Crocodilus vulgaris*) from Africa, presented by Dr. Lester; two Black Storks (*Ciconia nigra*), European, two King Parrakeets (*Aprosmictus scapulatus*) from New South Wales, purchased; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, deposited.

OUR ASTRONOMICAL COLUMN.

Researches on the Mean Density of the Earth.—The *Monthly Notices of the Royal Astronomical Society* for June contain a brief account by Prof. A. Cornu of the experiments M. Baille and himself have been making for some years to determine the mean density of the earth. The apparatus employed is fundamentally the same as that used by Cavendish. It consists of a horizontal aluminium rod, suspended by a torsion thread 4 metres long, carrying at each end a ball of copper, bismuth, iron, or platinum, and at its centre a vertical mirror reflecting the divisions on a millimetre-scale 5 metres away. Two globes of mercury are used to produce the torsion couple. The displacements of the scale-divisions are observed with a telescope, and indicate the angular displacements of the rod. The chief improvements which have been made upon the apparatus used by Cavendish, Baily, and Reich, are as follows:—(1) The length of rod connecting the suspended balls has been reduced to 0·50 metre, *i.e.* to a quarter the length adopted by the above-named observers. (2) The attracting masses have been reduced to 10 kilogrammes. Cavendish used masses weighing more than 140 pounds. And the method of using fixed globes which can be quickly filled with mercury has been advantageously substituted for the movable lead weights. (3) The complete oscillation of the balance arm is registered on a chronograph by observing and recording the transits of the reflected scale divisions. (4) The use of an annealed glass fibre to eliminate errors due to displacements of the zero point. (5) The screening from variations of electric potential by putting all parts of the apparatus in metallic connection with the earth. (6) The copper case protecting the balance arm is a good conductor of heat, and of sufficient thickness to eliminate the disturbances due to variations in temperature. The authors hope soon to obtain an estimation of the probable error of their measures, and to arrive at a definite result for the constant they are determining.

Parallax of P Ursæ Majoris.—Vol. xxxviii. of the "Astronomical Observations of the University Observatory of Königsberg" contains the heliometer observations of P Ursæ Majoris (Arg.-Oeltzen 11677) made by Dr. Julius Franz, from which he deduces the parallax 0″·1002 ± 0″·0065, or approximately 0″·10 ± 0″·01.

THE PROGRESS OF MEDICINE.

The Bournemouth meeting of the British Medical Association has been a great success, and a great deal of useful work and discussion has been recorded. Among the addresses we may refer to the President's (Dr. J. R. Thomson), on the present position of medical officers of health; of Dr. Lauder Brunton, on twenty-five years of medical progress; of Dr. J. Chiene, on rest as a therapeutic agent in surgery; and others on lunacy legislation, the uses and prospects of pathology, &c.

We make the following extracts from Dr. Brunton's address, which presents us with a most admirable and masterly analysis of recent progress:—

. . . Perhaps there is no period in the whole history of medicine in which such rapid changes have taken place as in the last five-and-twenty years. It is impossible to give anything like a complete account of these in the brief space of one hour, and I shall therefore restrict myself to a few of the more prominent points, and especially those that have come directly under my personal cognizance; for, like the man who made one-half of his fortune by attending to his own affairs and the other half by leaving other people's alone, I may probably utilize the time at my disposal best by speaking of what I know myself and leaving other things out.

Advances in Knowledge and Teaching due to Experimental Method.—These changes have occurred both in the profession itself and also to some extent—in this country at least—in the education and training of the men who enter it. We notice, first, that a very great increase has occurred in the knowledge of the nature, causation, and treatment of diseases possessed by the profession as a whole, but perhaps a still greater gain is the general adoption of the experimental method by which most of our recent knowledge has been acquired, and from which we may hope for even greater advantages in the future. In correspondence with the acquirement of knowledge, we notice also a great alteration in the teaching of medicine, and especially prominent is the tendency to make such teaching practical instead of theoretical by training men to place their dependence upon objective facts, and not to receive without experimental data the theories or speculations of any master, however great he may be. . . .

Direction of Advance.—The greatest advance made in the last twenty-five years has been in the direction of the accumulation, co-ordination, and teaching of facts instead of theories, of the phenomena of Nature as opposed to the fancies of the human mind.

Co-ordination of Facts.—But the mere accumulation of facts is of little use unless they can be so arranged, compared, and grouped as to bring them into relationship with some general law, and this we find in the world's history has been done from time to time by some master-mind. . . .

Influence of Darwin.—Medicine, both in its principles and practice, is really a subdivision of biology, and this, like all other branches of knowledge, has been most profoundly modified by the general acceptance of Darwin's great thoughts—the doctrine of evolution, the struggle for existence, and the survival of the fittest. Wherever we turn we find that Darwin's influence has modified the direction of thought, and whether the study concerns the evolution of the elements, the evolution of the planetary systems, of living beings, of communities, of customs, of laws, of literature, science, or art, in every department of human knowledge we find that men, consciously or unconsciously, are influenced by Darwin's work. It is with shame I confess that five-and-twenty years ago, although I had taken a University degree not only in medicine but in science, and might therefore be supposed to be acquainted with his work, I did not even know of the existence of his "Origin of Species," and I first heard its name in Vienna from the lips of an Austrian who was speaking of it in terms of the highest praise. "What is it?" I asked, and my question then seemed to cause my foreign friend as much astonishment as it causes myself now, when the possibility of such ignorance seems to me, as it must to you, almost incredible, and yet such was the fact. The publication of Darwin's "Origin of Species," in 1859, has done more to change the current of human thought than anything else for centuries, but while its influence is everywhere felt, biology and all its subdivisions have been more especially affected.

Changes in Medical Students.—But great as the changes have been during the last five-and-twenty years in the profession itself, they are perhaps quite as great in the men who enter it. . . .

Long ago the doctor's means of diagnosis consisted in inspecting the tongue, feeling the skin, counting the pulse, shaking the urine, and looking at the motions and the sputum. But now, in addition to a thorough training in auscultation and percussion, students have to learn the use of the laryngoscope, ophthalmoscope, and otoscope, and the application of electricity. They have to acquire a knowledge of the chemistry of the urine and its alterations in disease, and, what takes still more time, they have to learn the microscopical appearances, not only of the tissues and excretions in health, but their alterations in disease, and must be acquainted with the methods of staining so as to detect tubercle bacilli and other disease germs. . . .

Departments of Greatest Advance.—Five-and-twenty years ago we knew only too well that typhus was infectious, and that pyæmia and erysipelas were likely to spread in a ward when once they got into it, but we did not know then the causes of these diseases as we do now, nor had we the same means at our disposal wherewith to combat them. The departments in which the greatest advances have been made within the last five-and-twenty years are in those of fevers and diseases of the nervous system. A new era in the study of the latter was foreshadowed by the experiments of Fritsch and Hitzig on the brain of the dog, but it can only be said to have fairly begun with Ferrier's localization of the cortical centres, both motor and sensory, in the brain of monkeys. For the brain of the dog was too unlike that of man for experiments upon it to be of much practical use in the diagnosis of human ailments, while the likeness in the brain of the monkey to that of man at once allowed conclusions drawn from the experiments upon the former to be transferred upon the latter. Yet if we try to describe in one word the department in which medicine has made the greatest progress within the last quarter of a century, that word must be "fevers"; for during this time we have learned to recognize fever by the use of the thermometer in a way we never did before; we have learned the dependence of the febrile process in the great majority of cases upon the presence of microbes in the organism, and we have become acquainted with an immense number of chemical substances which have the power both to destroy the microbes and to regulate the febrile process.

Introduction of the Thermometer.—It is true that the thermometer was used by Danielssen, in leprosy, before the year 1848, and its more general use began with Wunderlich's observations nearly thirty years ago, but it is only within the last five-and-twenty years that its use has become at all general. . . .

Nature of Fever.—The thermometer has not only enabled us to detect the onset and to watch the progress of fever, but in conjunction with microscopical research, physiological experiment, and chemical analysis it has enabled us to gain a fuller knowledge of the nature of the febrile process itself. We know that during it the organism is consuming rapidly, or, as Dr. Donald MacAlister graphically says, it is like "a candle burning at both ends," and we have learned scientifically the reasons for the practical treatment, of which Graves was so proud that he wrote as his own epitaph, "He fed fevers." We have learned also, to a great extent, the necessity for the elimination of the waste products, or ashes as we may term them, which the excessive combustion produces, and thus we know why the surgeon is so anxious regarding the result of an operation when the kidneys of his patient are inadequate. For if any febrile attack following the operation should lead to increased demands upon these secreting powers, they might fail to meet it, and the retained excreta would poison the patient.

New Methods.—The rapid increase in our knowledge has been due not merely to the constant use of old methods, but to the introduction of new ones, and more especially to the general recognition of the fact that the same strategy which has often proved so successful in war is to be applied in attacking complex problems. They are to be separated as far as possible into their several components, and each of these is to be overcome in detail. As presented to us by observation at the bedside, the problems of disease are too complex for us to solve, and we are only succeeding in doing it by examining the various factors one by one in the laboratory. The greatly increased powers of the microscope and the better methods of illumination have been of the greatest service, but their utility would be very much less than it is had it not been for the general introduction of the microtome and the invention of new methods of staining. When I was a student the microtome was only used for cutting sections of wood in the class of practical botany. About that time it was employed by Mr. Stirling, Prof. Goodsir's assistant, in the preparation of animal tissues, but I believe that we owe its general introduction to Prof. Rutherford. The facility with which sections are made by it has made microscopical research much less tedious, and has enabled trained histologists to do more work in a given time, and medical students to acquire knowledge more rapidly. But without the method of staining introduced by Weigert and Ehrlich, we should, even with the best microscopes, be unable to recognize most of the microbes which are so important in the causation of disease.

Good Out of Evil.—It is very interesting to see how good may come out of evil, and a striking illustration of this is afforded by the history of medicine in the period we are now considering. For it seems to me that we can trace a great part of our knowledge of disease germs and of the antiseptic remedies we use in treatment to the cupidity and stupidity of the Spaniards of the Cordilleras. Their cupidity led them to cut down the cinchona trees of the Andes in order to fill their pockets with the gold they received in exchange for the precious bark, while their stupidity prevented them from planting new trees to replace those which they felled. The consequence of this was that quinine became so dear that it was evident that anyone who could produce it artificially would make his fortune. Amongst others, Perkins tried to do this, and, although he failed, yet in the attempt he discovered the anilin dyes, whose staining powers have not only helped us so much in ordinary histological research, but have made it possible to distinguish disease germs which without them would have been invisible. But the discovery of the anilin colours was only one outcome of the attempt to make quinine synthetically, for the impulse which it gave to the study of aromatic compounds has led to the production of salicylic acid and acetanilide, antipyrin, phenacetin, and all the other antipyretic remedies whose number is probably legion, and whose names already have become so numerous as to be troublesome. Here we see good has arisen out of evil; for if the price of quinine had not been so high, the researches which have proved so useful might not have been begun even yet.

Small and Great, Foolish and Wise.—In looking at another of the greatest advances which medicine has made—namely, the knowledge of infective disease—we can see how enormous results can arise out of very small beginnings, and the safety of nations may be consequent upon a research which many men would have termed useless or even frivolous. I can hardly fancy any better illustration of St. Paul's observation about the foolish things of this world confounding the wise than Pasteur's researches on tartaric acid; for what could seem more foolish to the so-called practical man than the question, "Why does a crystal of tartaric acid sometimes take one shape and sometimes another?" Yet from an attempt to answer this question has arisen the whole of Pasteur's work on fermentation in general, and on that of wine, beer, and vinegar in particular, whereby he has been able to save millions to his country by accelerating the production of vinegar and preventing the souring of wine and beer. His observation that tartaric acid sometimes turned the ray of polarization to the right, sometimes to the left; that, indeed, there were two crystals apparently alike, but really different; and that these could be combined so as to form a symmetrical crystal having no power of rotation, led him to look to life and living beings as the source of asymmetry. He tried to produce this asymmetry in salts of tartaric acid by fermentation, and found that during the process an organism developed which eats up the dextro-tartaric acid, and leaves the lævo-tartaric acid behind. This led him to investigate such minute organisms, and, by simplifying the soil in which they grew, and separating the organisms one from another, he learned the conditions of their growth, and showed that most processes of fermentation were due to the presence of living organisms. It is true that while Pasteur was still a boy at school, Peyen and Persoz had shown that the liquefaction of starch and its conversion into sugar was due to diastase, and that Dumas in a report on a paper by Guérin-Varry had pointed out that, although unlike diastase, the active principle of the gastric juice had not been isolated, it was probably a ferment of a somewhat similar kind. Dumas classed yeast as a ferment along with diastase, and the fact that such a process as conversion of starch into sugar could be effected without a living organism naturally rendered it all the more difficult for Pasteur to prove his thesis that most fermentations were due to living organisms.

Chemical and Biological Views of Fermentation.—The two views of the action of ferments—namely, the chemical and the biological—may, I think, fitly be likened to Pasteur's two kinds of tartaric acid, each by itself being lopsided and incomplete, forming a symmetrical whole only when united. There can be no doubt of the truth of the chemical view that diastase is not a living organism, and yet converts starch into sugar. There can be as little doubt of the biological view that yeast and other organisms which cause fermentation are living bodies, and that without the presence of these living bodies alcoholic, acetic, and other forms of fermentation would not exist.

Microbes and Enzymes.—But recently we have come to recognize that these living organisms may produce their effect by manufacturing chemical ferments, and that these ferments may occasionally do the work, although the organisms which form them may be absent. It is quite true that it is difficult—perhaps impossible—to get fermentation from the dead yeast plant, but we may find a parallel for this in the fact that the pancreas of the higher animals sometimes yields an active ferment and sometimes not. Nor need we wonder that the ferments produced by microbes have but a slight action compared with those of the microbes themselves, if we remember how very little power of digestion a dead pig's stomach has as compared with the amount which can be digested not by the live animal itself only, but by the herds of swine consisting of its "fathers and mothers, its brothers and sisters, its cousins and its aunts," during all the term of their natural lives; for in the process of fermentation microbes are growing, fermenting, and dying with great rapidity, and many generations occur in a fermenting fluid in the space of a few hours, so that the total effect they produce will be out of all proportion to any which can be got from the microbes themselves at a single instant.

Microbes and Disease.—From organisms as a cause of fermentation and of the diseases of wine and beer, Pasteur went on to investigate their action as causes of disease in living beings—first in the silkworm, next in the lower animals, and, lastly, in man. He established the dependence of the silkworm disease and of anthrax upon the presence of specific microbes which could be transmitted and communicate the disease, and by destroying the infected eggs of the silkworm he eradicated the disease and restored the silk industry to France.

Weakening of Disease Germs.—But while this investigation is interesting to us as illustrating the probable cause of the disappearance of typhus fever, to which I have already alluded, Pasteur's researches on anthrax are still more important as bearing upon the question of protective inoculation; for he found that the disease germ could be cultivated outside the living body and grown in flasks under varying conditions, some of which were favourable and others unfavourable to its growth. High temperature enfeebled the virus, so that it no longer killed an animal with the same certainty, and by inoculating first with a weak virus and then with one successively stronger and stronger, he found that animals could be completely protected either from inoculation by the strongest virus or by infection from other animals suffering from the actual disease.

Increase in Virulence of Disease Germs.—Another extraordinary fact which he made out was that the virus thus weakened, so that it will not kill a guinea-pig a year old, and still less a sheep or ox, may again be rendered most potent by inoculating a feeble animal, such as a guinea-pig a day or two old, from this older and stronger guinea-pig's, the strength of the disease germs increasing with every inoculation, until finally sheep and cows may be killed by it. We can thus see how an epidemic of disease beginning sporadically, and attacking weak individuals, may gradually acquire such strength as to attack and carry off the strongest.

Pure Cultures.—Pasteur's plan of growing disease germs outside the body in broth, although of the utmost value, did not allow a convenient separation of different germs; but this can now readily be done by Koch's plan of sowing them, not in a liquid medium, but on solid gelatine spread on glass plates, so that the growth of the germs can be daily watched under the microscope, and inoculations made from single colonies on other plates until pure cultures have been obtained. By thus isolating the different microbes, we learn their life-history, the mode in which their growth is influenced by differences of soil, of temperature, of moisture, by the addition of various substances which either favour or retard their growth, and, last but not least, the effect which one microbe has upon another when they are grown together at the same time.

Struggle for Existence amongst Microbes.—For even amongst these minute organisms the struggle for existence and the survival of the fittest exists, like that which Darwin pointed out so clearly in the case of higher plants and animals. . . .

Struggle for Existence between Microbes and the Organism.—But it is not merely between different species of microbes or different cells in an organism that this struggle occurs. It takes place also between the disease germs and the cells of the organism which they invade, and the result of the struggle may be determined, not by some powerful agency which weakens or destroys either the organism or the microbe, but by some little thing which simply inclines the scale in favour of one or the other. Thus, in the potato disease, the victory of the invading microbe and the destruction of the potato, or the death of the microbe and the health of the tuber, may depend upon some condition of moisture or possibly of electrical change in the atmosphere which aids the growth of the microbe disproportionately to that of the potato. These atmospheric conditions need not necessarily be antagonistic to the potato, they may even in themselves be advantageous to it; but if they help the microbe more than the plant, the microbe will gain the victory and the plant be destroyed.

Fight between Cells in Higher Organisms.—The fight between the organs which Æsop describes in his fables actually occurs between the cells in some vertebrate animals, and the schism predicted by St. Paul as the result of such a fight actually takes place. For in the tadpole, at one stage of its existence some of the cells at the base of the tail begin to eat up others, with the result that schism occurs and the tail falls off.

Phagocytosis.—This struggle for existence between the cells of an organism and microbes has been beautifully shown by Metschnikoff in the Daphne or water flea, where the process of the cells eating up the microbes or the microbes destroying the cells can be actually observed under the microscope. This process of phagocytosis is now regarded by many as only a small part of the struggle between an organism and a microbe, but it is impossible to see one part of a microbe half digested by the cell in which it is embedded, while the part outside remains unaltered, without believing that the process is one of great importance. At the same time, it seems that the process of phagocytosis, where the microbe and the cells meet in close conflict, bears about the same relationship to the total struggle that a bayonet charge bears to a modern battle. The main part of the fight is really carried on at some distance by deadly weapons—by bullets in the case of the soldier, and by ferments, poisonous albumoses, and alkaloids on the part of the cells and the microbes. In some of Metschnikoff's observations we can almost see this process, for he has figured leucocytes dead, and apparently burst by the action of conidia, lying close to but yet outside them, as if these conidia, like the dragons of fable, had spit out some venom which had destroyed them.

Venom of Microbes.—Within the last few years attention has been gradually becoming directed less to microscopical examination of the microbes themselves and more to chemical investigation of the ferments and poisons which they produce; yet, strangely enough, the very moment when chemistry is becoming more important than ever has been chosen to minimize the teaching of it in medical schools, and examination in it by licensing bodies. It is now possible to separate the albumoses and poisons from the microbes which produce them either by filtration, or by destroying the microbes by graduated heat; for, as a rule, they are destroyed by a lower temperature than the albumose or poisons which they form.

Microbes and Enzymes.—As the albumoses produced by microbes are nearly allied, chemically and physiologically, to those formed in the alimentary canal of the higher animals by digestive ferments, it is natural to suppose that microbes, like the higher animals, split up proteids, starches, and sugars by enzymes, which they secrete, and which in both cases may be obtained apart from the living organisms which produce them; that, in fact, we should be able to isolate from microbes bodies which correspond to pepsin or trypsin, just as we can isolate these from the stomach or pancreas of an animal. In some, although not in all cases, this attempt has succeeded.[1]

Poisonous Albumoses.—The albumoses produced by microbes resemble those formed during normal digestion in being poisonous when injected directly into the circulation, although they may not be so greatly absorbed from the intestinal canal. One of the most remarkable discoveries in regard to albuminous bodies is the fact that some of them which are perfectly innocuous, and, indeed, probably advantageous to the organism in their own place, become most deadly poisons when they get out of it. Thus, the thyroid and thymus glands, which are perfectly harmless and probably useful, were found by Wooldridge, when broken up in water, to yield a proteid which instantaneously coagulated the blood if injected into a vein, so that the animal died as if struck by lightning; while Schmidt-Mühlheim, under Ludwig's direction, found that peptones had an exactly opposite effect, and prevented coagulation altogether.

Neutralization of Poisonous Albumoses.—Perhaps the analogy is too vague, but we seem to find here something very like Pasteur's two kinds of tartaric acid, one rotating polarized light to the right, the other to the left; but, when united together, having no action at all, for here we have two bodies, one of which destroys coagulability entirely, the other increases it enormously; while many albuminous bodies have no action upon coagulation whatever. This view would lead us to suppose that one form of albumose may neutralize the action of another, thus rendering them both completely innocuous, whilst either one or other alone might be a deadly poison. The albumoses formed by microbes appear frequently, if not always, to have a double action, destructive and protective, on the higher animals. Pasteur's treatment of hydrophobia is based on the idea that the spinal cord of rabid animals contains a virus, and its antidote—Koch's tuberculin—may be similar in this respect, and may yet, by suitable alterations, fulfil the hopes of its able and single-minded discoverer.

Zymogens and Enzymes.—Perhaps a similar process of splitting up and recombination may explain the formation and disappearance of the enzymes, such as pepsin and trypsin, by which digestion is carried on. The pancreas of a fasting animal will not digest albuminous bodies like fibrin, while the pancreas of an animal killed during full digestion will do so rapidly. Yet the fasting pancreas contains the zymogen, or mother substance, which yields the digestive ferment, and, as Kühne has shown, by treating it first with acid and then with alkali, it becomes active. Again, to recur to the analogy of Pasteur's tartaric acid, we seem to find that the inactive, and possibly symmetrical, albuminous substance of the fasting pancreas is split up by this treatment after death or during the process of digestion in life, and yields the lopsided and active pancreatic ferment. But, if this be so, what becomes of the other half which has been split off? We do not at present know, but curiously enough Lépine has lately shown that while the pancreas is pouring into the digestive canal a ferment which will form sugar, it is at the same time pouring into the circulation another ferment which will destroy sugar.

Immunity.—We must be very careful in our speculations, and test them by experiment, but such observations as these may tend to throw some light upon the nature of immunity. Immunity is probably a very complex condition, and is not dependent altogether upon any single factor, but we can now understand that if a microbe has gained an entrance into an organism, and produces a proteid or an albumose poisonous to the organism which it enters, it may grow, thrive, and destroy that organism, while the injection of some other proteid which would neutralize the poison might save the animal while the microbe would perish.

Cure of Anthrax.—Thus Hankin has found that, while a mouse inoculated with anthrax will die within twenty-four hours, a rat resists the poison altogether; but if the mouse after being inoculated with the disease has a few drops of rat's serum injected into it, instead of dying, as it would otherwise certainly do, it survives just like the rat, and from the spleen of the rat Hankin has isolated a proteid which has a similar protective action to that of the serum.

Cure for Tubercle.—Working on similar lines, Bernheim and Lépine used the injection of goat's blood in phthisis so as to stop, if possible, the progress of tubercle, and Richet has used the serum of dog's blood, for the goat is quite immune, and the dog is to a great extent, though not entirely, immune from attacks of tuberculosis. The injection of goat's blood in somewhat large quantities has been given up, while dog's and goat's serum in small quantities of 15 to 20 minims at intervals of several days is still under trial.

Action of Blisters.—But if immunity can be insured by such slight changes in the organism as a few drops of serum from a rat will produce in the body of a mouse, it is natural to suppose that a similar change might possibly be effected by removing the albuminous substance from one part of the body, and introducing it, perhaps after it has undergone slight change, into another. As I have already mentioned, the albumoses of ordinary digestion are poisonous when they are injected into the circulation, and so are the proteid substances obtained from the thyroid and thymus glands. Why, then, may not the serum of one's own blood, withdrawn from the vessels by a blister and reabsorbed again, not be as good as the serum obtained from the blood of an animal? . . .

Bleeding.—It is quite possible, too, that the good effects of bleeding may be due to a similar cause. . .

Speculation and Experiment.—The human body is a most complex piece of mechanism. We learn its action bit by bit very slowly indeed, and we are only too apt to regard the little piece which attracts attention at the moment as all-important and to leave the other parts out of sight. But this is not true of our study of the body only, for the same tendency manifests itself in the pursuit of knowledge of all kinds, yet it is in medicine more especially that this tendency comes to be a matter of life or death, for upon the medical view prevailing at the moment medical practice is apt to depend, and erroneous views may lead to the death of many patients. So long as practice depends upon theories, unchecked by experiment, so long will medical practice prove fluctuating, uncertain, and dangerous. One of the greatest gains of the last five-and-twenty years is the general introduction of the experimental method, and the habit which has been growing up during it of accepting no statement unless based upon experimental data. Speculations such as those in which I have been indulging in regard to blisters and blood-letting are useful as indicating lines of experimental research, but until these have been thus tested it is foolish and may be dangerous either to accept and act upon them as true or to scout them entirely as false and absurd. Imperfect knowledge is almost sure to lead to one-sided practice, and thus, diverging further and further from the truth, ends at last in falsehood and folly.

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[1] *Vide* Brunton and Macfayden, Croonian Lectures on "Chemical Structure and Physiological Action," *British Medical Journal*, June 15, 1889, p. 1336.

Antisepsis.—Perhaps no better example of this can be found than antiseptic surgery, from the time of the good Samaritan down to Ambroise Paré and Sir Joseph Lister. The good Samaritan bound up the wounds of the poor traveller, pouring in oil and wine, which, only a few years ago, was recommended in an Italian journal as an excellent antiseptic. Ambroise Paré, when his ointments ran out, could not sleep for thinking of the miserable soldiers to whom they had not been applied, and was greatly astonished to find in the morning that these wretched neglected ones were better and happier than their comrades who had been treated *secundum artem.* I have no doubt that Paré's predecessors, in trying to improve upon the methods of the good Samaritan and upon the still useful friars' balsam, which is a powerful antiseptic but stings the wound or sore, had tried to make their applications more and more irritating, not knowing that it was the antiseptic power and not the irritant qualities which were desired. Paré abolished the ointments with the irrritation they caused, and thus did great service to surgery. But a greater one yet was rendered by Lister when he recognized that the danger of operations was due to the entrance of germs, and by preventing this has completely revolutionized surgical practice; nay, more, he has to a great extent revolutionized medicine, for the diseases of the internal organs, which were formerly entirely under the physician's care, are now becoming amenable to surgical treatment, and diseases of the stomach, intestine, liver, kidney, and lungs, and even of the brain and spinal cord, are now successfully treated by surgery when medicines are powerless to help. The most remarkable of all the recent triumphs of surgical operations upon the brain in which Mr. Horsley has gained such well deserved fame, would have been impossible without Ferrier's localization of cortical centres, and would have been equally impossible but for Lister's antiseptic method.

Disinfection.—But it is not only in surgery that recognition of diseased germs as a source of danger to the organism has led to their destruction outside the body, and insured safety from their attack. This occurs in all infective diseases, and this term now includes many which were not formerly regarded as such, for neither consumption nor pneumonia was formerly regarded in this light; but just about twenty-five years ago tubercle was shown to be inoculable, and since then the discovery of the bacillus of tubercle by Koch, and of pneumonia by Friedländer, has caused us to class both these diseases as not only infective, but as caused by definite organisms.

Prevention of Epidemic Diseases.—So long as people were ignorant of the causes of epidemic diseases, they were utterly unable to combat them, and they either in fury slew defenceless people for poisoning the wells, as in the Middle Ages, or appointed days of fasting and prayer, as in our own times. But once an epidemic is known to depend upon the presence of a certain organism, precautions can be taken for destroying the organism outside the body by means of disinfectants, or for lessening the susceptibility of the organism to its ravages inside the body by inoculation, or combating its effects by means of antipyretics. A knowledge of the life-history of microbes has enabled us to ascertain the power of different substances, either to destroy them completely or to arrest or retard their germination and growth, and in this way to prevent the occurrence of the diseases which these microbes might otherwise produce. . . .

Antivivisection.—Every now and again a loud outcry is raised against this method, partly from ignorance and partly from prejudice. Many—probably most—of the opponents of experiments on animals are good, honest, kind-hearted people, who mean well, but either forget that man has rights against animals as well as animals against man, or are misled by the false statements of the other class. These are persons who, blinded by prejudice, regard human life and human suffering as of small importance compared with those of animals, who deny that a man is better than many sparrows, and who, to the question that was put of old, "How much, then, is a man better than a sheep?" would return the reply, "He is no better at all." Such people bring unfounded charges of cruelty against those who are striving, to the best of their ability, to lessen the pains of disease both in man and also in animals, for they, like us, are liable to disease, and, like us, they suffer from it. I may perhaps be allowed to quote two sentences from a paper which I wrote twenty-four years ago, and therefore a considerable time before any antivivisection agitation had arisen, for they expressed then, and they express now, the objects of experimental pharmacology:—"Few things are more distressing to a physician than to stand beside a suffering patient who is anxiously looking to him for that relief from pain which he feels himself utterly unable to afford. His sympathy for the sufferer, and the regret he feels for the impotence of his art, engrave the picture indelibly on his mind, and serve as a constant and urgent stimulus in his search after the causes of the pain, and the means by which it may be alleviated" (*Lancet*, July 27, 1867).

Gains by Experiment on Animals.—It is said that our mouths are full of promises, but our hands are empty of results. The answer to this is, that anyone who doubts the utility of experimentation upon animals should compare the *Pharmacopœia* of 1867 with our present one. To it we owe, in great measure, our power to lower temperature, for to it is due not only the introduction of new antipyretics, such as salicylate of soda, antipyrin, antifebrin, and phenacetin, but the extension of the use of quinine from a particular kind of fever—malaria—to other febrile conditions. To it also we owe our greatly increased power to lessen pain by the substances just mentioned, which have not only an antipyretic but an analgesic action, and give relief in the torturing pains of neuralgia and locomotor ataxy when even morphine fails to ease, unless pushed to complete narcosis. The sleeplessness, too, which is such a frightful complication in some fevers, can now be combated by other remedies than opium and antimony; and we have the bromides, chloral, sulphonal, paraldehyde, urethane, chloralamide, and others, which, either by themselves or added to opium, enable us to quiet the brain instead of exciting it to further action, as opium alone so frequently does. Our whole ideas regarding cardiac tonics also have undergone a complete revolution within the last quarter of a century, for I was told, when a student, that digitalis was a cardiac sedative, and was apt to depress the heart, whereas now we know that it and its congeners—strophanthus and erythrophlœum and spartein—increase the heart's strength, raise the vascular tension, and are useful not only in sustaining the circulation, but in aiding elimination. This view of the action of cardiac tonics, which has revolutionized the treatment of heart disease, we owe chiefly to the experiments of Traube, although my own experiments, made in the laboratory of Sir Douglas Maclagan under the direction and by the help of my teacher and friend, Dr. Arthur Gamgee, may have helped towards its general acceptance in this country.

Future of Pharmacology.—But perhaps the most promising thing about pharmacology is that we are now just beginning to gain such a knowledge of the relationship between chemical structure and physiological action that we can, to a certain extent, predict the action of a drug from its chemical structure, and are able to produce new chemical compounds having a general action such as we desire; for example, anæsthetics, soporifics, antipyretics, analgesics, although we have not yet arrived at the point of giving to each one the precise action which would make it most suitable in any particular case. Even when we do not know the chemical structure of a drug, we may be able, from noticing one of its actions, to infer that it possesses others. We are, indeed, getting a knowledge of the action of drugs both of known and unknown chemical structure, and a power of making new remedies which will, I believe, enable us within the next five-and-twenty years to cure our patients in a way that at present we hardly think. . . .

THE INSTITUTION OF MECHANICAL ENGINEERS.

THE summer meeting of the Institution of Mechanical Engineers was held at Liverpool last week, commencing on Tuesday, the 28th ult., and concluding on Friday, the 31st ult. The President of the Institution, Mr. Joseph Tomlinson, presided throughout, and the meeting was highly successful, the long and varied programme being carried out with regularity and precision. The sittings for reading papers were held on the mornings of Tuesday and Wednesday; the afternoons of those days and also the Thursday and Friday being devoted to excursions. We will first deal with the papers and discussions.

The sittings were held in the concert-room of St. George's Hall, and the following list of papers was on the agenda:—A review of marine engineering during the past decade, by Alfred Blechynden, of Barrow-in-Furness; description of the warehouse and machinery for the storage and transit of grain at the Alexandra Dock, Liverpool, by William Shapton, of London; on the experimental engine and the alternative testing machine in the Walker Engineering Laboratories of University College, Liverpool, by Prof. H. S. Hele Shaw, of Liverpool; on the mechanical appliances employed in the construction of the Manchester Ship Canal, by E. Leader Williams, Engineer-in-Chief to the Canal Company. There was also a paper on the Liverpool water-works, but this was adjourned to the next meeting.

The Institution having been welcomed to Liverpool by the Mayor, Mr. J. B. Morgan, and the formal business having been transacted, Mr. Blechynden's paper was read. Mr. Blechynden has taken up the work commenced by Sir Frederick Bramwell at the Liverpool meeting of 1872, when the latter presented an historical paper giving a review of marine engineering up to that time. In 1881, the Institution met at Newcastle, when Mr. F. C. Marshall, a well-known Tyneside engineer, read a paper which consisted of a retrospect of the nine years since Sir Frederick Bramwell's paper had been read. We now have Mr. Blechynden carrying on the work. These periodical reviews are instructive. They cause the engineer to take stock of progress made, and enable him to see the lines upon which improvement may be expected to travel in the immediate future. Mr. Blechynden has been fortunate in the period which has fallen to his lot to review, for during the ten years past the triple compound engine has been developed. When Mr. Marshall read his paper, the ordinary compound engine with two cylinders was all but universal for steamships. Boiler pressures averaged 77·45 pounds per square inch, the average piston speed was 467 feet per minute, and the heating surface per indicated horse-power was 3·99 square feet. The consumption of coal per indicated horse-power was 1·828 pounds per hour. As a contrast to this, Mr. Blechynden tells us that at the present time the three-stage expansion engine has become the rule, and the boiler pressure has been increased to 160 pounds, and even as high as 200 pounds per square inch. Four-stage expansion engines of various forms have also been adopted. Forced draught has come to the front—largely, it would seem for the purpose of being abused—the piston speed has risen to 529 feet per minute, the heating surface per indicated horse-power is 3·274 square feet, and the coal consumption per indicated horse-power per hour is 1·522 pounds. By these figures it will be seen that during the last ten years the working pressure has about doubled, and that fuel economy has been improved by about 20 per cent. We may say that we do not always place full reliance in the details given with regard to fuel economy in connection with mercantile marine engines. We think that the power is apt to be taken on the best performance of the engines, so that they are credited with a duty they cannot maintain continuously throughout a voyage. Probably, however, the figures given by the author are accurate for comparative purposes, and they are not, as are some results claimed by marine engineers, altogether too good to be true. We would here draw attention to the author's expressions "three-stage" and "four-stage" compound engines. Engineers have been in the habit of referring to these types as triple expansion and quadruple expansion engines. This nomenclature is inaccurate and misleading for an ordinary two-cylinder compound, and even the simple non-compound engine expands the steam more than three or four times. Some engineers, recognizing this, have used the terms "triple compound" or "quadruple compound," but Mr. Blechynden's expression has the merit of greater accuracy and simplicity. We hope that engineers, who are apt to be somewhat loose in the naming of objects, will adopt Mr. Blechynden's terms. Added to the paper are tables giving details of construction and performance of representative steamers of the present day. A long discussion followed the reading of this paper. It turned chiefly upon the question of forced draught, corrugated flues, and the rules with regard to boiler testing which Mr. Sennett introduced when he was at the Admiralty. With regard to the forced draught question, the very sensible opinion seemed to have been arrived at that forced draught, though a good thing in itself, may prove a great ill if overdone. It is in the Navy chiefly that forced draught has gained an evil reputation, and naval officers are largely to blame for this, although the engineers must take their share of the responsibility. When it was found how great an accession of power could be obtained by forcing combustion with a fan, naval officers thought they had a royal road to speed. Boilers which had been designed on principles that had grown up under a simple chimney draught *régime*, were urged by fan-blast to duties beyond their powers of endurance; and then, when tube plates buckled and tubes leaked, forced draught was said by gallant admirals to be "the invention of the Evil One." The engineers, as we have said, were also to blame. The boiler has always been the Ishmael of the machinery-designer, nearly all the attention having been lavished on the engine. As a consequence boiler construction has been a matter of rule of thumb, and, when the empirical rules upon which it was based have no longer applied, the engineer has been nonplussed through want of a basis of scientific knowledge upon which to build anew. The torpedo-boat builders have no trouble with forced draught, though they blow far harder than in any other vessels; but then the torpedo-boat builders are good engineers—not mere blind followers of "practice"—as was proved by the paper read last spring on this subject by Mr. Yarrow before the Institution of Naval Architects. In speaking upon corrugated flues Mr. Macfarlane Gray made a remark on the subject which might have received more attention. It has long been claimed by the makers of this type of furnace that additional heating surface, and that of a most valuable kind, was obtained by the corrugations. This Mr. Gray said was a fallacy, for the heat from the furnace proceeded only in radial lines, and therefore no greater effective area of heating surface could be obtained than that due to a plain cylinder.

Mr. Shapton's paper was an interesting description of the building and machinery referred to in the title, by which grain is transported and stored. The warehouse in question consists chiefly of a vast cellular structure which might be described as a brick and mortar honeycomb, filled with grain in place of honey. There are 250 hexagonal bins or silos, each measuring 12 feet across the angles and 80 feet deep. The storage capacity is 2,240,000 bushels. The grain is lifted from vessels by elevators, and carried to the top of the building, from whence vertical movement is supplied by gravity. Horizontal travel is carried on by continuous moving belts or bands which run over wheel pulleys. The way in which streams of grain can be diverted into any required direction is very curious to watch. A good part of the discussion on the paper turned on the best form of bin or silo. At first one would think that the bin designer could not do better than follow the bee, but it was shown that cylindrical chambers made of sheet iron would give a large saving of space over the hexagonal brick bins. The advantage is due of course to the thinner walls of sheet iron, the cylinder being a form by which advantage can best be taken of the high tensile strength of iron. In America, where the silo system was in common use long before it made its appearance in this country, the bins are made wholly of wood, but this is subject to rot, and harbours weevils. Sheet-iron rusts and brick retains moisture, so that with brick the grain heats unless well looked after and ventilated. On the whole, however, brick has the preference in this country. Sir James Douglass made a suggestion which will, we should think, receive attention at the hands of future silo designers. The representation of the Eddystone Lighthouse at the Royal Naval Exhibition is a building not altogether dissimilar from a silo. It has very thin walls, which are constructed of expanded sheet steel, or sheared lattice work, which forms the bond for a crust of Portland cement. The result is a wall of great tenacity and rigidity, and one which would not have the same defect as brickwork with regard to harbouring damp. The problem of ventilating grain is one of difficulty; and it may be said that it has not yet been solved. The most serious effort yet made was the building of a granary on the banks of the Thames, known, we believe, as the Patent Ventilating Granary. This granary was referred to during the discussion by Mr. Percy Westmacott, so long the chief of the hydraulic department at Armstrong's. The patent ventilating arrangement consisted of a perforated tube running down the centre of each bin. This was provided with a movable stop or plug, and, by adjusting the height of the stop, a blast of air could be directed through the perforations of the tube into any part of the grain. The idea was of French origin, and, Mr. Westmacott said, more ingenious than practical, so that the granary was pulled down after a time. It is easy to understand that those parts of the grain which required most ventilation would form into hard lumps, into which the air would not penetrate. As a matter of fact it is found more advantageous to air the grain by giving it a constitutional over the carrying bands.

Prof. Hele Shaw's paper on his experimental engine and alternative centre-testing machine was one of great interest. The engine in question, which is described as a marine engine, though it has a large fly-wheel, is, we believe, the most elaborate from an experimental point of view, yet made. The question has been raised whether it is not too elaborate, so that satisfactory results will not be reached on any one point. That is a problem which remains to be proved by facts; for the engine has only just been erected. It is 150 horse-power, and is of the ordinary vertical three-cylinder three-stage compound type. The high-pressure and intermediate cylinders have cylindrical valves, and the low pressure has a flat valve. Each valve is worked by a different type of motion—namely, ordinary Stephenson link motion, Joy gear, and Hackworth gear. The cylinders are jacketed at sides and ends, and there are provisions in the way of connections for working in every possible manner, *i.e.* cylinders all jacketed, not jacketed at all, or any one or two jacketed. Any combination of cylinders can be worked, or any one cylinder alone. In addition to this the cranks are adjustable on their shaft, so that any combination can be got in this way; in short, the number of different combinations that are at command would require years to work through. There are the usual measuring tanks and other apparatus for quantitative tests. An excellent suggestion was made by Prof. Goodman during the discussion. He proposed that arrangements should be made for testing the students' knowledge by putting the engine into conditions not in accordance with proper design. For instance, he would have valve-rods or excentric rods of improper length, valves ill-set with improper lap or lead, leaky valves and pistons, and various other ills, to which engines are subject, purposely introduced. He would also provide a means of passing water into the cylinders. He would then have the student take diagrams from the engine, and leave him to determine the cause of the defect by the appearance of the cards. We hope Prof. Goodman will be able to follow up this useful suggestion in his own laboratory at Leeds. The alternative testing machine is a 100-ton single-lever machine of the Wicksteed type. The alteration in power is got by substituting one fulcrum for another a few inches distant. The mechanism by which this is done is ingenious, but the details would be difficult to explain without the aid of diagrams.

The last paper read at the meeting was that of Mr. Leader Williams. The author commenced by saying that 46½ million cubic yards had to be excavated in making the Manchester Ship Canal, and as only 17,000 men and 200 horses have been used there was evidently required a large power in the shape of mechanical appliances in order to get the work done in anything like reasonable time. Ninety-seven steam excavators and eight steam dredgers of large power have been employed; and the spoil has in most cases been taken a distance of several miles. For this work, and for the general purposes of construction, 173 locomotives and 6300 trucks and waggons have been used. The railways laid for the purpose amount to 228 miles of single line. The rate of excavation has varied from three-quarters of a million to 1¼ million cubic yards per month. There are also employed on the works 124 steam-cranes, 192 portable and other steam-engines, and 212 steam-pumps. The coal consumed by the engines is about 10,000 tons a month. These figures will give some idea of the heroic proportions upon which large constructive works are carried out, and the capital required to start them. The whole plant of the Manchester Ship Canal has cost, we believe, close on a million sterling. The machines described in the paper which were of greatest interest were the excavators. The chief of these is the now well-known "steam navvy," made by Ruston and Proctor, of Lincoln. It first came prominently into notice during the construction of the Albert Docks, and is looked on as a standard tool wherever large excavating work is undertaken. It has the immense advantage of being able to work in any kind of soil, even including sandstone rock, if not very hard. It is only in hard rock that blasting has to be done as an auxiliary. The most interesting, or, perhaps, we should say the most novel machines are the French and German excavators, or land dredgers, which have been introduced into this country for the first time in connection with this work. These are on the same general principle as a floating ladder and bucket dredger of the common type. In place of the ladder and motive machinery being held by a floating hull, there is a small house mounted on wheels, and this runs on a line of rails on the summit of a bank. The ladder slopes outward from the side, reclining on the bank, which the buckets scrape away as they traverse, and deposit the spoil in waggons on the bank above. There are differences in detail between the French and German types, but in general principle they are alike. The German machine appears to us the better designed, but Mr. Leader Williams says the French excavator is of more substantial construction. The weight of these machines is from 70 to 80 tons, and under favourable conditions they have been known to excavate the enormous bulk of 2400 cubic yards in one working day. Mr. Williams's paper was not discussed, which is a fact to be regretted by engineers, as the subject is one which requires ventilation; but time was running short. After the usual votes of thanks, the sittings of the meeting were brought to a close.

We can only add a few words about the excursions. On the Tuesday there was a lunch on board the big White Star liner the *Majestic*, for one section of the members; whilst others visited the grain warehouse, described by Mr. Shapton in his paper, and the new overhead railway, which has been designed by Mr. Greathead, the Engineer of the City and South London Railway, and which runs along the line of docks. This railway is of steel and iron throughout, and possesses the novel advantage of forming a water-tight roof, under which the people of Liverpool will be able to walk on rainy days without getting wet. In the evening there was a *conversazione*, which, of course, was the social feature of the meeting. On Wednesday afternoon the members visited the new engineering laboratories which have been added to University College, Liverpool, where the engine and testing machine described in Prof. Hele-Shaw's paper were examined. On Thursday one party visited the Mersey Docks, the Mersey Tunnel, and Laird Bros.' ship-yard and engine works. At the latter there are several interesting vessels in progress, including the big battle ship *Royal Oak*, of 14,000 tons. Another party went to Horwich, and saw the fine locomotive works which have just been completed there by the Lancashire and Yorkshire Railway. These works have been beautifully planned and laid out under the superintendence of Mr. Aspinall. Although not so large as some other establishments of a similar kind, they may be taken as a model of design. Mr. Aspinall naturally had a unique opportunity with a clear field to work upon, and an accumulated experience at his command. Friday, the last day, was devoted wholly to the Manchester Ship Canal, the members being carried down the line of works in a special train, under the guidance of Mr. Leader Williams.

THE NEW GAS, CHLOROFLUORIDE OF PHOSPHORUS.

AS briefly announced in the report of the proceedings of the French Academy of Sciences, a note upon a new gaseous compound, containing phosphorus, fluorine, and chlorine, has just been presented by M. Moissan, on behalf of M. Poulenc. During the course of his work upon the fluorides of phosphorus, M. Moissan observed that, when phosphorus trifluoride was brought in contact with chlorine, the green colour of the latter at once disappeared, and there appeared to be formed a new and colourless gas. The gas thus formed has been prepared in considerable quantity by M. Poulenc, and its properties investigated. It appears to be directly formed by addition, according to the simple equation—

$$PF_3 + Cl_2 = PF_3Cl_2;$$

for the trifluoride of phosphorus and chlorine are found to react in equal volumes, and the combination is attended by a contraction of one-half. The new gas may therefore be considered as phosphorus chlorofluoride, PCl_2F_3, the chlorine derivative of phosphoryl and thiophosphoryl fluoride, POF_3 and PSF_3.

The most convenient mode of preparation is described as follows. Two flasks of equal capacity (about 500 c.c.) are taken, and filled respectively with phosphorus trifluoride and chlorine. They are connected together by a bent tube passing through the stoppers, and the flask containing the phosphorus trifluoride is further connected with a reservoir of mercury in such a manner that a gentle pressure may be placed upon the trifluoride, so as to gradually displace it over into the chlorine. The two flasks being of equal capacity, it is evident that, when the whole of the trifluoride has thus been transferred, the reaction is completed, the green colour of the contents of the other flask disappears, and the remaining gas is almost pure chlorofluoride. After allowing to stand a few days in contact with the mercury, in order to remove the last traces of chlorine, the gas is ready for examination.

Phosphorus chlorofluoride is a colourless incombustible gas, possessing a powerfully irritating odour. It is instantly absorbed and decomposed by water and by solutions of alkaline or alkaline earthy hydrates. A determination of its vapour-density gave the number 5·40, sufficiently near the theoretical density of a substance PCl_2F_3 (5·46). It is comparatively easily liquefied, a temperature of $-8°$ C. being sufficient at ordinary pressures. It is dissociated at a temperature of 250° C. into gaseous pentafluoride and solid pentachloride of phosphorus. The induction spark effects the same decomposition.

Sulphur reacts with phosphorus chlorofluoride in a most interesting manner. The reaction commences about the melting-point of sulphur, 115° C., and the products are chloride of sulphur and gaseous thiophosphoryl fluoride, PSF_3. And here a most emphatic protest must be made against the manner in which many French chemists persistently ignore the work of the chemists of other countries. Thiophosphoryl fluoride, PSF_3, was discovered and prepared three years ago in the Research Laboratory of the Royal College of Science, South Kensington, by Prof. Thorpe and Mr. J. W. Rodger; and a detailed account, illustrated by experiments, of the mode of preparation and properties of this remarkable gas, was laid before the Chemical Society and published in their Journal.[1] And yet, in the memoir just presented by M. Moissan, we find this compound, a description of which long ago found its way into the abstracts or referate of most foreign journals, described as "un nouveau composé gaseux." Indeed, a considerable amount of unnecessary trouble appears to have been taken in order to ascertain the composition of this "new gas"—trouble which, as the compound is so readily recognizable by its extraordinary properties, might have been saved, if the author had taken the pains to look up the literature of the subject. It is high time that French chemists should look to their "prestige" in this respect, for, unfortunately, the present is by no means the only case which has within the last few months come before the notice of the writer of this note, in which compounds fully described and worked out by English chemists have been rediscovered and described as new by French authors.

When phosphorus chlorofluoride is passed over free phosphorus heated to 120°, it is decomposed with formation of phosphorus trifluoride, which passes away as gas, and phosphorus trichloride, which condenses in liquid drops. Metallic sodium, when slightly heated, appears to absorb the chlorofluoride entirely, while magnesium, aluminium, iron, nickel, lead, and tin, when heated to about 180°, attack the gas with formation of anhydrous chlorides and liberation of phosphorus trifluoride. Mercury attacks it very slowly at the ordinary temperature, but very rapidly at 180°, with formation likewise of a chloride of the metal and gaseous trifluoride of phosphorus. Hence, when purifying the gas from the last traces of chlorine, the mercury should not be agitated, but allowed to remain at rest, as agitation brings about a perceptible amount of decomposition.

Water reacts in two stages with phosphorus chlorofluoride. When a little aqueous vapour is admitted into the vessel inclosing the gas, phosphoryl fluoride and hydrochloric acid are formed in accordance with the equation—

$$PCl_2F_3 + H_2O = POF_3 + 2HCl.$$

When passed into water, however, the gas is completely decomposed into phosphoric, hydrochloric, and hydrofluoric acids—

$$PCl_2F_3 + 4H_2O = H_3PO_4 + 2HCl + 3HF.$$

Ammonia gas reacts at the ordinary temperature with production of a white solid compound, readily soluble in water, which appears to be fluophosphamide, $PF_3(NH_2)_2$.

$$PCl_2F_3 + 4NH_3 = PF_3(NH_2)_2 + 2NH_4Cl.$$

Phosphorus chlorofluoride is absorbed by absolute alcohol with production of a compound possessing a penetrating odour, and which burns with a bright flame bordered with green, and leaves a white residue of phosphoric acid. The nature of this compound has not yet been fully ascertained.

These properties of phosphorus chlorofluoride indicate that the gas is much less stable than the pentafluoride, and that the two atoms of chlorine possess a mobility which renders their removal a matter of considerable ease.

A. E. TUTTON.

PROF. MENDELEEFF ON THE VARIATION OF THE DENSITY OF WATER AT DIFFERENT TEMPERATURES.

THE last number of the Journal of the Russian Physical and Chemical Society (1891, No. 5) contains an important paper, by Prof. Mendeleeff, upon the variation of the density of water at different temperatures. In a work, published in 1884 and translated into English in the Journal of the Chemical Society, the Russian Professor proposed the formula $S_t = S_o(1 - kt)$ as a first approximation to a mode of expressing the expansion of liquids at a certain distance from the temperatures at which they change their state, and within the limits of accuracy attained in the present determinations. But he remarked that the expansion of water would require a separate formula, and he now proposes the formula

$$S_t = 1 - \frac{(t-4)^2}{(A+t)(B-t)C},$$

which embodies, with sufficient accuracy, all that is yet known about changes in the density of water (S_t) within the limits of from $-10°$ to $+200°$. For all liquids save water, the increase of density with the increase of temperature, that is, the derived $\frac{ds}{dt}$, varies but little; it but slightly increases or slightly decreases with considerable changes of temperature; while for water, $\frac{ds}{dt}$ not only changes its sign at $+4°$, but very rapidly varies even at temperatures remote from zero, and even superior to 100°. After confirming the above by a few examples, Prof. Mendeleeff indicates the faint relations between his new formula for water and the general law of the expansion of liquids, by explaining the way in which he arrived at his new formula. He points out, moreover, that under the present state of the determinations of the density of water at various temperatures, it would be impossible to find exact figures for the constants A, B, and C, in the above formula, and that provisionally, and especially for temperatures between 0° and 40°, they may be taken as follows:—$A = 94·10$, $B = 703·51$, and $C = 1·90$.

Prof. Mendeleeff then goes on briefly to analyze the various corrections which ought to be taken into account in the determinations of the density of water; namely, the influence of pressure, the expansion of solids, and the measurements of temperature. All these being taken into account, it appears that the errors of the best determinations of densities attain several units in the fifth decimals, even at common temperatures. After many unsuccessful attempts at improving the current figures of densities by introducing into them several corrections, Prof. Mendeleeff abandoned the idea, and he now gives the authentic figures, as they were published by the investigators themselves, simply expressing all determinations in volumes for the sake of facilitating comparison. The figures published by Hallström (1823), Muncke (1828), Stampfer (1831), Despretz (1837), Pierre (1847), Kopp (1847), Plücker and Geissler (1852), Hagen (1855), Henrici (1864), Jolly (1864), Matthissen (1865), Weidner (1866), and Rosetti (1869), are thus given in a first table. The figures, as they were corrected by Biot in 1811, Hallström in 1835, Miller in 1856, Rosetti in 1871, Volkmann in 1881, Mendeleeff in 1884, and Makaroff in 1891, are given in a second table.

The averages of the volumes of water derived from the original figures (Table I.), at temperatures from -5° to $+100^\circ$, taking the volume at 4° equal to 1,000,000, and the pressure being equal to one atmosphere, appear as follows in the second column (V_t) of the subjoined table. They are followed, in the third column, by the volumes as calculated from Prof. Mendeleeff's new formula :—

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[1] Journ. Chem. Soc. Trans, 1889, vol. lv. p. 305.

t.	V_t.	$V_t = \frac{1}{S_t}$ calculated from the formula.	$\frac{dV}{dt}$ for 1°.	$\frac{dV}{dp}$ for 1 atmosphere.	Possible errors of the present determinations.
° −5	1 000 662	1 000 676	−157	−52	∓ 29
0	1 000 122	1 000 127	− 65	−50	∓ 12
+5	1 000 008	1 000 008	+ 15	−48	± 3
10	1 000 263	1 000 262	+ 85	−47	± 15
15	1 000 847	1 000 849	+148	−46	± 26
20	1 001 733	1 001 731	+204	−45	± 35
25	1 002 871	1 002 880	+254	−44	± 43
30	1 004 248	1 004 276	+302	−43	± 49
40	1 007 700	1 007 725	+386	−41	± 59
50	1 011 933	1 011 967	+461	−40	± 67
60	1 016 915	1 016 926	+530	−39	± 75
70	1 022 513	1 022 549	+595	−40	± 85
80	1 028 849	1 028 811	+656	−41	± 98
90	1 035 719	1 035 692	+719	−42	±118
100	1 043 180	1 043 194	+781	−44	±145

Finally, a third table is given, being the result of the calculation made by taking

$$S = 1 - \frac{(t-4)^2}{1000\phi(t)},$$

$$\phi(t) = 128{\cdot}78 + 1{\cdot}158t - 0{\cdot}0019t^2,$$

and

$$1000\phi(t) = 1{\cdot}90\,(94{\cdot}10 + t)\,(703{\cdot}51 - t),$$

and extending the calculation to $+200^\circ$ and -10°. The most important values of $\frac{ds}{dt}$ are given in the fourth column of the subjoined table ; so, also, the approximate values of $\frac{ds}{dp}$, which are "but a first rough approximation," to show the importance of pressure in the determinations of volumes of water :—

t° C.	Calculated densities, S_t.	Possible error of present measurements (in 1,000,000th parts).	Derived ds/dt for 1° C. (in 1,000,000th parts).	Derived ds/dp for 1 atmosphere (in 1,000,000th parts).	Numerical values of $\varphi(t)$.	Calculated V_t.
−10	0·998 281	∓ 49	+ 264	+54	114·01	1·001 722
5	999 325	∓ 29	+ 157	52	119·94	000 676
0	999 873	∓ 12	+ 65	50	125·78	000 127
+ 5	999 992	± 3	− 15	48	131·52	000 008
10	999 738	± 15	− 85	47	137·17	000 262
15	999 152	± 26	− 148	46	142·72	000 849
20	998 272	± 35	− 203	45	148·18	001 731
25	997 128	± 43	− 254	44	153·54	002 880
30	995 743	± 49	− 299	43	158·81	004 276
40	992 334	± 53	− 380	+41	169·06	1·007 725
50	988 174	65	− 450	40	178·93	011 967
60	983 356	72	− 512	39	188·42	016 926
70	977 948	80	− 569	39	197·53	022 549
80	971 996	92	− 621	40	206·26	028 811
90	965 537	109	− 670	41	214·61	035 692
100	958 595	133	− 718	42	222·58	043 194
120	943 314	±600	− 810	+43	237·38	1·060 093
140	926 211	650	− 901	48	250·66	079 667
160	907 263	700	− 995	55	262·42	102 216
180	886 393	750	−1093	64	272·66	128 167
200	863 473	800	−1200	73	281·38	158 114

In conclusion, Prof. Mendeleeff repeats that he proposes as soon as possible to make anew the determinations of the densities of water, because the former determinations were made on assumptions (permanency of the coefficient of the expansion of glass and mercury, and no notice being taken of pressure) which can no longer be maintained. If new measurements confirm the formula, or lead to a more correct one, we shall be better able to understand the laws of the expansion of all liquids, and therefore of gases as well. "In the case of water," he says, "we have begun to understand more clearly the influence of heat upon densities and volumes, and I believe that with the help of water we may expect some further progress in the study of the influence of heating upon matter."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The examiners in the Final Classical Schools issued the Class List on Wednesday week, completing the results of the examinations held in Trinity Term.

The summer meeting of Extension students commenced on Friday last, when Mr. Frederic Harrison, M.A. Wadham College, delivered the inaugural lecture. The popularity of the movement is proved not only by the continual increase in the number of students who avail themselves of the advantages offered by this system of education, but by the interest which foreign Governments are taking in the development of the plan. The French Government have sent two special commissioners to report on the prospects and condition of the University Extension movement, and a large number of the representatives of the American University Extensionists are now in Oxford.

The number of students attending the various lectures is greater than on any previous occasion, more than 1100 having subscribed, while last year the number did not greatly exceed 900. A more rapid growth and a still greater measure of success attending the work may be anticipated from the fact that various County Councils, finding themselves in possession of funds arising from the operation of the Local Taxation Act, and which they propose to devote to the purposes of technical instruction, are availing themselves of the machinery of the University Extension system to accomplish this desirable end.

SCIENTIFIC SERIALS.

IN the *Botanical Gazette* for June, Mr. T. Holm coutributes a study of some anatomical characters of North American grasses. In a paper entitled "On the Relation between Insects and the Forms and Characters of Flowers," Mr. T. Meehan epitomizes his views on fertilization opposed to the current theory, viz. that the part played by insects in the fertilization of flowers has been greatly exaggerated ; that flowers do not abhor cross-pollen ; and that all annuals can self-fertilize when cross-fertilization fails, annuals in almost all cases having every flower fertile.

THE most important paper in the *Journal of Botany* for July is the commencement of a detailed account of the Algæ of the Clyde sea-area, by Mr. George Murray, Secretary to the Committee for the Exploration of the Marine Flora of the West of Scotland. This is prefaced by an account of the physical features of the Clyde sea-area, by Dr. John Murray. Following this is the commencement of a hand-list of the Algæ, by Mr. E. A. L. Batters. The Rev. H. G. Jameson concludes his key to the genera and species of British mosses, which it is hoped may be published in a separate form ; and Mr. George Murray sinks Hooker's genus of sea-weeds *Cladothele* in *Stictyosiphon*.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, July 27.—M. Duchartre in the chair.—Proofs that Asia and America have been connected in recent times, by M. Emile Blanchard. In this paper the author points out certain species of Asiatic fauna and flora which are found in North America, as, in the preceding one, he indicated the representatives of European fauna and flora which occur in the same continent. Without making an extensive enumeration of the different species, the facts brought forward give considerable support to the idea that Europe, Asia, and America have been connected by land in comparatively recent times.—The Ichthyosaurus from St. Columbe, by M. Albert Gaudry. This is a description of an Ichthyosaurus exhibited at the Paris Exhibition of 1889. It is proposed to name the fossil *Ichthyosaurus burgundiæ*.—Examination of samples of native iron of terrestrial origin discovered in gold washings from the environs of Berezowsk, by MM. Daubrée and Stanislas Meunier. The specimens examined weighed respectively 11·5 grams and 72 grams, and were discovered near the Berezowsk gold mines, Persia. The metal is very magnetic, but manifests no polarity. Its density is 7·59. When treated with an acid it is sensibly attacked, but does not show the Widmanstätten figures as is the case when acid is applied to a clean face of meteoritic iron. This fact and the absence of nickel leads the authors to conclude that the iron is truly native. About one per cent. of platinum is present.—On the volatility of nickel under the influence of hydrochloric acid, by M. P. Schützenberger. When dry hydrogen is passed over pure anhydrous nickel chloride at a red heat, it may be shown that the hydrochloric acid gas which comes off from the tube in which the reduction occurs contains a sensible amount of metal in the form of a volatile product. The same result is obtained if, instead of reducing nickel chloride by hydrogen, finely divided nickel is acted on by dry hydrochloric acid gas. M. Schützenberger has not yet been able to isolate this body for the purpose of determining its constitution.—Note on a proposed Observatory on Mont Blanc, by M. J. Janssen.—On the retardation of luminous impressions, by M. Mascart.—Works of applied zoology effected at the Endoume maritime station during 1890, by M. A. F. Marion.—On a geometrical representation and formula expressing the law of the passage of perfect gases through orifices, by M. Henri Parenty.—On the densities of oxygen, hydrogen, and nitrogen, by M. A. Leduc. The values obtained are: hydrogen 0·0695, oxygen 0·1050, nitrogen 4·9720. From the densities of oxygen and nitrogen the percentage proportion of the former element in air is found to be 23·235 by weight and 21·026 by volume. The atomic weight of nitrogen deduced from these results is 13·99, and that of oxygen 15·905.—Remarks on the transport of metallic iron and nickel by carbon monoxide, by M. Jules Garnier. Some observations of the character of the flames issuing from furnaces in which these metals are being reduced are shown to be easily explained in the light of recent work on iron and nickel carbonyls.—Action of water on the basic salts of copper, by MM. G. Rousseau and G. Tite. Certain borates and oxychloride of antimony are transformed to oxides by the prolonged action of water at a sufficiently high temperature. Similarly, by heating copper nitrate, brochantite, and atacamite with water in sealed tubes they have been reduced to oxides. Libethenite has been experimented upon, but has resisted the transformation even when kept in the presence of water for three days at a temperature of 273° C.—On an actual mode of formation of mineral sulphides, by M. E. Chuard.—Researches on thallium, by MM. C. Lepierre and M. Lachaud. Thallium chromate has been prepared by acting on thallium sulphate with potassium chromate. Reactions with various bodies are described.—On parabanic and oxaluric acids, by M. W. C. Matignon. The heat of combustion of parabanic acid is found to be 212·7 cal., of oxaluric acid 211 cal. Hence the heats of formation have been calculated, 139·2 cal. and 209·9 cal. The heat of solution of parabanic acid at 20° and with a concentration of $\frac{1}{20}$ mol. per litre is − 5·1 cal. The formation from oxalic acid of its ureide, parabanic acid, gives + 2·2 cal. and of its uramic acid, oxaluric acid, + 2·5 cal. The formation of the ureides thus gives only a feeble heat-liberation. Each of these acids dissolved in a large excess of potash yields the neutral potassium oxalate. Potassium oxalurate has been prepared by dissolving the acid in its equivalent of potash and evaporating. Fine prismatic needles are obtained, differing from the salts of Menschutkin and Strecker. The heat of neutralization of oxaluric acid is 30·2 cal., as against 34·2 cal. for oxalic acid.—The transformation of gallic acid and tannin into benzoic acid, by M. Ch.-Er. Guignet.—On the polymeric acids of ricinoleic acid, by M. Scheurer-Kestner.—On the fermentation of bread, by M. Léon Boutroux. During an examination of the conditions essential for the fermentation of bread, the author has isolated five species of yeast and three species of bacteria. The parts played by each of these organisms are described, and the conclusion is finally drawn that the fermentation of bread consists essentially of a normal alcoholic fermentation of sugar pre-existing in the flour, and that only the yeasts producing alcoholic fermentations are necessary; the ordinarily co-existing alteration of gluten is a subsidiary and unessential action due to some of the bacteria present.—On a thermogenic substance in urine, by M. Paul Binet.—On the transformation of carboxy-hæmoglobin into methæmoglobin, and a new process of examination for carbon monoxide in the blood, by MM. H. Bertin-Sans and J. Moitessier.—On a new apparatus for measuring muscular power, by M. N. Gréhant.—Measure of the muscular power of animals under the action of certain poisons, by MM. Gréhant and C. Quinquaud.—On the concordance of Prof. S. P. Langley's experimental results on the resistance of the air (see NATURE of July 23, p. 277) with the values obtained by calculation, by M. Drzewiecki.—Analysis by means of chrono-photography of the movements of the lips during speech, by M. G. Demeny. Using M. Marey's method for photographing objects in rapid motion, the author has succeeded in portraying the movements of the lips during speech, and finds that it is possible to distinguish the letters of the alphabet when the photographic results are spun in a zootrope.—Relation between oscillations of the retina and certain entoptic phenomena, by M. A. Charpentier.—The nanny-goat is not refractory to tuberculosis, by M. G. Colin.—Researches on the pathogenic microbes in muds from the Dead Sea, by M. L. Lortet.—On the excretory apparatus of Carididæ, and on the renal secretion of Crustaceæ, by M. P. Marchal.—On the nervous system of Monocotylidæ, by M. G. Saint-Remy.—Contribution to the natural history of a cochineal, *Rhizœcus falcifer*, Künck, discovered in the greenhouses of the Museum and living on the roots of the vine in Algeria, by MM. Kunckel d'Herculais and Frédéric Saliba.—On specific assimilation in Umbelliferæ, by M. Génean de Lamarlière.—Document relative to the trajectory of the Ensishein meteorite of 1492, by Prof. H. A. Newton.—On the erosion and transport by torrential rivers having glacier affluents, by MM. L. Duparc and B. Baeff.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Cosmical Evolution: E. McLennan (Chicago, Donohue).—The Artillery of the Future and the New Powders: J. A. Longridge (Spon).—British Rainfall, 1890: G. J. Symons and H. S. Wallis (Stanford).—Epidemic Influenza, Notes on its Origin and Method of Spread: Dr. R. Sisley (Longmans).—Essays upon Heredity and Kindred Biological Problems; authorized translation, vol. i., 2nd edition: Dr. A. Weismann, edited by E. B. Poulton, S. Schönland, and A. E. Shipley (Oxford, Clarendon Press).

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THURSDAY, AUGUST 13, 1891.

THE INTERNATIONAL CONGRESS OF HYGIENE AND DEMOGRAPHY.

THIS Congress, the work of which we refer to in another column, which is now in full swing, promises to be one of the most important meetings of the kind that has ever been held, not only in point of numbers, but also on account of the far-reaching results likely to accrue from it.

A remarkable combination of circumstances has contributed to its success. In the first place, it is held in the country which has been the pioneer of sanitary work; and then it has the patronage of Her Majesty the Queen, who, it is well known, takes a deep personal interest in its success; and has as its President, not merely in an honorary sense, His Royal Highness the Prince of Wales, who presided and gave an admirable address of welcome at the splendid opening meeting on Monday in St. James's Hall.

This is the seventh of a series of similar Congresses which have been held in various parts of Europe, and one is tempted to ask what they have accomplished. An answer is at once forthcoming. The all-important question of quarantine has been discussed at several of these Congresses. Not to go farther back than the Congress at The Hague, held in 1884, we find, from the excellent reports issued by the editors of the *Lancet*, that then the feeling in Europe was so strongly opposed to the English views as to the inutility of quarantine and the superiority of our method of medical inspection, that the English delegate was not even allowed to explain the English position in the matter, but the discussion was peremptorily closed, on the ground that the subject had been sufficiently discussed on the previous day. At the Vienna Congress, in 1887, quarantine was again discussed under the subject of cholera; and the veteran Pettenkoffer told the members of various countries present that they had only to follow the example of England, in looking after their systems of water-supply and sewerage, and in isolating cases of infectious disease, and they would be no more afraid of cholera than the English were, even with their continual communication with India, the home of that disease, and would have no need of quarantine, with all its vexatious and ineffective restrictions, and all its unnecessary interference with commerce. Now, Continental opinion is almost entirely on our side, and it is doubtful whether there will be any serious discussion on the matter.

But there are many other subjects with which the Congress will interest itself, and about which such an interchange of views as can only be obtained at an International Congress must be of the greatest benefit. The whole subject of bacteriology has grown up within the last few years, and one of the most important and best attended Sections of the Congress is devoted to it, many of the highest authorities on this subject having been attracted here to take part in the discussion under the presidency of Sir Joseph Lister. The abnormal prevalence of diphtheria, not only in our own large towns, but also in those of other parts of Europe and in America, in many cities of which, especially in the Western States of North America, it has become a veritable plague, is likely to occasion an important discussion in Section I., under the presidency of Sir Joseph Fayrer. The mention of his name leads us to observe that India is well to the front in this Congress, for not only have a number of delegates been sent by her Provinces and Native States, but they have also largely contributed to the funds of the Congress.

Influenza, too, our new plague, about which we seem to know so little, might be discussed, as to its mode of spread and methods of prevention, with great advantage at a meeting where so much experience from all parts of the world is focussed.

An especial feature in this Congress is, as might be expected in England, the prominence which is given to engineering and architecture in connection with hygiene, there being two separate Sections devoted to these branches of the subject.

The division of demography, too, which has been so much talked about on account of its name, which was up to the present time unfamiliar to English ears, and which has been defined by some wag as "the art of drawing the public," has attracted, under the presidency of Mr. Francis Galton, many of the most eminent statisticians of Europe, whose discussions cannot fail to promote the attainment of more uniformity in the methods of statistical inquiries.

This is an age of Congresses, and if they are, as it is universally agreed that they are, of any use at all, it is self-evident that the most useful and the most important are the international ones.

A LIFE OF DARWIN.

Charles Darwin: His Life and Work. By Charles Frederick Holder. (New York and London: G. P. Putnam's Sons, 1891.)

BETWEEN the voluminous "Life and Letters" of his father, by Prof. Francis Darwin, and the brief epitome of Darwin's work, by Mr. G. T. Bettany, published in 1887 in the "Great Writers" series, there has hitherto been a gap which has only been partially filled by such books as Grant Allen's "Charles Darwin" in the series of "English Worthies." In the first of the works mentioned, our great naturalist is chiefly allowed to speak for himself, while in the second we have a digest of his scientific achievements. Although it has been generally considered that the life of Darwin from the time of the return of the *Beagle* was too uneventful to make an interesting biography, we have always been of opinion that there existed sufficient material for a popular "Life" of the very greatest interest provided that this material could be skilfully and judiciously worked up. The work under notice supplies this want, and American and English readers are now provided with a biography which is both entertaining and accurate.

Of course the material out of which Mr. Holder has woven his story is for the most part to be found in Darwin's own writings, or in the "Life and Letters," and readers who turn to the pages of this book with the hope of finding new matter may be disappointed. But the very circumstance that out of the familiar records of the voyage of the *Beagle*, and the later writings of Darwin, the author has been enabled to construct such a very readable volume, is the best tribute to his skill.

The task which Mr. Holder took up was by no means an easy one; the difficulty which he had to confront did not arise from paucity of material, but from a superabundance of records, owing to the very complete account of his own travels and observations which Darwin has bequeathed to us. To extract the salient points from these records, and to dress them up in the writer's own language, was a labour requiring considerable literary ability. Mr. Holder has shown that he was well qualified for the undertaking, and it is refreshing—after the "Summary of the Darwinian Theory," and similar productions to which we have recently been treated in this country—to find that an American naturalist is able to write an account of Darwin and his work in language expressing his own ideas on the subject, instead of stringing together a lot of disconnected quotations from Darwin's writings. Not the least praiseworthy feature of the book is the comparatively small number of extracts from the writings of his hero; the author is wise enough to recognize the fact that most reading naturalists may be supposed to be familiar with the text of the "Naturalist's Voyage," the "Origin of Species," and other Darwinian classics.

The present volume is one of the "Leaders in Science" series, published by the firm of Putnam's Sons. The author says in the preface:—

"When the publishers proposed to me the subject of the present volume, a life of Charles Darwin for American and English readers, I was particularly gratified with the suggestion that the work should be adapted to young readers as well as old. It has always seemed to me that the life of Charles Darwin was one eminently fitted to be held up as an example to the youth of all lands. He stood as the central figure in the field of natural science in this century, and while it is yet too early to present his life with any approximation of its results upon the thought of the future, it is apparent to everyone that his influence upon the intellectual growth of the country, and upon biological science in particular, has been marked and epoch-making.

"In the preparation of the work I have not attempted an analytical dissertation upon Darwin's life-work, neither have I discussed his theories or their possible effect upon the scientific world, but have simply presented the story of his life, that of one of the greatest naturalists of the age; a life of singular purity; the life of a man who, in loftiness of purpose and the accomplishment of grand results, was the centre of observation in his time; revered and honoured, yet maligned and attacked as few have been."

Having thus defined his object, the author proceeds to narrate his story, beginning with the boy Darwin, passing on to his Cambridge career, and then leading us through the scenes of his wanderings as naturalist to the *Beagle*. The major portion of the volume (twelve out of the twenty chapters) is thus pleasantly filled up; all the little personal incidents which give colour to the individuality of the man are skilfully brought in, and here and there the author interposes observations of his own which help to throw light on the questions discussed and the facts recorded by Darwin. Having in view the taste of his younger readers, a number of full-page illustrations have been introduced, some being reproduced from Spry's "Voyage of the *Challenger*," others from Gosse's "Romance of Natural History," others from Brehm's "Natural History," from Figuier's works, and from the *Century Magazine*. Many of the illustrations are new, the frontispiece, representing Darwin in his garden with the squirrels running up him, being well worthy of notice.

The working period of Darwin's life from the return of the *Beagle* to his death is dealt with in three chapters, in the course of which the author relates the history of the "Origin of Species," and the impetus given to the publication of that work by the independent discovery of the principle of natural selection by "Alfred Russel Wallace, a young Welsh naturalist, who was then travelling in the Malay country." This incident is of course familiar to all, but as an old story retold by a transatlantic admirer of Darwin it reads even now with the charm of freshness. The later works are referred to in chronological order, and in a succeeding chapter we have a catalogue of the honours conferred upon Darwin during his life. The seventeenth chapter contains an account of the Darwin family, beginning with William Darwin, of Marton, near Gainsborough, in 1500, and concluding with Erasmus, elder brother of Charles Darwin, the friend of Carlyle, who was described by the latter in his "Reminiscences," and whose amiable character was more fully portrayed by Miss Julia Wedgwood in the *Spectator* in 1881. The latter description from the pen of Miss Wedgwood is given by Mr. Holder *in extenso*.

The narrative, as such, ends with the death of Darwin in 1882, and the reader will turn with renewed interest to the eighteenth and nineteenth chapters, containing Mr. Holder's account of the Darwinian theory. The principles of this theory are fairly well expounded, considering the small amount of space which has been devoted to them. Natural selection is illustrated by a happily chosen and original example from the animal kingdom, viz. the adaptive coloration of the fauna of the Sargasso Sea. Another illustration of the principle is drawn from the vegetable world, viz. the evolution of a hairy seed adapted for aërial transport. The questions of geological time and the palæontological evidences of organic evolution are also touched upon, and here we think the author might have used more judgment. The formation of the chalk, for example, is not quite satisfactorily given, and the statement that the chalk cliffs of Dover have been elevated "by some convulsion of nature" (p. 185) will jar upon the geological susceptibilities of his readers. In a work intended for popular reading it would also have been safer to avoid any estimate of the time required for the denudation of the Weald, the more especially as Darwin himself admitted the unsoundness of such estimates by omitting this section in the later editions of the "Origin." The ancestry of the horse, and Prof. Marsh's discovery of the *Odontornithes*, are well brought in in connection with the palæontological evidence. We may point out in passing that the diagram illustrating the evolution of the horse, which fronts p. 62, is referred to both on pp. 189 and 190 as "the accompanying diagram," which is obviously an oversight.

In tracing the history of pre-Darwinian evolution, the author mentions the views of Bonnet, the doctrines of

Thales and Anaxagoras, the speculations of Leibnitz, De Maillet, Wright, Lambert, Herschel, and La Place. Of Buffon he says :—

"Buffon was the naturalist of the day in the time of Louis XV. and Louis XVI.,—a period somewhat famous for the restrictions which were placed upon men, and the denunciations with which new and advanced ideas were received. Thus advanced thinkers found that their theories in many instances, instead of leading them on to fame, but opened the doors of the Bastile.

"It is not improbable that Buffon was in accord with the feeling of the time, as while his great discursive work—'Histoire Naturelle,' of 1749–88—fully outlines the theory of evolution, in which he was a believer, it is done in an ironical, partly satirical manner, so that he could, if attacked, retreat by claiming that it was a satire on the advanced scientific thought of the time ; . . . he was ready to believe that from a single unit in the beginning might have descended all the various forms of existing animal and plant life. It is curious to note that this pioneer evolutionist suddenly corrects himself and says : 'But no ; it is certain from revelation that every species was directly created by a separate fiat.' We may suspect that this secession from a position so broadly taken was forced upon the evolutionist. Perhaps the clergy gave him close and suggestive attention, and he was offered the choice between the Bastile, the Sorbonne, and apology to offended orthodoxy. Be this as it may, Buffon was one of the early delineators of the modern theory of evolution, and despite his peculiar attitude, history accords him this recognition."

The works of Wolff, of Goethe, Geoffroy St. Hilaire Oken, Pander, Von Baer, Schleiden and Schwann, Von Mohl and Max Schultze, Lord Monboddo and Erasmus Darwin, are all referred to in due order ; and a well-bestowed paragraph of praise is given to Lamarck. Later writers, such as Robert Chambers, Von Humboldt, Owen, Asa Gray, Herbert Spencer, and Youmans, bring us down to the birth of modern Darwinism.

To English readers the last (twentieth, but erroneously headed eighteenth) chapter will be one of the most interesting. It is entitled "The Darwin Memorial," and contains a series of addresses by American men of science, delivered at a special memorial meeting of the Biological Society of Washington soon after the death of the illustrious naturalist in 1882. The address of Dr. Theodore Gill, of the Smithsonian Institution, is a masterpiece of eloquence, treating of "The Doctrine of Darwin," and contrasting the doctrines of special creation and evolution. The address by William Dall, of the United States National Museum, is equally eloquent, and treats of Darwin in the form of a biographical sketch. Dr. John Powell, the Director of the United States Geological Survey, follows with an admirable address on "Darwin's Contributions to Philosophy." We cannot refrain from transcribing some of his remarks :—

"But Darwin's investigations have not ended research or completed philosophy. He brought scientific men to the frontiers of truth, and showed them a path across the border. Yet more than this he did. He pointed out one of the fundamental methods of research. Before his time philosophers talked about deductive methods and inductive methods. Darwin has taught us that both are fruitless. . . . By inductive methods, men are to collect facts, unbiased by opinions or preconceived theories. They are to gather the facts, put them together, arrange and combine them to find higher and still higher generalizations. But there are facts and facts—facts with value, and facts without value. The indiscriminate gathering of facts leads to no important discoveries. Men might devote themselves to counting the leaves on the trees, the blades of grass in the meadows, the grains of sand on the sea-shore ; they might weigh each one and measure each one, and go on collecting such facts until libraries were filled and the minds of men buried under their weight, and no addition would be made to philosophy thereby. There must be some method of selecting, some method of determining what facts are valuable and what facts are trivial. The fool *collects* facts ; the wise man *selects* them. Amid the multiplicity of facts in the universe, how does the wise man choose for his use? The true scientific man walks not at random through the world, making notes of what he sees ; he chooses some narrow field of investigation ; . . . his investigations are always suggested by some hypothesis—some supposition of what he may discover. He may find that his hypothesis is wrong, and discover something else ; but without an hypothesis he discovers nothing. . . . Working hypotheses are the instruments with which scientific men select facts. By them, reason and imagination are conjoined, and all the powers of the mind employed in research."

The succeeding address, by Dr. C. V. Riley, gives an account of Darwin's entomological work, and comprises a graphic description of the naturalist in his home, drawn from personal reminiscences of a visit to Down. Dr. Lester Ward follows with his address on "Darwin as a Botanist," in the course of which he discusses, among other points, the bearing of Darwin's researches on the power of movement in plants on the great question wrapped up in the expression "tendency to vary." Dr. Frank Baker contributes the next address, on the expression of the emotions, and in this we again meet with a spirited advocacy of the Darwinian method :—

"But not as a fact-gatherer do we find him greatest. Many others have struggled with ant-like toil to amass piles of facts, which, like the ant-heap, remain but sand after all. Darwin brings to his work an informing spirit, the genius of scientific hypothesis. Breathed upon by this spirit, the dry bones of fact come together 'bone to his bone,' the sinews and the flesh come upon them, they become alive and stand upon their feet, 'an exceeding great army.' He searches always for the principles which underlie the facts and make them possible, realizing that the *phenomena*, the things which are seen, are temporal and transitory ; the things which are not seen, the cosmical forces which govern and control, are eternal."

A Darwinian bibliography, by Frederick W. True, the Librarian of the United States National Museum, and an appendix giving a list of Darwin's works, conclude a volume of which enough has been said to commend it to all readers, whether youthful or adult, and which we on this side of the Atlantic cannot but appreciate as a most inspiriting picture of the life and work of the man who, of all others, has helped to emblazon our country's fame on the scientific scroll of the nineteenth century.

R. MELDOLA.

PINES AND FIRS OF JAPAN.

Monographie der Abietineen des Japanischen Reiches. Bearbeitet von Dr. Heinrich Mayr. Mit 7 Colorirten Tafeln. (Munchen : M. Nieger'sche Universitäts Buchhandlung, 1890.)

FROM the time of Kaempfer and that of Thunberg to our own day, the Japanese Conifers have been the objects of special predilection on the part of botanists.

Zuccarini figured and described several that had been collected by Siebold, Lindley, Andrew Murray, Maximowicz, Franchet, and others, contributed greatly to the elucidation and delimitation of the several species. Robert Fortune, John Gould Veitch, and Charles Maries introduced many to our gardens. Horticulture has, indeed, rendered great service in this matter. The trees in question are valuable for ornamental purposes, and potentially as timber trees. The consequence of this is that collectors have accumulated specimens in large numbers and in different stages of growth. They have, moreover, supplied our nurserymen with seed, so that young plants are now numerous in our nurseries and plantations.

The study of the seedling plants, in their progress from the seed-bed towards maturity, has afforded valuable evidence concerning the morphology of the group and its probable genealogy, its filiation and classification. Cultivation has, for instance, shown that many of the very curious forms known under the name of Retinospora are, in reality, stages of growth of one, or at least of a few, species of Thuya, of Cupressus, or of Juniperus, so that the so-called genus is purely fictitious. In like manner *Abies bifida* and *Abies firma* have been proved to belong to one and the same species.

To fill up the gaps in our knowledge, and to correct errors arising from inadequate or imperfect material, it was necessary that the trees should be studied by a trained observer in the forests themselves. This was the more necessary as, to a large extent, our knowledge has been derived from plants cultivated by the Japanese and, in some cases, not a little distorted in the process. The earlier botanists had little or no opportunities of studying the native flora for themselves. Even Fortune was largely dependent on the Japanese nurserymen. John Veitch collected for himself on Fusi-yama, and Maries penetrated even to the forests of Yesso. Dr. Mayr, the latest writer on these plants, has enjoyed opportunities denied to his predecessors. After a distinguished career in Munich, Dr. Mayr proceeded to the United States, visiting the forests in all parts of the Union, and producing, as a result, a work which the best judges speak of in terms of high appreciation. Subsequently, our author visited Japan to organize the Forest Department, and fill the office of Professor of Forestry in the Imperial University of Tokio. In the execution of his duties Dr. Mayr travelled through the various provinces, and derived much information from the native foresters. One result is before us in the shape of a volume, printed in German at Tokio, and illustrated with seven coloured plates. The group specially studied by Dr. Mayr is remarkable for the relatively large number of endemic species. Thus, Dr. Mayr enumerates six species of Abies, all of which are peculiar to the Japanese islands. Five species of Picea are nearly as much restricted in geographical area. Tsuga, a genus represented in both the North-eastern and the North-western States of America, as well as in the Himalayas, has two species peculiar to Japan. The genus Larix, which also has a wide distribution in the northern hemisphere, has two species native to Japan, and not extending far beyond its limits. Six species of Pinus are enumerated by Dr. Mayr, and these also are almost exclusively Japanese, though some are found on the mainland adjoining.

The Japanese islands, then, form a centre of distribution of a group of species of a distinct character, differing markedly from a similar group of Chinese nativity, but approximating to the Californian and to the East American coniferous floras, and having representatives in other parts of Northern Asia and of Europe. The distinct character of the Japanese Coniferæ and their relationships are even more prominently brought into view when the other tribes of Conifers are considered. Dr. Mayr confines himself, however, to the Abietineæ, and we must here follow his example, in the hope that on another occasion we may be able to accompany him also through the other tribes.

In speaking of the distribution of these plants, Dr. Mayr alludes (1) to the tropical zone in which the genus Podocarpus is represented, but which does not specially concern us now; (2) to a sub-tropical zone in which are other two species of Podocarpus, as well as *Pinus Thunbergii*, which extends round the coast of all the islands, and less frequently *Pinus densiflora;* (3) a region of deciduous trees, such as chestnuts in the south or at the base of the mountains, or beeches and birches to the northward or at higher altitudes. Here grow especially the Cryptomeria, the various species of Chamæcyparis, Thuyopsis, and Sciadopitys. (4) The fourth zone, that of firs and spruces, occupies the high mountains in the centre of the island. Here are found *Abies Veitchii*, *Picea bicolor*, *P. Hondoensis*, and *Larix leptolepis*, which are peculiar to the main island, together with *A. Mariesi*, *A. sachalinensis*, *Picea ajanensis*, and *P. Glehni*, which extend northward, some even as far as the Sachalin and Kurile Islands. *Tsuga diversifolia* occurs from the region of the beech upwards to the Alpine zone. (5) The fifth, or Alpine region, also designated that of the Alpine pines, includes forms such as *Pinus pumila*, which is allied to the Swiss *P. Cembra*. We can only indicate these regions, as the discussion of their climatal features and plant population turns mainly upon plants different from those which form the staple of Dr. Mayr's present treatise.

Passing into detail, Dr. Mayr proceeds to describe each species separately, devoting much space to literary references, Japanese as well as European, and giving a description of the main peculiarities of the tree from an economic as well as from a botanical aspect.

A few new species are indicated, of the value of which we can hardly form a trustworthy opinion in the absence of authentic specimens. We venture, however, to doubt whether *Abies homolepis* is, as, however, others besides Dr. Mayr think, identical with *A. brachyphylla*. The leaf structure of the two is certainly different, and cultivation may yet reveal other differences. The names *bicolor*, *Alcockiana*, *ajanensis*, *jessoensis*, *japonica*, *microsperma*, as applied to one or more species of Picea, have been so variously understood by botanists, owing partly to accidental misplacement of labels, admixture of seeds, and to imperfect information, that it is very important to have an authoritative statement from such an observer as Dr. Mayr. If allowances be made for a large amount of variability within the conventional specific limitations, it would seem from the figure as if Dr. Mayr's *Pinus pumila* might be referred to *P. Cembra*, whilst *P. pentaphylla* is obviously a near ally of the East American *P. strobus*.

Dr. Mayr's "diagnose," however, is really a rather diffuse description in German, not conveniently adapted for the comparison of one form with another. In this absence of concise comparisons in Latin, modern botanists, especially German ones, compare unfavourably with their predecessors. On the other hand, Dr. Mayr establishes some sectional characters which may prove useful, such as the three sections into which he divides the genus Picea, viz. Morinda, Casieta, and Omorica, the last, indeed, having been already proposed by Willkomm.

Hybrid forms between *Pinus Thunbergii* and *P. densiflora* are mentioned, as well as a whole series of garden varieties which have either originated in Japanese gardens or have occurred as "sports" on the wild trees, and which have been propagated by grafting by the Japanese gardeners. These are likely to prove of scientific interest, and will be specially interesting for garden purposes.

Seven quarto coloured lithographic plates accompany the volume, giving details of the foliage and cones. We could have wished that representations of the trees themselves could have been supplied, and that an alphabetical index of species and varieties had been added to the classified table of contents. When we have so much that is valuable and interesting presented to us, it may seem ungracious to hint at deficiencies, but really in this case to ask for more shows how greatly we appreciate what we have, and is about the greatest compliment we can pay to the author. MAXWELL T. MASTERS.

ELEMENTARY HYDROSTATICS.

Solutions of Examples in Elementary Hydrostatics. By W. H. Besant, Sc.D., F.R.S., Fellow of St. John's College, Cambridge. (Cambridge: Deighton, Bell, and Co., 1891.)

THIS is a collection of solutions, or a *crib*, to the author's well-known "Elementary Hydrostatics," which has held the ground in elementary instruction unchallenged since 1863.

It was cruel, though, as Dr. Besant apologetically explains, unavoidable, to keep the world of instructors waiting so long for these much-needed solutions and explanations of the questions in his Hydrostatics.

The Solutions are stated to be almost entirely drawn up by Mr. A. W. Flux, who has found it necessary to explain that the equation $p = g\rho z$ must be interpreted as giving the pressure p in *poundals per square foot* (or in C.G.S. *barads*, might well have been added); but he has not explained that the effect of this reverential interpretation is to make ρ and w signify the same thing; so that two symbols are used to denote the same quantity, although one, ρ, is called the *density*, and the other, w, the *intrinsic weight.*

But in 1863 the word *poundal* was not known, nor was any mode of measuring force and pressure in use, except in terms of gravitation units.

It would take too long to recount the despair of the instructor and the confusion of the student at the different modes of reconciliation of the equations $p = g\rho z$ and $p = wz$, variously used as measuring the pressure at a depth of z feet.

Because thirty and more years ago it was thought convenient in dynamical equations to replace W/g by a single letter M, merely for purposes of convenience in writing and printing, it was and is still taught in our theoretical treatises that the equation $W = Mg$ is the expression of a subtle and fundamental law of Nature, to be introduced even into a treatise on Elementary Hydrostatics, presumably taken up before a student has commenced Dynamics, and before he can understand what acceleration in general, and the particular acceleration g, can mean.

What must, for instance, be the feeling of Tommy Atkins, when the Musketry Instructor begins on p. 1 of the official "Treatise on Military Small Arms," 1888, with this definition of Mass, taken in a garbled form from chapter ii. of the Hydrostatics and elsewhere.

> "Mass: The *quantity of matter* in any body, the sum of all the particles of the body; it is proportional to the weight, whatever be the figure, or whether the bulk or magnitude be great or small; for the weight is equal to the mass multiplied by the force of gravity, or $W = Mg$, and the letters M and W are usually employed to denote the *mass* and *weight* respectively."

In short, this definition amounts to saying that *mass* is something we denote by the letter M, while *weight* is something we denote by the letter W; but we must always remember that $W = Mg$, where g is something unexplained, even when we measure mass in pounds and weight also in pounds; so that if g appears in one place, it will cancel again somewhere else, and not affect the ultimate numerical result.

But if, according to former instructions, we calculate the pressure from the equation $p = g\rho z$, we must notice that ρ, the density as defined in chapter ii., "Elementary Hydrostatics," is the weight in pounds of one-gth part of a cubic foot of the liquid, or $g\rho$ is the weight in pounds of one cubic foot of the liquid, so that $g\rho$ and w now measure the same quantity.

The unfortunate instructor was formerly called upon to reconcile these opposing statements, that w is sometimes the same as ρ, and sometimes as $g\rho$; now, however, he can take refuge behind the definite statements of this authorized collection of solutions.

But what is most wanted is a mathematical Censorship, to go through our hydrostatical treatises, expunging all the g's.

As to the mere mathematical geometrical part of the solutions, this is doubtless carried out with true Cambridge elegance, of which Dr. Besant is so well known an exponent; a trifle however, in comparison with the difficulty of the interpretation of the units in some extraordinary questions relating to the equations $W = sV$ and $W = g\rho V$, questions at one time considered a valuable test of clear thinking on the part of the student.

We counsel everyone who values his peace of mind to procure a copy of these Solutions, if called upon to interpret and expound the numerical results of the original "Elementary Hydrostatics"

A. G. GREENHILL.

OUR BOOK SHELF.

Plane Trigonometry for the Use of Colleges and Schools. With numerous Examples. By I. Todhunter, F.R.S. Revised by R. W. Hogg. (London: Macmillan and Co., 1891.)

TODHUNTER's "Trigonometry" is a very familiar friend of ours, and we have now before us a bundle of letters which we received from the author in 1861 and 1862, in reply to our criticisms and corrections of the early editions. The first edition swarmed with small *errata*, for the pointing out of which we received warm thanks. It was a good book for some years, on account of the excellent collection of problems, but of late it sadly wanted bringing up to date. Mr. Hogg has done his work well, but possibly he would have produced a better independent book. The first 200 pages have undergone very little change, and we have only noted here and there an interpolated article. Chapter xviii., "Miscellaneous Propositions," contains several novelties (as contrasted with the last edition we have of the original work), such as geometrical proofs of familiar formulæ and graphs of the trigonometrical functions. There are numerous important additions in chapters xxi.-xxiv., which bring this part of the work more *en rapport* with present day requirements, notably Schlömilch's resolution of sin θ into factors, and a too brief account of hyperbolic functions. The prime feature is the addition of a very great number of excellent recent exercises in all parts of the subject. The work forms a good school-book, and will meet the requirements of a large number of students.

Lessons in Astronomy. By C. A. Young, Ph.D., LL.D. (Boston, U.S.A., and London: Ginn and Co., 1891.)

THIS is the third of a series of text-books recently prepared by Prof. Young for use in schools and colleges of different grades. The two previous ones have already been noticed in NATURE (vol. xxxix p. 386, and vol. xli. p. 485). The present work is described on the title page as "a brief introductory course without mathematics, for use in schools and seminaries." The three books have much in common, and each one has many good points. We cannot help feeling, however, that the steps between them are too small. Almost exactly the same ground is covered by each, and they differ chiefly in the amount of previous knowledge assumed. But the acquaintance with mathematics required for a thorough comprehension of the "General Astronomy" is by no means great, and even for the "Lessons" a certain knowledge of geometrical principles is essential. If we must needs have three books, the "General Astronomy" contains too little, and the "Lessons"—a book of some 350 pages—contains too much.

The chief variation calling for notice is in the portion dealing with uranography. This now forms chapter ii., and, with the aid of the maps, forms a fairly complete and easy guide to the constellations. The notes on the legendary mythology of the constellations, which have been added for the benefit of students not acquainted with classical literature, gives this chapter an additional interest.

The book is brought well up to date, and is a model of good printing.

Cosmical Evolution: a New Theory of the Mechanism of Nature. By Evan McLennan. (Chicago: Donohue, Henneberry, and Co., 1890.)

THE author states that the essential principle of the new theory is "that every known heavenly body is connected with its neighbouring heavenly bodies by means of real, material bonds, and that every phenomenon of the universe, without exception, is due solely to the action of bodies upon one another through, and by means of, these bonds which join them together" (p. 48).

Among the principal evidences in favour of the existence of this material planetary connection is that "we actually see them with the naked eye" in the zodiacal light and in the streamers of the solar corona.

The theory is of a very general nature, and includes not only cosmical but terrestrial phenomena, such as aërial and aqueous tides, terrestrial electricity and magnetism. The author is of opinion that "the greater tidal wave is due to the sun, and the lesser to the moon" (p. 291).

The conditions of prelunar and other races of mankind are also discussed (p. 360). The work consists of 399 pages. There is no index.

The Telescope: an Introduction to the Study of the Heavens. By J. W. Williams. (London: Swan Sonnenschein and Co., 1891.)

THE writer of this book is author of "British Fossils, and where to Seek Them," and "Land and Water Shells, &c." In his preface he quotes with approval the adage, "Ground your knowledge of any special group on a general knowledge of nature as a whole." This is perhaps why he now turns his attention from shells to astronomy. However this may be, the work has been carefully compiled, and is to be recommended as a safe guide. Some of the illustrations are excellent.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Silver Lodes and Salt Lakes.

SINCE the discovery, some five or six years ago, of the extraordinary Broken Hill lode of silver-bearing ores, the public excitement on the subject in this part of the world has been attended with comparatively little scientific interest in regard to the geological features of the argentiferous country and the probable origin of deposits so vast and so remarkable in character; yet I believe that an examination of the main topographical and geological features of the eastern parts of South Australia and the western parts of New South Wales will probably throw more light upon the interesting subject of the origin of argentiferous lodes than the study of any other now known part of the globe; and, as I have had an opportunity of going closely into the matter during a recent visit to Broken Hill, I propose to lay briefly before your readers a few facts which seem to afford presumptive evidence in favour of the supposition that salt lakes and silver lodes are causally connected.

An examination of the ores *in situ* at Broken Hill, and especially in the portions of the lode which are known as blocks 10 and 11, reveals the fact that stratification almost exactly similar to that of an ordinary alluvial deposit is practically universal throughout the lode. So obvious has this been from the very beginning of the working, that almost every mining man who has had anything to do with Broken Hill has remarked upon the very obvious fact that the ores are to be ascribed to an aqueous origin. The fissure in which the lode occurs varies from a few feet to seventy or eighty yards in width, and has almost vertical walls. Within these boundaries the stratified deposits of carbonates and chlorides are intermingled with immense bodies of kaolin and sulphides, with a considerable amount of an interesting silicate of zinc also carrying silver and lead. The Barrier District is one of the driest in the whole of this very dry continent, and there is no river within about seventy or eighty miles. The few intermittent watercourses which exist in the locality do not suggest anything but a dry and arid climate. In fact, the greatest difficulty now met by the mines and by the town of Broken Hill, which contains about 27,000 inhabitants, is the scarcity of water, and the doubtful nature of any catching grounds that have been suggested. If, therefore, water was the agency by which the deposits of ore took place, it is evident that the conditions at the time must have been very different from what they are at present.

The key to the whole situation seems to lie in the fact, which has been so well pointed out by Mr. Alfred Russel Wallace and others, that the whole of the regions of Central Australia have emerged from the ocean at a period which, from a geological point of view, is comparatively recent. The axial lines of the watershed ranges appear to be rising at a more rapid rate than the neighbouring plains, and consequently some strange and interesting changes are taking place in the relations of the catchment areas of flood waters and their outlets. In the locality of Innamincka, almost due north from Broken Hill, there occurs a phenomenon which is obviously due to some such change of relations. The Strezlecki Creek runs to the south-west from Innamincka. Its bed holds immense deposits of drift-sand, and in the adjoining plains are to be seen many curious parallel ridges of sand-hills, all strongly suggestive of the action of drift water, such as at times passes across the surface of these vast interior plains at flood time. The Strezlecki Creek was apparently the outlet for most if not all of the water of Cooper's Creek at some period of time not at all remote. But at the present day it is only once in every four or five years that the stream runs at all. When a very high flood fills the bed of the Cooper to overflowing, the waters find their way over the low ridge of land which separates the present bed of the Cooper from that of the Strezlecki, and so on to Lakes Blanche and Gregory—those large salt evaporation pans which can scarcely with propriety be dignified with the name of lakes. The gradual elevation of the low ridge would appear to be the most probable explanation of this interesting phenomenon. Now, to the south of Broken Hill, and in the vicinity of the River Darling, there is to be found ample evidence of a somewhat similar occurrence. Vast accumulations of sand in parallel ridges are still to be seen crossing the plains, and a large river bed extends, from a place quite close to the junction of the Darling and Murray, northwards in the direction of Broken Hill. Whether this "Anabranch," as it is called, is really an old bed of the River Darling or not, I will not stay to inquire. It would, however, appear practically certain that some slight alteration in the level of the land has been responsible for the change in the direction of the flow of water.

The case is not an ordinary one of the diversion of a river owing to the accumulation of its own alluvium; and the sand ridges, which in places extend right down to the bed of the present river, suggest the action of water on a scale of magnitude very different from that which is at present to be seen. Here, then, we have both to the north and to the south of Broken Hill evidences of the existence in former times of floods of water, which at the present day are never at all to be seen on the southern side, and only once in every four or five years on the northern side. It is to be observed that both of these localities are within the line of country formed by the parallel mountain systems of the Flinders Range on one side and the Grey and Barrier Ranges on the other side. Between these two ridges the land slopes gradually to the west, and three salt lakes, of which the largest is Lake Frome, attest the fact that in all probability at one time vast quantities of sea-water were imprisoned by the rising of the land from the level of the ocean. But in South Australia these two ridges are joined by a band of high land, on which the present railway to Broken Hill has been laid. This band of country forms, along with the ranges at each side, a sort of *cul de sac*, from which at present the waters could have no escape to the southwards unless they rose to a higher level than is ever noticeable under existing conditions.

But this has not been the case in times gone by. The evidences of the action of water in the neighbourhood of the "Anabranch" make it to appear practically certain that at one time the flood waters, which swept through the salt lakes, must have poured over the ridge towards the River Darling, and found an outlet by that means. This, then, brings me to the most significant fact to which I wish to direct attention. The locality of Broken Hill is the lowest point in the axis of the line of country which forms what I have alluded to as the *cul de sac*. In the absence of any survey from which full data could be deduced, it is sufficient to take the levels of all the railway stations from the Flinders Range to Tarrawingie—a point forty-two miles north-east of Broken Hill. These show that the railway (which happens to follow the line of the ridge) dips gradually to Broken Hill, and then rises again towards Tarrawingie. The conclusion is therefore inevitable, from the data to which I have already referred, that Broken Hill is the locality at which the accumulations of flood-water from the great region of the salt lakes must have found their way across the connecting ridge and on towards the River Darling. I believe, if the localities of the silver lodes of Potosi and Comstock are examined, they will be found to bear somewhat the same relation to the extensive salt marshes south of Lake Titicaca, and to the salt, mud, and alkaline lakes respectively, that Broken Hill does to Lakes Frome, Blanche, and Gregory; but, in the case of the last-named, the time at which the action took place is apparently much more recent, and the evidences which it has left are, therefore, all the more evident. It is a significant fact that the whole of the horseshoe-shaped line of country, of which, as I have said, Broken Hill is the lowest point, is highly mineralized, and contains mines for gold, silver, copper, and lead; but of all these mines, the Broken Hill lode is really an epitome, containing, as it does, nearly every metal which is known to the practical miner, and some also of those which are more of scientific than of practical interest. In view of the existence, among other things, of large lateral shoots from this gigantic lode containing the largest specimens of native silver yet discovered in any part of the globe, it seems difficult to account for some of the phenomena present at Broken Hill without premising the agency of electro-deposition. Several of the arguments which were adduced by me in NATURE of March 20, 1890, in regard to the occurrence of gold, would appear to furnish equally strong presumptive evidence that earth-currents acting along the axis of the range have had something to do with the deposition of metals from their solutions during their passage across the ridge. My present purpose, however, goes no further than to call attention to the probable origin of the lode, which I believe is to be found in the minerals held in solution in the waters of some of the vast Australian lakes and evaporation pans. GEORGE SUTHERLAND.

Adelaide, South Australia.

A Magnificent Meteor.

AT 1.15 a.m., on July 31, I observed a most magnificent meteor—a veritable Andromedes. It was much larger than Jupiter, which was on my right, in the constellation Pisces, shining to the best advantage on a dark blue sky. After the retina of my eye got clear of the dazzling light of the meteor, I turned to Jupiter, which was in a favourable condition for comparison, the clouds being opportunely absent, but it looked at least three times smaller than the meteor, which, indeed, was entitled to be called a fire-ball. It illuminated the whole district with the brilliancy of the noonday sun. I traced it back through Mira Ceti, and to the right of Mesartum Arietis, into the direction of λ Andromedæ. This magnificent meteor exploded near the earth, without any detonation. The light was perfectly white. There was a very peculiar feature in the apparition of this meteor: it appeared to be very near to me, and between its body and the horizon behind it there seemed to be a vast distance. In its explosion it assumed very large dimensions, and the effulgence lasted for three seconds with undiminished splendour. In reality there were two explosions. The trail of light was dim, except immediately behind, where it was thick and bright, but of short duration.

On the night of July 31, and on the morning of August 1, there was a brilliant display of stars for this time of the year; the Milky Way was well defined from horizon to horizon, denoting a certain degree of frost. At present, Jupiter is the most conspicuous and most brilliant ornament in the nocturnal heavens; here in Scotland its glory is enhanced by the somewhat frosty nights which occasionally visit us about this season. DONALD CAMERON.

Paisley, August 3.

Bees and Honey-dew.

NEAR here is an avenue of alternate beech and oak trees, and, in walking through it, my attention has lately been drawn to a loud humming in the beeches, similar to that heard in lime trees when in flower, while the oaks are silent. The sound is, I find, produced from bees in search of the Aphis secretions on the leaves of the beeches, the under sides of which are sticky with the substance. The bees appear to be all of one type—a small size of the large humble-bee—with a white tail. They never settle on the under sides of the leaves direct, but just on the margins and then creep underneath, when, after running about and exhausting the supply, they fly off to another leaf, exactly as if they were visiting flowers. The leaves of the oaks are clean, and have no "honey-dew" on them. F. M. Burton.

Highfield, Gainsborough, August 5.

Dredging Products.

Amongst the products of the dredgings which my friend the Rev. J. H. Crawford and I are procuring from the Voe here, I am glad to be able to record the presence of Actinotrocha. We only got two or three specimens at first, but to-day a large number was procured from the surface net. One or two have attained to the Phoronis condition since being brought in. They answer in all respects to *Actinotrocha branchiata*, but seem to be as a rule less pigmented than the specimen found in St. Andrews Bay.

Actinotrocha branchiata has now been found on both sides of Scotland and England, and also at Heligoland; but, besides being got in the North Sea and on the west coast of Britain (*vide* Nature, vol. xxxiv.), it seems also to be found on the western side of the Atlantic, for Wilson records it from Chesapeake Bay. It is thus distinctly a northern form, but has a wider distribution than has hitherto been supposed.

Alexr. Meek.

Sullom, Northmavine, Shetland, August 4.

THE INTERNATIONAL CONGRESS OF HYGIENE AND DEMOGRAPHY.

NEVER before, perhaps, in the history of science has there been assembled together such a numerous gathering of eminent men of science of different nationalities, or representing so many countries, for the purpose of discussing scientific problems.

Although it is little to the national credit that the importance of international Conferences on Health was suggested by the Belgians and not by ourselves, the conditions we are under here must not be forgotten. All other civilized countries have strongly represented among their Ministers, and among administrators, men of knowledge and competence; and elsewhere such Congresses are treated as of national concern.

Here, even in the matter of health, such powerful and economical methods of obtaining and distributing knowledge, such as Congresses like the present afford, are absolutely ignored by the party politicians to whom we commit our national welfare.

There can be little doubt that most of the good which is certain to arise from the deliberations now going on must be ascribed to the Queen and Prince of Wales, who came forward as Patron and President of a Congress ignored, as we have said, by our party rulers. This has been pointed out by the *St. James's Gazette*:—"The Prince of Wales has rendered a not inconsiderable service to his country by good-naturedly pulling the Congress out of the fire, and rendering a partial success of what came near to being a sad *fiasco*. But for his complaisance in sacrificing his holiday in coming up to London to take the chair, no public personage would have been present to welcome the two or three thousand guests bidden to the metropolis, or to give attraction and dignity to the opening meeting. . . . There are three Ministers whose departments have relation to the subjects treated by the Congress: Mr. Ritchie, who is our *quasi* Minister of public health and relief; Mr. Chaplin, whose department deals with the hygiene and prevention of disease of animals; and Lord Cranbrook, who controls medical education. Not one of these Ministers was present yesterday. Not even the Registrar-General, the head of the department of vital statistics, or a representative of the Home Secretary, took part in yesterday's meeting. The Prince, however, saved the position."

The devoted and unpaid labours of many eminent men have, however, with this slight touch of national feeling in high quarters, already rendered the success of the Congress unparalleled, and it is really wonderful to see what they have done, in spite of the enormous difficulty of arranging for a large number of people in such a city as London. Even the facilities afforded by Burlington House and the University of London buildings do not include a hall large enough for an adequate reception room; at first, therefore, there were difficulties, largely owing to its absence. This will hardly be wondered at, when we state that the numbers enrolled already are about 3000, and that there are 40 delegates from the German Empire and 70 from India, only to give two instances.

In anticipation of the meeting, among other official documents too numerous to mention, was prepared a Hand-book to London, with special reference to the needs of the members. This is a volume of 250 pages, in French and English, with eight plates showing the position of hospitals, cemeteries, markets, and the like. This has been published by Messrs. Cassell. There is another volume of 233 pages, containing abstracts of the more important papers to be read. Nor have the English Committee been the only workers. We have "Denmark: its Medical Organization, Hygiene, and Demography," with numerous illustrations and maps, published in English by authority of the Danish Government in time for the International Congress. This has been published by Messrs. Churchill.

In spite of the abstentation of any notice on behalf of the Government, it is pleasant to note the way in which the Lord Mayor and the Corporation, the Royal Colleges of Physicians and Surgeons, and numerous other public bodies and private individuals have kept up the credit of the nation for hospitality. Among the *conversazioni* must be specially mentioned that at the Guildhall on Tuesday evening, when the Lord Mayor received the members of the Congress. It was a brilliant and impressive sight, enhanced by the uniforms of foreign officers, and the unfamiliar garbs of members of our own distant dependencies. The various social arrangements made by the organizing committee are recorded in a special pamphlet of fourteen pages.

The proceedings began on Monday by a meeting in St. James's Hall, presided over by the Prince of Wales. Sir Douglas Galton first presented the Report of the Permanent International Committee, and *inter alia* gave the following account of the general organization:—

"The work of the Congress has been arranged in two divisions, viz. hygiene and demography, and it has been found necessary to divide the former into nine sections, each under a separate president, and with separate organization. Committees have been organized in foreign countries to further the interests of the Congress in a more direct manner than could be done from England. Delegates have been appointed by all the Governments of Europe, and also by the United States, Mexico, Venezuela, Japan, Persia, Egypt, by the provinces and native states of the Empire of India, by the most important colonies, and also by numerous municipal authorities, universities, scientific and medical societies, and other institutions throughout the world, and large numbers of the most important authorities on the subjects to be treated of have sent communications to be laid before the Congress."

After the reading of this Report, the Prince of Wales opened the proceedings by a careful and sympathetic address. One part of it referred to the dangers to health inevitable to the conditions under which we live. He remarked in relation to these dangers:—

"It will be no trivial work if their sources and probable remedies can be clearly pointed out, and especially if this can be done, as in a Congress such as this it should be, in a strictly scientific manner, calmly and dispassionately, without any reference to either general or municipal politics, or for any other purpose than the promotion of health. It is only on conviction such as may thus be produced that the appointed sanitary authorities can compel the changes necessary to be made; for such changes are almost always inconvenient or injurious to some, and might even seem unjust to them, unless it be made quite clear that they would be very beneficial to the community. But my hope is that the work of this Congress may not be limited to the influence which it may exercise on sanitary authorities. It will have a still better influence if it will teach all people in all classes of society how much everyone may do for the improvement of the sanitary conditions among which he has to live. I say distinctly 'all classes,' for although the heaviest penalties of insanitary arrangements fall on the poor, who are themselves least able to prevent or bear them, yet no class is free from their dangers or sufficiently careful to avert them. Where could one find a family which has not in some of its members suffered from typhoid fever or diphtheria, or others of those illnesses which are especially called 'preventable diseases'? Where is there a family in which it might not be asked, 'If preventable, why not prevented?' I would add that the questions before the Congress, and in which all should take a personal interest, do not relate only to the prevention of death or of serious diseases, but to the maintenance of the conditions in which the greatest working power may be sustained."

The *Times*, in a leading article on the Prince's address, points out one very important practical matter in which we lag far behind many foreign countries, and which may serve as an excellent illustration of the Prince's words about inconvenience or apparent injustice to individuals. "The weak point of English sanitary law is in respect of regulations for the slaughter of animals. In London, for example, slaughterhouses are small private establishments, often situate up little alleys or courts, surrounded by dwelling-houses, and not only destitute of many conveniences which they should possess, but also affording great facilities for the slaughter of diseased animals, and for the distribution of their flesh as food. In many Continental cities public *abattoirs* have been established upon a large scale, and all private slaughtering is forbidden. At these *abattoirs* there is an abundance of space, of air, of light; there is an excellent water supply; and the slaughtering is conducted under the supervision of officials, governed by rules which not only protect cattle against unnecessary cruelty or ill-usage, but which provide for the systematic inspection of meat before it is permitted to be sold. We shall certainly hear a good deal, during the sitting of the Congress, as to the importance of preventing the consumption of the flesh of tuberculous animals; but this, however important it may be, can never be done while the innumerable small private slaughterhouses are suffered to remain."

At the conclusion of the Prince's address, speeches were delivered by representatives of France, Italy, Austria-Hungary, Saxony, and Prussia. It is pleasing to record that all bore high tribute to the part which has been played by England in the promotion of measures calculated to preserve and improve the public health. On this point, Dr. Brouardel (France) was indeed specially emphatic:—

"In the year 1837, the year of the coronation of Her Gracious Majesty, appeared the Act which rendered obligatory the registration of deaths. This Act inaugurated the era of administrative reforms concerning the public health which our valued colleague of the Local Government Board has rightly called 'the Victorian era.' This Act did not long remain alone. Under the impulse given by two of your most illustrious patriots, William Farr and Edwin Chadwick, you have organized a system of registration of the causes of diseases and of deaths. Certain important cities, before the law made it obligatory, obtained supplies of water beyond all suspicion of pollution, and adopted systems of removal of foul water and waste matters. In these cities, whose action cannot be too much praised, the sickness and death rates diminished rapidly; this furnished the necessary proof it was time for reform. Twenty years ago the Local Government Board was established, and in 1875 had submitted to Parliament a Bill for the protection of the public health. During its discussion in Parliament one of your greatest Ministers (Disraeli) pronounced in the House of Commons these memorable words, which should be repeated in all countries and in all Parliaments: 'The public health is the foundation on which repose the happiness of the people and the power of a country. The care of the public health is the first duty of a statesman.' Since this, each year you have made fresh improvements in your sanitary laws; if in your eyes they are not perfect, in the eyes of the nations who surround you they are an ideal towards which all their most ardent aspirations tend. It is your example they invoke when they claim from the public authorities the powers necessary to oppose epidemics, to combat the scourges which decimate their populations. You have taken the first rank in the art for formulating laws for the protection of health; this is not all that you have done in the domain of hygiene. Among the diseases which one can properly term pestilential, there are, thanks to the work of the hygienists of all countries, certain ones which from the present time may be considered as preventable: such are small-pox, typhoid fever, dysentery, and cholera. For one of these, the most terrible, the immunity conferred by vaccination is absolute. The person upon whom this immunity is conferred can pass through the most severe epidemics, and expose himself to all sources of contagion without being affected. Who is it who thus preserves from death, from blindness, from infirmity, millions of human beings of all countries and of all races? On May 18, 1796, a date which might well be the date of a great battle, Jenner inoculated with vaccine matter by means of two superficial incisions, the youth James Phipps. Protection against small-pox belongs to you; the world will be to you for ever obliged. Let us consider two other epidemic diseases. Is it possible to establish the conditions of propagation of typhoid fever without quoting the names of Budd or of Murchison? I am aware that in 1855 Dr. Michel de Chaumont had for the town in which he lived experimentally established the *rôle* played by drinking-water in the propagation of this disease. Unhappily, public opinion was not prepared, and his discovery was not listened to. In the work which we are considering, the efforts of the English school were most fruitful. May I recall the fact that it was the epidemic of cholera in 1866 in England, which gave birth to the theory of its propagation by drinking-water? Was it not at that date that, under the influence of your hygienists, the Lords of the Privy Council issued an order formulating the laws of prevention which we adopt to-day? Certain it is that even in England these discoveries have not immediately borne all their fruit. The anti-vaccination leagues are not yet dead. Proofs accumulated during a century have not sufficed to cure that mental blindness which is congenial. . . . Can France be represented in a Congress of Hygiene without recalling the name of M. Pasteur? For centuries we have asserted that epidemic diseases were propagated by means of contact, by the air, by the effluvia, by miasmata. The idea of morbific germs, if not the name, is even found in the works of Hippocrates, but in what an uncertain sense. The theory of contagion has passed from century to century with strange modifications; the uncertainty of the methods of research and the difficulties of observation bound up together truth and error. It remained for Pasteur to prove the existence of these germs, their form, their life, their mode of action, and by their attenuation to solve the problem of immunity. Thanks to his work, and thanks to those of his pupils, realities have succeeded to contingent possibilities. We know some of our enemies, their habits, and their mode of penetrating the body; up to this time man was conquered by these infinitesimal beings, but, thanks to recent discoveries, he will be their conqueror. When, at the beginning of a century, one can inscribe the name of Jenner, and at its end that of Pasteur, the human race may rejoice. More has been done for it against misery, disease, and death than in any one of the centuries which have preceded it. You, gentlemen, you have been the initiators; this title will never be disputed with you. When a great people has given such an example; when, by her gracious patronage, Her Majesty the Queen, and when, by his presence, His Royal Highness the Prince of Wales, testify that for them this era of reforms is not closed, it is only right that those who try to imitate them, and to give their country similar institutions, should come to bring to that people, and to their Sovereign, the homage of their profound respect."

Dr. Van Coler, the Medical Director-General of the Prussian Army, the representative of the German Government, followed suit, and showed the aid rendered to armies by the improvements in sanitary science. We give the following short extracts from his speech:—

"It is indeed with a feeling of joyous pride that from this place and in this country, where we have to trace the very cradle of all modern science of public health, I am permitted to point out how the many efforts made in the direction of hygiene radiating from England were, especially in Germany, hailed with much delight; where they received the most careful attention, and where they ever since have been most actively promoted. . . . If from our army, diseases like malaria, small-pox, dysentery, have completely, or almost completely, disappeared; if typhus fever and diphtheria become more and more diseases of the past, we have to be thankful for these attainments to the development and application of hygiene. . . . It is now an established fact that infectious diseases are by no means a necessary evil in the army. They are simply diseases which can be avoided, which can be powerfully opposed, and against which the science of our days battles victoriously with ever-increasing success."

Dr. Körösi's address will be welcome to many, as he exactly defined demography—which is a puzzle to many outsiders—and pointed out the early work done by members of the Royal Society:—

"This branch of science, the very nucleus of statistical work, which, in fact, is quite a science in its own right, has chosen the task to investigate the laws which regulate the life, increase, and decrease of nations. Its work, therefore, comprises three main parts: statistics of natality, of mortality (this part including biometry, the science of measuring the duration of human life), and of the increase of population. And when inquiring now who were the founders of this new science, we shall hear unanimously quoted the names of England's sons—Graunt, Petty, Halley, Malthus. Gentlemen, to-morrow, when we are to begin our work, we shall meet within the venerable hall of the Royal Society. The representatives of demography must feel a deep emotion when entering those rooms, which are so intimately connected with the history of their science, for this is the place where, 220 years ago, demography was created. It was in those halls, in their very first youth then, but soon conspicuous to the whole world by the genius of Newton, that appeared the work of Graunt which forms the starting-point of demography; and here the King himself, admirably appreciating the work done, recommended the author to be received as a member of the learned Society. It was there that shortly afterwards Sir William Petty, by his eminent power, raised the new science to political importance and to popularity, and in the same place, again, in 1693, the famous Halley became the founder of the most important part of demography, of biometry, by working out the first table of mortality. And now the young science, which two centuries ago left those halls shy and even without a name, has found its way over the whole globe. Having been worked out in Germany, having received a name and new ideas in France, and having been enlarged and imbued with a more scientific character by Quetelet, having got its well-equipped office in every country of the civilized world, we are proud to see now its numerous representatives meet at the same place where two centuries ago this science was born. After a triumphant career of 220 years, it returns to its home, to the old rooms in which it awoke to light, and again the Throne of England receives it with favour and benevolent interest. For demography not less than for all statistical work, it is of the highest importance that its representatives, scattered as they are over the whole globe, should fully understand each other, for only so we can accomplish our aim, that our observations comprise equally all countries of the world, that our researches are conducted and worked out on the same principles everywhere, and that we may unite the incomplete and often discrepant descriptions of the single nations to a full descriptive history of the whole of civilized mankind. This great aim fully deserves the praise the illustrious Prince Consort bestowed upon it from this very place thirty years ago. He said, 'The importance of the Congresses cannot be over-rated; they not only awaken public attention to the value of these pursuits, bring together men of all countries who devote their lives to them, and who are thus enabled to exchange their thoughts and varied experiences, but they pave the way to an agreement among different Governments and nations to follow up these common inquiries in a common spirit by a common method and for a common end.'"

The meeting was subsequently addressed by Sir James Paget, Dr. G. Buchanan (of the Local Government Board), and others.

The Sectional work of the Congress began on Tuesday. The Divisions and Sections are as follow:—

Division I.—Section 1. Preventive Medicine. President, Sir Joseph Fayrer, K.C.S.I.—Section 2. Bacteriology. President, Sir Joseph Lister, Bart.—Section 3. The Relation of the Diseases of Animals to those of Man.—Section 4. Infancy, Childhood, and School Life.—Section 5. Chemistry and Physics in Relation to Hygiene. President, Sir Henry Roscoe, M.P.—Section 6. Architecture in Relation to Hygiene. President, Sir Arthur W. Blomfield, A.R.A.—Section 7. Engineering in Relation to Hygiene. President, Sir John Coode, K.C.M.G.—Section 8. Naval and Military Hygiene. President, Lord Wantage, K.C.B., V.C.—Section 9. State Hygiene. President, Lord Basing.

Division II.—Demography. President, Mr. Francis Galton.

We shall endeavour next week to give an idea of the results of the many important discussions which may be anticipated, but it is already clear that it will be impossible for us to give anything like a full report, for the programme of work to be gone through is enormous. The addresses of the various presidents on the opening day were in themselves important communications, and well fitted to give tone to the subsequent discussions.

PROGRAMME OF TECHNOLOGICAL EXAMINATIONS.

A SIGN of the general advance in technical education is shown in the new Programme of Technological Examinations just published by the City and Guilds of London Institute. The Programme contains 37 pages of additional matter, and the number of different subjects of examination has now reached sixty. The Council appear to be genuinely desirous of adapting the examinations to the conditions of the more important trades as practised in the principal centres of industry. To this end, many of the sixty subjects are divided into different sections, corresponding to the separate branches of the same trade, or to the practice of the trade in separate localities.

In the new Programme we notice many important additions. A practical test, which is the surest preventive of cram, and excludes those who are not engaged in the trade from presenting themselves for examination, has been added to the syllabus of nearly all the subjects. Thus, next year, for the first time, there will be practical examinations in such widely different subjects as photography and boot and shoe manufacture. In many subjects dealing largely with the practical applications of science the syllabus has been entirely re-written. This is the case with "Electrical Engineering," which is now divided into two main subjects—"Telegraphy" and "The Transmission of Power"—the former being again subdivided, in the honours grade, into "Telegraphy" and "Telephony," and the latter into "Electrical Instruments," "Electric Lighting," and "Dynamos, Motors, &c." The subject of "Mechanical Engineering" is similarly divided into different sections. The Programme has been increased by the addition of a syllabus of instruction in "Goldsmiths' Work," in which subject a large class has been already established in Birmingham, and of a syllabus in "Ship Carpentry and Joinery," which is intended to meet the requirements of artisans engaged in the different shipbuilding yards throughout the country.

The continuous increase in the number of candidates for these examinations, and in the number of students receiving instruction in the different centres throughout the country, shows that there is a genuine demand among artisans for practical and concrete instruction dealing, in the first place, with the facts with which they are familiar in their every-day work, and, afterwards, with the scientific principles explanatory of those facts. From the table found on p. 17 of the Programme, it appears that this year 7322 candidates presented themselves, as against 6667 in the previous year, and that the number of students under instruction increased from 12,022 to 13,202.

The memorandum issued to County Councils, to which we have already referred in these columns, is re-published in the Programme. It draws the attention of County and Borough Councils to the fact that, after the examination in May 1892, the grants hitherto paid on the results of the examination will be withdrawn, and that a substantial portion of the funds thus set free will be devoted to the improvement of the machinery of the examinations. Indications of the direction in which these improvements will be made will be found in the new Programme. It is important that the managers of technical classes should fully understand that, in future, the maintenance of such classes will depend entirely on local support. The large sums placed at the disposal of County Councils clearly render it no longer necessary that the City Guilds Institute should continue to make grants on results, which, although amounting in the aggregate to a large sum of money, proved to be quite inadequate to properly support the classes. It is, however, to be feared that the grant-earning tendency of the teachers and managers of local schools may cause the distinctly technological subjects of instruction to be neglected for the sake of science subjects by which grants may still be obtained from South Kensington. To prevent this, it is necessary that County Councils should realize the full importance of the work which Parliament has thrown upon them, and should recognize that in future they will be the authorities responsible for the conduct of the technical and, indeed, the secondary education also of the county. In the competition for money grants, technical subjects will be placed at a distinct disadvantage as compared with ordinary science subjects, and it is the more necessary, therefore, that the teaching of these subjects should receive adequate support from local authorities.

In order that the teaching in different localities may be duly adapted to the trades practised in those localities, and may be regulated by these requirements, and not by the grant-earning capacity of the subjects of instruction, it is very desirable that County Councils should organize, independently, or in connection with the City Guilds Institute, a system of inspection of local classes. The value of examinations is immeasurably increased when they are supplemented by inspection by competent experts, and it is to be hoped that some system of inspection of technical schools, which shall include the methods of instruction adopted, will soon be organized.

The Institute's Programme offers to different localities a wide choice of trade subjects, ranging from simple handicrafts to industries involving some of the most difficult applications of physical and chemical science. To the syllabus of each subject is added a valuable list of works of reference, which forms by itself a very complete guide to books in technology. The list of examiners, many of whom have this year been newly appointed, includes well-known experts in each branch of trade, and is a guarantee of the efficiency of the examinations. The future development of technical education is now very largely under the control of County Councils. They possess the funds without which no real progress can be made. But, besides funds, experience and organization are needed, and there can be no doubt that the members of County and Borough Councils will derive much valuable information, and many serviceable suggestions, from the new edition of the City Guilds Institute's Programme of Technological Examinations.

BOTANICAL SURVEY OF INDIA.

The organization of a Botanical Survey of India, which has been under consideration since 1885, has been finally settled by the following resolution of the Government of India, dated Calcutta, February 26, 1891:—

(1) The scheme for carrying out the botanical survey of India, which has been under consideration for some time, was finally completed a year ago, and His Excellency the Governor-General in Council considers that it is now desirable to publish the details for the general information of local Governments and Administrations.

(2) In February 1885, Mr. Thiselton Dyer, Director of the Royal Gardens at Kew, prepared for the Government of Madras a Memorandum on the constitution of a Botanical Department for the Madras Presidency, one result of which was the eventual establishment of a Botanical Department for that Presidency. In sanctioning the Madras Department, the Secretary of State for India took the opportunity to suggest for the consideration of the Government of India whether, without interfering with the control exercised by the Provincial Governments, it would not be possible to bring into communication the various Botanical Departments of the different Provinces, the desirability of such an association having been prominently noticed by Mr. Thiselton Dyer in his Memorandum of February 1885. The wider scheme thus suggested by the Secretary of State was accordingly considered; and the first step taken for the organization of a Botanical Survey for all India, which was to have its centre in the Royal Botanical Gardens at Seebpur, Calcutta, was the transfer from the control of the Government of the North-Western Provinces and Oudh, to that of the Government of India, of the Superintendent of the Botanical Gardens at Saharanpur. This measure was demanded by the need for botanical survey in the Punjab, Rajputana, Central India, and the Central Provinces, which had hitherto been unrepresented by any

botanical officer, as well as by the necessity for having a botanical officer at the disposal of the Government of India to accompany military expeditions beyond the frontier.

Arrangements were then made, with the concurrence of the local Governments concerned, under which the following territorial division of India was prescribed for the purposes of botanical survey :—

Under the Superintendent, Royal Botanical Gardens, Calcutta.—The Provinces of Bengal, Assam, and Burma, the Andamans and Nicobars, North-East Frontier Expeditions.

Under the Government Botanist, Madras.—The Presidency of Madras, the State of Hyderabad, the State of Mysore.

Under the Principal, College of Science, Poona.—The Presidency of Bombay, including Sind.

Under the Director, Botanical Department, Northern India.—The North-Western Provinces and Oudh, the Punjab, the Central Provinces, Central India, Rajputana, North-West Frontier Expeditions.

The distribution above stated was reported to Her Majesty's Secretary of State, and his Lordship has been pleased to express his satisfaction with these arrangements.

(3) The Government of India now desire to communicate the following observations as to the central position which, in conformity with the suggestions of the Director of the Royal Botanical Gardens at Kew, the officer at Seebpur will occupy in the scheme for the botanical survey of India, and as to the sphere and nature of duties of each botanical officer, so far as they are connected with botanical survey.

It is desirable that the Seebpur Institution—which, as remarked by Mr. Thiselton Dyer, "though technically Provincial, must, at any rate in external estimation, from its age (it has passed its centenary), from its scientific traditions, and from the splendour of its maintenance, rank as Imperial"—should, without any interference with the Provincial control over the Royal Botanical Gardens, be officially recognized as the acknowledged centre of the Botanical Survey of India, and that to it should be referred the solution of all problems rising out of the practical or scientific study of Indian botany. In view of the important position which the Superintendent of the Royal Botanical Gardens, Calcutta, will thus occupy as the central authority in the Botanical Survey of India, the Government of India have, with the concurrence of the Secretary of State, added to Dr. King's present designation the official title of "Director of the Botanical Survey of India," and it is requested that in all correspondence dealing with subjects relating to general botanical exploration the latter title should be employed. The more effective botanical survey of Burma and Assam has also been intrusted to the Director, who will arrange a definite programme each year for the purpose in communication with the Chief Commissioners of those Provinces. He will also submit a separate Annual Report on the botanical exploration and researches effected during the year. The Government of India record with satisfaction that the local Administrations of Burma and Assam have each contributed an annual grant from Provincial revenues as an addition to the Imperial grant for the botanical survey of their provinces.

The investigation of the flora of the Madras Presidency and of the Hyderabad and Mysore States has been intrusted to Mr. M. A. Lawson, the Government Botanist and Director of Cinchona Plantations, who has expressed his opinion that the whole survey of the territories in question might, if diligently prosecuted, be completed in three or four years.

In Bombay, a scheme involving an annual expenditure of Rs. 4500 per annum on botanical work has been sanctioned, and Dr. Cooke, Principal of the College of Science, Poona, is officially recognized as in charge of botanical research in that Presidency. A herbarium exists at the College of Science, and a botanical collection is in course of formation at the Victoria Gardens, Bombay. The former place is to be the head-quarters of botanical research and collections, and the existing herbarium there is to be developed.

By the transfer of the services of the Superintendent of the Government Botanical Gardens, Saharanpur—who now bears the designation of Director of the Botanical Department, Northern India—the services of this officer are, as already explained, available for scientific investigation in all Provinces and States in Northern and Central India, as well as on expeditions beyond the north-west frontier. Mr. Duthie, the officer now holding the appointment, was thus in 1888, by his deputation to accompany the Black Mountain Expedition, enabled to acquire information concerning the flora of a country which had not hitherto been botanically explored. During the last three years, Mr. Duthie has also been deputed to Simla in the hot weather to assist in the preparation of the "Dictionary of the Economic Products of India," and during the same period he has been engaged in the botanical exploration of Rajputana and of the Central Provinces.

M. FAYE'S THEORY OF CYCLONES.

IN his admirable work on "The Principles of Science," the late Prof. Jevons thus sums up the characteristic mental attributes of the great scientific discoverer :—

"He must be fertile in theories and hypotheses, and yet full of facts and precise results of experience. He must entertain the feeblest analogies and the merest guesses at truth, and yet he must hold them as worthless till they are verified in experiment. Where there are any grounds of probability, he must hold tenaciously to an old opinion, and yet he must be prepared at any moment to relinquish it when a single clearly contradictory fact is encountered."

In his theory of cyclones, M. Faye has abundantly proved himself to possess those attributes that are defined in the first phrase of each of these sentences, and particularly the final one. Whether, however, in his treatment of this subject, the manifestation of the remaining and qualifying attributes is equally recognizable; whether he has fairly grasped and duly weighed all the established facts that are relevant and even essential to his hypothesis; and whether, among those that he has overlooked, there are not some that are "clearly contradictory" to the requirements of his theory, and therefore fatal to it—these are the questions that I propose to inquire into in the present article.

A true theory of cyclonic storms has not merely a scientific interest, it has also practical bearings of very high importance. When a ship is involved in the outer circle of a tropical cyclone, the vital problem which the seaman has to solve is, how to escape the fearful squalls of the inner vortex and the tremendous cross-seas of the central calm. In order to do this he must be able to judge of the bearing of the storm-centre from the actual position of his ship, and, to determine this point with even approximate accuracy, his sole guide is the direction of the wind. It may well be, then, that the safety of his ship, his own life and those of his fellow-seamen, are involved in the right answering of this question, "Does the storm-centre bear at right angles to the local direction of the wind, or is it from two to four points in advance of this position?" M. Faye's theory assumes and inculcates the former; the latter is consistent only with the hypothesis of an indraught from all sides, and an ascending current over the storm, the existence of which M. Faye persistently denies.

M. Faye's views on the nature of cyclonic storms are too well known to render necessary any detailed description of them. An account given by Mr. Archibald in vol. xxxviii. of this journal (p. 149) is quoted without disapproval by M. Faye in his latest publication in the *Comptes rendus*, and may therefore be accepted as just. Its essential points are that cyclones are generated as great eddies in the higher regions of the atmosphere, and that there is a downrush of air in the vortex. "Dans ces tourbillons, tout semblables à ceux qui se forment dans les cours d'eau, les spires, d'abord très larges, iront en se rétrécissant par en bas, et leur girations progressivement accélérées, en vertu d'une loi bien connue de mécanique, amènent au contact du sol, et y concentrent sous une aire bien plus étroite que celle de leur embouchure les énergies continuellement renouvellées du fleuve aérien jusqu'à ce que son élargissement croissant aboutisse à la décomposition du cyclone."

Further on, with respect to the descending current in the vortex, he remarks: "L'air envoyé en bas sera en petite quantité mais animé d'une vitesse de rotation énorme."

I leave aside for the present any criticism of the physical and mechanical actions which M. Faye conceives to take place in these unfortunately inaccessible vortices of the higher atmosphere, and which I, for one, am unable to reconcile either with the results of direct observation or with well-established physical laws. For the moment I wish to concentrate attention on the question of fact, whether there is an indraught of air to the cyclone vortex at the earth's surface, and therefore necessarily an ascending current over it, or, on the contrary, an outflow from a descending current. This is the crucial point of the controversy, and by the answer M. Faye's theory must stand or fall. Indeed, M. Faye seems to recognize this, since he says:—

"L'argument le plus solide, celui qu'on m'opposait toujours pour prouver que l'air était ascendant dans les cyclones, à savoir le fait que les isobares étaient partout et toujours coupés sous un angle assez notable par les flèches des vents, de manière à accuser une tendance nettement centripète, &c."

He admits, too, that in certain cases there is really an indraught and ascent of air; only, on his view, these are not cyclones.

In order to forestall any objection on this score, I will take as the subject of inquiry the cyclones of the Bay of Bengal, the typical cyclones to which Mr. Piddington first applied the name, however etymologically incorrect. I trust, by this restriction, to escape ignominious dismissal from court on the plea that my witnesses are impostors—merely "prétendus cyclones"—and that their evidence is consequently irrelevant.

My first experience of a great tropical cyclone was the memorable storm that devastated the port and city of Calcutta on October 5, 1864. Up to that time, my acquaintance with cyclones was, like M. Faye's, "academic"; and under the impression that Reid's and Piddington's description of the winds, as blowing in circles or at right angles to the radius vector of the vortex, was an established scientific fact, on the evening of that day I sketched out, for the information of some friends, the probable course of the storm that was then passing away, having swept the port of its shipping, and left half the houses around us more or less wrecks. Having no other guide at the moment than the changing directions of the hurricane as experienced at Calcutta, on the supposition that the centre lay at right angles to these directions, I inferred that the storm had reached us from the northeast corner of the bay, and had followed a north-west or west-north-west course past Calcutta. What was my surprise, then, when accounts began to come in from other places in Bengal, showing that the course of the storm had been almost due north; and when, further, on plotting down the wind directions reported from other stations according to the hours at which they had been observed, I found that, instead of being at right angles to the radius vector, they were strongly inclined inwards; and such as, after making all allowances for their being only estimated directions and perhaps, therefore, a point or two in error, could be reconciled only with a sharp spiral indraught to and up to the central calm. Later on, when I obtained copies of the logs of ships that had been involved in the storm in its passage up the bay, I found that their wind observations, equally, were compatible only with spiral directions. Unlike M. Faye, I had no theory to support, and I submissively accepted the teaching of the evidence which lay so plainly before me.

This evidence is set forth on Plates I. and II. of the Report drawn up by Colonel Gastrell and myself, which was widely distributed at the time to scientific bodies, so that, in all probability, a copy must exist in the library of the Académie des Sciences.

Since then, many other storms in the Bay of Bengal have been carefully investigated, and their full details embodied in Reports drawn up by Messrs. Wilson, Eliot, Pedler, and myself. Without a single exception, the evidence thus accumulated has been to the same effect as that of the cyclone of 1864, and these gentlemen have all arrived at conclusions similar to mine. Thus, Mr. Wilson says[1]:—"The following rule may be used to determine the approximate bearing of the centre with as much accuracy as it seems to be possible to arrive at: *In the northern hemisphere, with the face to the wind, the direction of the centre is from ten to eleven points to the right-hand side*"; and, to quote only one of Mr. Eliot's numerous references to this subject,[2] "The air is drawn into the centre [of a cyclone], but is not drawn directly to it. The particles move by a kind of spiral path to the centre." And he gives a diagram, followed by charts of the Balasore cyclone of May 1886 and the Madras cyclone of November of the same year, as illustrative examples. And Mr. Pedler, in summing up the evidence of the False Point cyclone of September 1885, says[3]:—

"It is therefore clear, from these autographic records, that there was a very strong indraught towards the storm-centre, and that for a considerable portion of the time, even when the storm-centre was comparatively close to Hazaribagh, the winds were part of a well-defined spiral system. In fact, for a large part of the time they subtended an angle of less than 45° with the radius of the storm. . . . The records of five anemographs within the influence of the storm . . . show that the theory of the circular movement of winds in a cyclone, which was advanced by Reid and Piddington, and has been supported by some later writers, is utterly untenable. At considerable distances from the storm-centre the winds approach more to the radial direction of indraught towards the centre, as advocated by Espy, than to any circular movement. As the centre of the storm is approached, the circulation appears to become more defined; but even just outside the storm-centre there is no evidence to show that the direction is tangential."

The reports here quoted and many others, all leading to the same conclusions, have been communicated officially to a large number of scientific bodies in Europe and elsewhere, and taken together they probably furnish the most copious and complete body of existing evidence relative to the cyclones of a tropical sea. Not long since I examined the whole of the charts given in these reports, in order to verify Mr. Wilson's rule (quoted above) for ascertaining the bearing of the storm-centre when the local wind direction is the only datum available, and I found that in the north of the Bay of Bengal, as the mean result of 132 measurements, the angle included between the wind arrow and the radius vector of the vortex was 122° (or 32° greater than a right angle), and that of twelve positions within 50 miles of the storm-centre, that is to say, in the inner circle of the hurricane, 123°. In the south of the bay it was 7° greater. Prof. Loomis, taking into account the land as well as the marine observations, and all barometric depressions, whether storms or otherwise, obtained an angle 25° greater, and differing only by 33° from the radial direction. It is hardly necessary to refer to Prof. Loomis's results of his examination of the Manilla cyclone of October 1882, which gave an angle of 118°, or to Mr. Meldrum's work on the cyclones of the South Indian Ocean, which has already been quoted by Mr. Archibald in his article in NATURE, mentioned above. All testify uniformly and in the strongest manner to the sharp spiral indraught of the winds in tropical cyclones, so that, as Prof. Loomis has truly remarked, "we thus see that tropical storms are spouts and not cyclones, and it is unfortunate that the term cyclone should have been ever adopted." In this view I fully agree, and I make M. Faye a present of the admission, that in an etymological sense, if in no other, Mr. Piddington's typical cyclones are not cyclones at all.

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[1] "Report on the Midnapore and Burdwan Cyclone of October 15 and 16, 1874," p. 86. The italics are as in the original Report.

[2] "Hand-book of Cyclonic Storms in the Bay of Bengal," p. 14 (1890).

[3] "Indian Meteorological Memoirs," vol. iv., Part 2, p. 127. The barometric reading recorded when the centre of this storm was passing False Point Lighthouse is the lowest that has ever been observed at the sea-level.

With all these results of a quarter of a century's experience present to my mind, when a gentleman holding the high position of M. Faye reiterates the assertion that the winds of tropical cyclones blow in circles, and that if ever they are found to blow spirally inwards such instances are not true cyclones (in the ordinarily accepted, *i.e.* denotative, meaning of the term), the impression I receive is somewhat such as M. Faye would probably experience were some equally eminent scientific authority to assert in his presence that the Ptolemaic system truly represents the relative movements of the sun and planets, and that the heliocentric scheme of Copernicus is a "prétendu système." If, indeed, M. Faye prefers to avail himself of the admission made above, to relegate Mr. Piddington's typical cyclones to the category of "prétendus cyclones," and therefore to exclude them from his theory, my present argument falls to the ground; but in that case his cyclone becomes the mere abstract definition of a term, and it remains to be shown that there is anything corresponding to it in Nature. That, however, in his latest communication to the *Comptes rendus*, he intended his assertions to apply to these tropical cyclones is abundantly apparent.

Can it be that M. Faye is unacquainted with the mass of original evidence embodied in the Indian cyclone reports, in Mr. Meldrum's writings on the cyclones of the South Indian Ocean, and with Prof. Loomis's work, in which these and many others are discussed? It would indeed seem so, since in none of his writings have I ever seen any reference to any other Indian author than Mr. Piddington, and even in his case it is difficult to believe that M. Faye has done more than simply accept Mr. Piddington's conclusions, without attempting to verify them by an examination of the original data. But if this be really the case—if he has taken so little pains to ascertain the fundamental facts, and to test the soundness of his speculations by an appeal to the evidence of the last twenty-five years—it is indeed strange that he can put forward confident assertions on a matter with which his acquaintance is so imperfect, and that he can disseminate statements that are demonstrably erroneous, and may be fraught with danger to the lives and property of those who accept him as their guide, backed with the high authority that must necessarily attach to his name.

It is a far from edifying spectacle to see such a man, in his latest communications to the *Comptes rendus*, quoting with complacency any isolated passage in the writings of leading meteorologists which seems to promise some support to his tottering theory, and ignoring all that would tell against it. That such cyclones as originate beyond the tropics are, in the first instance, movements of the higher atmosphere, has been rendered very probable by Dr. Hann's demonstration of the temperature relations of cyclones and anticyclones; but nothing that Dr. Hann has ever written has shown that he is in the least inclined to accept M. Faye's strange hypothesis of a descending current as the leading feature of cyclones and tornadoes. That the clearing of the skies in the central calm of a tropical cyclone may be due to the descent of a certain amount of air, although not decisively proved, is yet not improbable; but what would be thought of a man who, standing on a river bank, and seeing an upward current in the back-water immediately below him, should shut his eyes to the broad stream beyond, and assert, on the strength of his observation, that rivers flow from the sea to the mountains? Yet such, and no other, is the relation of this descending current to the great body of the cyclone. All may admit, with Prof. von Bezold, that there is much in the views hitherto prevalent as to the origin of cyclones and anticyclones that requires modification, and it may yet be long before these phenomena are fully and satisfactorily explained. There are many points of difference between the storms of the tropics and those of the temperate zone which seem to show that the forces that are principally active in the former play but a secondary part in the latter. But certainly there is no apparent tendency on the part of the leading meteorologists of Europe and America to accept M. Faye's *idolon specus* as a true theory of cyclones and tornadoes, nor is it in the least likely that such will ever be witnessed.

HENRY F. BLANFORD.

NOTES.

THE arrangements for the meeting of the British Association are now nearly complete. In a former note we referred among other matters to the excursions. We now learn that among them the organization of the pedestrian excursions to the Black Mountains is so far advanced that the detailed programme is now ready, and can be obtained by application to the Local Secretaries.

THE Royal Archæological Institute of Great Britain and Ireland opened their annual meeting in Edinburgh on Tuesday. At noon there was a reception of the members in the National Portrait Gallery by the President and Council of the Society of Antiquaries of Scotland. The inaugural meeting took place in the lecture-hall of the Royal Geographical Society. Sir Herbert Maxwell, on taking the chair, remarked that the closing years of a century naturally suggested the process of stock-taking, and as they had arrived at the last decade of a century which claimed to have witnessed beyond all precedent the accumulation of scientific knowledge, it was not unnatural that they should direct inquiry into the standing obtained by that particular branch of science in which they were all concerned. After a brief summary he stated that one of the problems which was pressing upon antiquaries at the present time was that relating to those mysterious rock sculptures which from time to time were found in increasing numbers all over Scotland. They bore a striking resemblance to similar rock sculptures found not only in Scandinavia and Central Europe, but in such remote parts of the earth as Asia, and Northern, Central, and Southern America. They could hazard no guess even at the race by whom they were made, still less at the object of their authors. All they could do was to record the discovery of them with careful drawings, and wait till perhaps light would flash upon them from the habit of some uncivilized tribe or from a passage in some hitherto unnoticed writer.

In the evening Dr. John Evans opened the Antiquarian Section with an address on the progress of archæology. The address covers the whole ground from Christy and Lartet's researches on the Dordogne to the Assyrian tablets.

BY an Imperial Decree of June 8, the Gold Medal for Art and Science was bestowed by H.I.M. the Emperor of Austria on Dr. R. Bowdler Sharpe, of the British Museum.

AT the graduation ceremony of the University of Edinburgh, held on the 1st inst., the Cameron prize was presented to Dr. Ferrier, F.R.S., by Prof. Fraser. Prof. Fraser said that Dr. Ferrier's researches had gained for him a well-merited fame throughout the whole civilized world. He had contributed to the alleviation of suffering in some of its most distressing and painful manifestations, and therefore the Senatus had thought that they were fully justified in awarding to him the prize, which had been founded for the recognition of important and valuable contributions to practical therapeutics. He had much pleasure in announcing further that Dr. Ferrier would, early next session, communicate to the University a paper describing some portion of his researches into this important subject. Prof. Ferrier, on appearing upon the platform to receive the prize, was received with most enthusiastic cheers.

AT a meeting of the Academy of Medicine of Paris on the 28th ultimo, Sir Joseph Fayrer, of London, and Dr. Bateman, of Norwich, were elected Associates of the Academy. These gentlemen had both been for some years Corresponding Members of the Academy, but they shared the Membership with only six other members of the profession in this country, viz. Sir James Paget, Bart., Sir Richard Owen, Sir Joseph Hooker, Sir Thomas Longmore, Dr. West, and Sir Spencer Wells, Bart.

DR. THORNE THORNE, F.R.S., has been elected a Corresponding Member of the Royal Italian Society of Hygiene.

PROF. DU BOIS-REYMOND has been elected Dean of the Medical Faculty of the Berlin University for this year. He has already more than once filled this post. Prof. Foerster, the astronomer, has been chosen Rector of the University.

HER MAJESTY'S Commissioners for the Exhibition of 1851 have offered nomination to Science Scholarships for the year 1892 to the following Universities and Colleges. The Scholarships are of the value of £150 a year, and are tenable for two years. The scholars are to devote themselves exclusively to study and research in some branch of science the extension of which is important to the industries of the country:—University of Edinburgh, University of Glasgow, University of Aberdeen; Mason College of Science, Birmingham; University College, Bristol; Durham College of Science, Newcastle; Yorkshire College, Leeds; University College, Liverpool; Owens College, Manchester; University College, Nottingham; Firth College, Sheffield; University College of North Wales, Bangor; Queen's College, Cork; Queen's College, Galway; University of Toronto, University of Adelaide, University of New Zealand.

IT has been decided to perpetuate the memory of the connection of Dr. Leidy with the University of Pennsylvania by raising a fund to endow the Chair of Anatomy and to found a memorial museum. Dr. Leidy was Professor of Anatomy for thirty-nine years, and his devoted services will be suitably recognized by connecting his name with the chair which he so long adorned.

THE arrangements for the World's Fair at Chicago seem to be advancing quickly. Seeing that so much benefit to science may be anticipated from the comparison of the best instruments and methods of working in use in different countries, which such exhibitions render possible, it seems a pity that political questions may render them less representative than might be wished. The *New York Nation* refers to the reluctance of French manufacturers to take part in the World's Fair, due to the bad feeling created by the McKinley Bill, and to the belief entertained that any expense incurred in exhibiting goods would be lost by reason of the commercial restrictions which that measure was intended to create and has created. "Nobody cares to spend his money for mere purposes of show. Unless trade follows as a consequence of the exhibition, the money will be sunk. . . . . It does not advance matters, or help on the Fair, to show that both countries are wedded to a false system. It should serve, however, to open the eyes of people on both sides to the absurdity of inviting each other to show their goods, and then creating barriers to prevent each other from buying and selling. Imagine an American McKinleyite meeting his French brother at a World's Fair in Paris or in Chicago, and exhibiting to the latter a choice lot of provisions put up in Mr. Armour's most approved style, while the latter exhibits a fine assortment of woollens, silks, gloves, &c. If they could look in each other's faces without laughing, they must have a gravity exceeding that of two Roman augurs. Ordinary self-respect ought to teach the commercial classes of both countries to keep away from World's Fairs until they learn the A B C's of trade."

E pur si muove. Technical instruction in the provinces is growing apace, small thanks to our statesmen and legislators, for we owe to an accident the possibility of meeting the most crying needs of the time. We may refer to what is going on in Lancashire as an indication of the general awakenment. The total sum available for technical instruction is £40,391, and, after the sums already guaranteed by the County Council and some special amounts now in question are taken into account, there is a balance of about £29,000 to be dealt with, which the committee of the Council recommend should be apportioned between the urban and rural districts of the administrative county on the dual basis of rateable value and population. The committee recommend that a director of technical instruction be appointed at £500 per annum, with travelling expenses; that £3600 be set apart to provide twenty scholarships not exceeding £60 each for a term not exceeding three years, apportioned as follows—eight for science (tenable at Owens College, Liverpool University College, or other approved public institution), two for art, four for commercial subjects, and six for the science of agriculture, including horticulture; that £1200 be set apart for providing eighty exhibitions of £15, tenable for one year at Owens College and Liverpool University College evening classes, or at some approved technical, commercial, or intermediate school, to be apportioned as follows—thirty-two exhibitions for science, eight for art, sixteen for commercial subjects, and twenty-four for agriculture; that £2000 be set apart for founding travelling scholarships and free studentships of £1 to £10 to assist students in attending technical schools; that the various urban and rural sanitary authorities, through or in conjunction with any district committees that may be appointed, be permitted to nominate candidates for the above, two-thirds of whom shall be children of parents whose incomes do not exceed £300 per annum; that all the scholarships and exhibitions be opened to students of both sexes resident in the county; that a sum not exceeding £1000 be granted for the purpose of aiding University Extension lectures; that a sum not exceeding £500 be granted to carry out the arrangements with the council of the Harris Institute in Preston for the promotion of technical instruction in agriculture; and that a sum not exceeding £1000 be granted for staff and office expenses. The migratory dairy school having been much appreciated, arrangements have been made to start a second school at Ulverston on August 11. A scheme for agricultural instruction is also being arranged (estimated to cost £500 per annum), but the details have not yet been finally settled.

THE managers of the New Gallery announce a "Victorian Exhibition," covering the fifty years of Her Majesty's reign from 1837 to 1887. As in the case of the preceding exhibitions, it will contain pictures and other records of events illustrating the history of the Royal Family and of the nation; and it will contain, above all, a series of portraits of the illustrious men and women who, in so many different ways, have made their mark upon the age. We gather from an article in the *Times* that science, in this of all reigns, is not likely to fall behind. We are promised pictures of Charles Darwin, Faraday, and Sir John Herschel, of Lyell and Murchison, of the two Stephensons, of Fox Talbot, one of the inventors of photography, and of Wheatstone, one of the inventors of the telegraph. The article adds that "it would be easy to quadruple this list, supposing the eminent men of science to have had the time and the vanity to sit for their portraits." We agree.

THE *Pall Mall* returns to the charge on the subject of the imagined unpopularity of the British Museum, and states that although the evening openings have so far been a failure, and a very costly failure, the first installation of the electric light costing over £17,000, the problem is being carefully considered. It is also stated that it is an open secret that for some years past the Trustees have been unanimous in favour of Sunday opening, which, as they have more than once pointed out, would entail little or no extra work on the officials, but merely change of work for a few policemen. Among the things that are wanted are certainly continuity in the hours during which the Museum is open on any one day, and the possibility of obtaining some decent refreshment. If in these matters the Trustees will imitate he arrangements at the South Kensington Museum, we believe the attendance will be increased—the attendance of workers certainly will.

WE are requested to state that the designs submitted in competition for the completion of the buildings of the South Kensington Museum are now on view at that Museum from 10 till 6.

DURING the whole month of July little variation in the state of Vesuvius was observable; the lava flowed steadily on, and had at one time extended down the Fossa della Vetrana, nearly opposite the lodge and gate of Messrs. Cook's private road to the Vesuvian railway, but immediately cooled, and again started flowing much nearer its source. At the summit of Vesuvius the vapour appeared to issue almost as in the normal state of the mountain, except for momentary interruptions and occasional ejection of dust and sand. Dr. Johnston-Lavis, who has recently visited the scene, sends us the following details:—"On July 30, I again visited the top of the great cone. The central crater has considerably enlarged, and has now an elliptical plan, with the major axis directed north-west to south-east, but this form has been derived from its original circular shape by the greater destruction of the lips towards the south-east. The edges were in a most unstable state, and attempts at photographing the interior were accompanied by considerable danger, and required many precautions. On the inner walls I was, however, able to make out several dykes besides the hollow one that has supplied the great eastern rift for its several eruptions from 1881–82 to 1890. These may be enumerated as directed north-east; north-north-west, probably the dyke formed at the commencement of this eruption; north-west; south-west, probably the cooled upper extremity of the lava sheet filling the south-west fissure which I have so often mentioned; and lastly, the hollow dyke to the south-south-east, which supplied the lava of May 1885, is again exposed. There may be other dykes, but the large amount of vapour filling the crater, and the danger and impossibility of approaching the edges in most parts, prevent a very detailed examination. So far as I could make out, the situation of the vent is quite to the south-east of the crater bottom, so that this fact, combined with the prolongation of the crater in that direction and the existence of numerous radial fissures, would indicate that the general tendency is for the next lateral disruption to take place towards Pompeii, or Torre Anunziata. On July 30 the lava was flowing very slowly just at the junction of the Atrio del Cavallo and the Fossa Vetrana. To an experienced observer the whole state of the mountain is still very unstable, and a fresh outburst might occur at any moment, although the volcano may gradually quiet down. But a few days before my visit, four strong earthquakes were felt at the lower railway station, showing that important fracturing, injection, or other dynamic disturbances were taking place in the great cone."

WE have received from Mr. C. Mostyn an interesting letter on the well-known appearance of the green ray at sunrise or sunset caused by the refraction of the air. He states:—"This 'green ray' is seen to best advantage at sun-*rise*, owing I imagine to the eye not being wearied with watching the previous glare, as is apt to be the case at sunset. At the same time, I had many very satisfactory observations at sunset, one in particular, when we were running before a very heavy sea in the Southern Ocean, and the 'green ray' was seen no less than three times in as many seconds, as the ship rose and fell on the huge waves causing as it were two sunsets, with a sunrise between them. The best displays took place when the refraction near the horizon was of such a character that the sun assumed a balloon, or vase, shape as he came close to the sea-line. When, on the contrary, the sun appeared flattened out in its horizontal diameter, the 'green ray' was either entirely absent, or was seen only in an indistinct and uncertain manner."

SIR EDWARD WATKIN having now, we presume, cured unpunctuality on the many lines of railway which he is highly paid to manage, is again turning his attention to Snowdon. It will be remembered that he proposed in the first instance to erect an astronomical observatory there. This, of course, was ridiculous. We are now told that the authorities of the Trinity House have expressed warm approval of his more recent proposal to place an electric light on the summit. The Elder Brethren consider that the light should prove an invaluable addition to those already erected round the North Wales coast for the guidance of mariners. Sir Edward hopes to have the light burning before Christmas.

THE Directors of the Crystal Palace, in deference to the wish of the Electrical Trade Section of the London Chamber of Commerce, have decided to postpone the opening of the Electrical Exhibition from November 1891 till January 1, 1892, on which date the Exhibition will be formally opened.

WE learn from the *Photographic News* that the great progress that has been made in the methods by which rapid movements can be analyzed is well seen in a series of photographs lately taken by Anschütz, of Lissa, who has already given to the world some of the best instantaneous pictures ever taken. The subject of the pictures at present under consideration is a dog jumping over a small bush. In the act of making one jump the animal has been photographed twenty-four separate times, and each picture is not a mere silhouette, as was the case with Muybridge's first attempts of this kind, but a little picture showing half-tone and detail. Some of the attitudes are, of course, comic in appearance, for they represent phases of a movement which the eye is unaccustomed to, and cannot possibly appreciate. Notably is this the case in the commencement of the jump, when the dog's hind toes only touch the ground; and again at the finish of the jump, when his legs are gathered together in a heap.

A GERMAN specialist, Dr. Cold, has recently pleaded for giving young people more sleep. A healthy infant sleeps most of the time during the first weeks; and, in the early years, people are disposed to let children sleep as much as they will. But from six or seven, when school begins, there is a complete change. At the age of ten or eleven, the child sleeps only eight or nine hours, when he needs at least ten or eleven, and as he grows older the time of rest is shortened. Dr. Cold believes that, up to twenty, a youth needs nine hours' sleep, and an adult should have eight or nine. With insufficient sleep, the nervous system, and brain especially, not resting enough, and ceasing to work normally, we find exhaustion, excitability, and intellectual disorders gradually taking the place of love of work, general well-being, and the spirit of initiative.

THE *Entomologist's Monthly Magazine*, among much interesting matter, refers to the possibility of the destruction of some of the inclosures in the New Forest which have proved themselves to be among the happiest hunting-grounds of the entomologist.

A RECENT number of the Proceedings of the Academy of Natural Sciences of Philadelphia contains a paper on Echinoderms and Arthropods from Japan, by Mr. J. E. Ives. The specimens described were collected by Mr. Frederick Stearns, of Detroit. The new species of Echinoderms and Crustacea are enumerated. A new Ophurian, a new crab, and a new Pycnogonoid are described, and several species of star-fishes hitherto unfigured are illustrated. The plates are admirable.

BULLETIN No. 10 of the University College of Agriculture at Tokyo contains an account of some manuring experiments with paddy rice (second year) by Dr. O. Kellner, Y. Kozai, Y. Mori, and M. Nagaoka. The principal purpose of the researches carried out in 1889, and reported in Bulletin No. 8, was to ascertain how much nitrogen, phosphoric acid, and potash can be consumed by rice from the stock of nutrients in the unmanured soil, and how much of them is needed in the manure for the production of a maximum crop if the three nutrients are applied in the most assimilable form. On the basis of the results then obtained, the present experiments were tried with the object of getting information on the following questions:—(1) How much nitrogen, phosphoric acid, and potash is taken up from those plots which had not received the respective nutrients in the preceding year? (2) What is the effect of unrecovered phosphatic manure on the succeeding crop? (3) How much nitrogen can be supplied to rice by the preceding cultivation of a leguminous plant (*Astragalus lotoides*, Lam.) for green manuring? (4) What is the effect of various phosphatic fertilizers on rice? (5) What is the effect of various nitrogenous manures on rice? The work seems to have been carefully done, and affords a good instance of the way in which scientific questions are now being treated in Japan.

THE July number of the Proceedings of the Society for Psychical Research has reached us, and contains the following contributions:—"On Alleged Movements of Objects, without Contact, occurring not in the Presence of a Paid Medium," by Mr. F. W. H. Myers; "Experiments in Clairvoyance," by Dr. A. Backman; and "A Case of Double Consciousness," by Mr. R. Hodgson.

AT the Bournemouth meeting of the British Medical Association, a discussion on the subject of alcohol was initiated by a paper by Dr. Samuel Wilks. In the course of his paper he stated that he had no acquaintance with any organic changes attributable to alcohol in the lungs and kidneys, but it seemed that the digestive and nervous systems suffered. Physiologists had failed to demonstrate the chemical changes which it underwent in the body, and consequently it was impossible to say whether it was of the nature of a food or not. No one had yet seen a person who lived on alcohol, although there was evidence of persons taking large quantities of alcohol who yet preserved their weight with a minimum of food; and that supported the theory that, although alcohol was not nutritive in itself, it prevented the wear and tear of the body. The opposite theory also existed, that alcohol acted as a spur to the nervous system and quickly wore it out. He could not disapprove of the use of wine and beer, if taken in moderation, by the masses of the people; but as to spirits or spirits and water, he had not made up his mind that they were in any way useful, and he seldom recommended them. Dr. Bucknill thought that the wise use of wine might cure some cases and be useful in others. Dr. Norman Kerr said that alcohol was a poison, analogous in many respects to other poisons. Sir Risdon Bennett agreed with Dr. Wilk in not approving of spirits as a beverage. He believed it to be useful in fever and in some nervous diseases, but he did not think it desirable at the present time to lay down any broad principles with regard to alcohol with reference to the whole community.

THE Philadelphia *Satellite* states that, during the abortive attempt to cut a canal through the isthmus of Panama, as much as 200,000 ounces of quinine were used annually in combating malarial fever.

ACCORDING to the *Pharmaceutical Journal of Australia*, the practice has been introduced into Victoria, on the recommendation of Baron von Mueller, of placing green branches of eucalyptus in sick rooms as a disinfectant. Dr. Curgenven states, after twelve months' trial, that in cases of scarlet fever, if the branches be placed under the bed, the bedding undergoes thorough disinfection, the volatile vapour penetrating and saturating the mattress and every other article in the room. Its vapour is also said to have a beneficial effect upon phthisical patients, acting not only as an antiseptic, but as a sedative and to some extent as a hypnotic.

THE Bulletin of the (American) Essex Institute just received contains an account of the annual meeting held last May, and a retrospect of the year, from which we learn that Mr. Perley, in a lecture on "Old-time Winters in Essex County," gave interesting particulars on many subjects, including weather. We give the following extract:—"The lecturer spoke of the watch, church services, dress, food, and schools of the early winter seasons; how the people spent their evenings, the winter employment of the people in cutting off the forests, sledding timber and wood, making pipe staves and barrel hoops, and, most interesting of all, the institution of the old-fashioned shoemakers' shops, of which nearly every farm had one a century ago. Women in those days engaged in spinning and weaving. The holidays were referred to—Thanksgiving, Christmas, and New Year's; and the winter pleasures, such as sleigh-rides, dancing, spinning and quilting parties, and games, shuffle-board, coasting, skating, trapping, gunning, fishing, singing-schools, and girls' samplers. He also spoke of the old modes of travel, snow-shoes, &c. Nearly all the heavy teaming was done on sleds, and he mentioned the winter of 1768-69, when the travelling was so bad that the farmers in the western part of the State could not get their grain and provisions to the coast to market. Snow remained on the roads as it fell until about a century ago. Mr. Perley then spoke of particular winters: that of 1641-42, when the Indians said they had not seen the ocean so much frozen for forty years; of 1646-47, when there was no snow to lay; of 1696-97, said to be the coldest winter since the first settlement of New England; of 1701-2, which was 'turned into summer'; of 1717-18, when the snow was from ten to fifteen feet deep and the drifts twenty-five feet, many one-story houses being buried; of 1740-41, said to be the severest winter known by the settlers, Salem Harbour being frozen over as early as October; of 1774-75, a wonderfully mild winter; of 1779-80, when for forty days, including March, there was no perceptible thaw, and the snow was so hard and deep that loaded teams passed over the fences in any direction, arches being dug under the snow so that men on horseback could ride under them, and which was long remembered as *the* hard winter; of 1784-85, when, as late as April 15, snow was 2 feet deep, and frozen hard enough to bear cattle; of 1785-86, when in the remarkable storm of November 25, the snow blew into balls, one of which had rolled 76 feet, measuring 17½ by 22 inches; of 1794-95, when the *Betsey* was launched in Salem on Christmas Day, the thermometer indicating 80° above zero at noon, and men and boys went in swimming; of 1801-2, when the *Ulysses*, *Brutus*, and *Volutia*, three Salem vessels, which sailed out of the harbour on a summer-like morning in February, were all cast away at night on Cape Cod, in a terrible snow-storm, which continued a week. He also referred to more recent seasons, and of the cold winter of 1856-57, when in one week in January was the coldest day by the thermometer ever recorded of late years, mercury in Salem 20° below zero; travel on the railroad between Boston and Salem entirely suspended from Tuesday morning to Thursday afternoon. The recent mild winters were also alluded to."

IN the volume of Bavarian meteorological observations for 1890, Dr. C. Lang (the Director of the Service) contributes an article on the "Secular Variations of Damage by Lightning and Hail." He points out that in almost all recent investigations the conclusions come to are that during the last 50 years damage by lightning has much increased, but this is not borne out by his inquiry, but is probably owing to more attention having been paid to the subject recently. The numerous impurities introduced into the air of towns from fire-places, &c., would make it probable that they would be more liable to damage than country places, but exactly the opposite is the case, the ratio of damage to buildings in towns to that in the country being 1 : 2. This result is possibly to some extent due to the more numerous lightning-conductors, and to railway lines in the towns. He finds that the damage from hail shows a very probable connection with the period of sun-spot frequency, but the secular range of the former points more particularly to the influence of temperature, so that the curve of hail-frequency shows, not only a minimum occurring with the 11-year sun-spot maximum, but also a period of about 35 years. The damage from lightning, on the other hand, does not show any connection with a secular range of temperature, but the minimum occurs with the maximum of sun-spot frequency. In other words, damage from hail seems to be more decidedly connected with terrestrial, and damage from lightning more with cosmical influence.

THE application of science in the direction of domestic comfort seems to be advancing with great strides in the United States. The *Nation*, in reference to the announcements that the inhabitants of Kansas City are about to be supplied with cool air in summer and warm air in winter through a system of pipes laid in the streets; and that the people of Framingham, Mass., are to be furnished with gas for heating purposes at the price of 50 cents a thousand feet, thus writes:—"Thus the ends of the land are advancing in the art of living while the metropolis remains stationary, and is kept from falling behind only by incessant grumbling. And yet the possibilities of comfort, of health, and even of cheapness revealed in these schemes are wonderfully alluring, and their realization would be prevented by no physical obstacles. If we consider that wonderful work of human hands, the kitchen range, under the management of the regular cook, who knows how to put on all the draught at once and keep it on, what a devourer of fuel it is! We need a cup of tea or a chop in summer, and a fire is kindled that would generate steam enough to drive an ocean racer a mile upon her course, the kitchen is turned into a Tophet, the miserable servants swelter in the apartments which their own stupidity and that of mankind have rendered uninhabitable, and their employers are rendered uncomfortable above. The extravagance of the Chinese, who, as related by Charles Lamb, at first thought it necessary to burn down a house whenever they wanted to roast a pig, is nothing to ours." Has anybody ever calculated the annual waste caused by the above described "use" of the ordinary "kitchen range"?

AN interesting paper upon the slow combustion of explosive gas mixtures is contributed to the current number of *Liebig's Annalen* by Dr. Krause and Prof. Victor Meyer. The experiments described were made with electrolytic mixtures of hydrogen and oxygen, and detonating mixtures of carbon monoxide and oxygen. The first experiment consisted in heating in a bath of vapour of diphenylamine (305°) a detonating mixture of hydrogen and oxygen contained in a U-shaped tube closed by mercury. The heating was continued without intermission for a fortnight, at the end of which time very little gas remained, almost the whole having slowly combined to form water. The experiment was then repeated in an apparatus constructed entirely of glass, and in which the use of mercury was avoided, except in a small manometer used to indicate the pressure. It was then found that no trace of water was formed at the temperature of diphenylamine vapour (305° C.); at the temperature of boiling sulphur (448°) the amount of combination was exceedingly small; while at 518°, the boiling-point of phosphorus pentasulphide, a considerable amount of combination occurred, but no quantitative rule could be deduced. In all these experiments the gases employed were moist, and no particular care had been taken to remove the last traces of admixed air. Now Bunsen and Roscoe, in their celebrated work on detonating mixtures of hydrogen and chlorine, showed that regular results were only obtained when the film of air condensed upon the surfaces of the glass vessels employed was removed by allowing the gas to stream through the apparatus for several days previous to the experiment. A fresh series of experiments were therefore made, in which these precautions were most rigidly observed; most complicated pieces of apparatus were constructed of glass throughout, which admitted of the drying of the gases prepared (in case of hydrogen and oxygen) by the electrolysis of hot water, so as to exclude ozone and hydrogen peroxide; and the pure gases thus obtained were allowed to stream through the series of bulbs united by capillary tubes for a fortnight, night and day, before the bulbs were sealed off at the capillaries. It was found that, with pure dry gases, scarcely a trace of combination occurred by the fusion of the very fine capillaries. As regards the temperature of ignition of electrolytic hydrogen and oxygen, or detonating carbon monoxide and oxygen, it was found that bulbs containing them do not explode when placed in boiling pentasulphide of phosphorus (518°), but do explode in vapour of stannous chloride (606°). The temperature of ignition lies, therefore, between 518° and 606° C. The mode of explosion differs considerably under different circumstances. In case of explosion in vapour of stannous chloride, the bulb was never shattered, but a sudden appearance of flame within the bulb occurred, accompanied by a slight detonation, and in some cases the point of the capillary was blown off. It is also astonishing how long one requires to hold such a bulb in a Bunsen flame before explosion occurs; it never occurs until the flame becomes coloured yellow, and the glass begins to soften, and frequently only causes a swelling out of the glass at the heated spot. Thin-walled bulbs, however, are sometimes shattered. In two cases it was noticed that the glass at the softened part was violently forced in, owing to the previous heating having caused a large percentage of combination, and hence the production of a partial vacuum. Even after taking the rigid precautions to insure purity above described, no definite quantitative rule connecting the time and percentage of combination has been discovered, experiments performed simultaneously upon similarly treated mixtures yielding widely different results; showing that the irregularities of glass surfaces, even after removal of their air-films, are quite sufficient to modify very sensibly the conditions under which combination occurs.

THE additions to the Zoological Society's Gardens during the past week include an Egyptian Gazelle (*Gazella dorcas*) from North Africa, presented by Mr. S. C. Saunders; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Mr. Edward J. Brown; two Herring Gulls (*Larus argentatus*), British, presented by Mr. T. A. Cotton; two White-bellied Sea Eagles (*Haliaetus leucogaster*) from Australia, presented by Mr. Hugh Nevill, F.Z.S.; a Lesser Sulphur-crested Cockatoo (*Cacatua sulphurea*) from Moluccas, presented by Miss Partridge; three Barbary Turtle Doves (*Turtur risorius*) from North Africa, presented by Miss D. Bason; an Indian Cobra (*Naia tripudians*) from India, presented by Mr. H. E. Lindsay; two Harnessed Antelopes (*Tragelaphus scriptus* ♂ ♀) from Gambia, a Paradoxure (*Paradoxurus aureus*) from Ceylon, two Grey Ichneumon (*Herpestes griseus*) from India, four grey Parrots (*Psittacus erithacus*) from West Africa, deposited.

OUR ASTRONOMICAL COLUMN.

THE SPECTRUM OF β LYRÆ.—A study of twenty-nine photographs of the spectrum of β Lyræ has led to some interesting results, noted by Prof. E. C. Pickering in *Astronomische Nachrichten*, No. 3051. The spectrum of this star contains, in addition to the absorption lines, several bright lines, the most conspicuous of which are about λλ 486, 443, 434, 410, 403, and 389, to use a three-figure reference. The lines near λ 443 and λ 403, are two of the most prominent lines in the spectra of the Orion stars, and the remaining four coincide with the hydrogen lines F, G, *h*, and α. From the investigation it appears that these bright lines change their positions, so that sometimes they have a greater wave-length than the corresponding dark lines, whilst at other times the reverse is the case. In some of the photographs several bright lines are double, and the dark lines are also not free from changes. This naturally led to the inquiry as to whether the changes were connected with the variations of the star's brightness. Starting from a minimum of brightness there is a maximum at 3d. 5h., a secondary minimum at 6d. 11h., another maximum at 9d. 16h., and then the principal minimum is again reached after a total period of 12d. 22h. The point of interest is that the fourteen plates in which the wave-length of the bright lines was increased were taken during the first half of this period of variation—that is, before the secondary minimum; whilst on the eleven plates taken during the second half of the period the displacement was towards the blue end of the spectrum. And since the photographs extend over more than four years, there can be little doubt that the displacements are intimately connected with the variations of the star's brightness. One of the explanations suggested by Prof. Pickering to account for the observed phenomena is that the bright lines are emitted by an object revolving in a circular orbit round the principal star, with a maximum velocity of about 300 miles per second, and completing its circuit in a period of 12d. 22h. The corresponding periastron distance is about 50,000,000 miles. If this be so, β Lyræ is a binary of the β Aurigæ type, but differing from it in the fact that the component stars have unlike spectra. The phenomena could also be produced by a meteor stream, or by an object like the sun, rotating in 12d. 22h., and having a large protuberance on it extending over more than 180° of longitude. The study of the additional photographs which are being taken will doubtless elucidate the matter.

THE POLARIZATION THEORY OF THE SOLAR CORONA.—In the Publications of the Astronomical Society of the Pacific, vol. iii. No. 16, 1891, Prof. Frank H. Bigelow gives some further results of his investigations of coronal forms, and arrives at some new results. It can be shown that in the case of repulsion of matter in a spherical rotating body like the sun, two poles of repulsion are formed, and the body is polarized about an axis. Within the body the lines of force are parallel to the axis of polarization, and their curvature outside the surface may be calculated. Applying these considerations to the similar coronal forms exhibited in the eclipse photographs of July 1878 and January and December 1889, Prof. Bigelow finds that the axis of polarization is at the surface of the sun about 4½° from the axis of rotation, and taking the radius of the sun as 866,500 miles, the length of the axis to which the lines of force are parallel is 1,729,700 miles. Its direction is fixed, and in 1878 the north and south coronal poles had the positions, north pole = 201°·2, south pole = 301°·6, when referred to the ascending-node of the sun's equator on the plane of the ecliptic. If 138 + 349°·85, 151 + 311°·40, and 12 + 312°·55 be taken as the number of revolutions and the angular excess during the three intervals between the dates of the above eclipses, the mean daily motion in longitude at the latitude of the coronal pole, 85°·5, is found to be 13°·13307. From this the following periods of the sun's rotation in latitude 85°·5 is deduced—

Sidereal period 27·41171d. = 27d. 9h. 52m. 52s.
Synodic period 29·63580d. = 29d. 15h. 15m. 33s.

The formula proposed to express the rotation-period in different solar latitudes is $X = 862' - 76' \sin l$, where X is the mean daily motion in minutes, and l the latitude. With these elements it is possible to predict the positions of the coronal poles at any epoch, and in consequence the relative form of the corona at the time, as seen from the earth. A comparison of the calculated results and photographs, obtained during some recent eclipses, displays a striking concordance. The investigation "also serves to strengthen the conviction that the sun-spots are probably formed by the descent of material from the extremities of the coronal streamers, in a vertical direction upon the sun."

OBSERVATIONS OF THE MOTION OF SIRIUS.—At the Berlin Academy of Sciences on June 4, Prof. Vogel communicated some observations of the motion of Sirius in the line of sight. Using the iron spectrum as the term of comparison with the spectrum of the star, it was found that the velocity of approach on March 22 was 1·96 geographical miles per second with respect to the sun. With hydrogen comparison lines the velocity found was 1·73 miles per second.

RETURN OF ENCKE'S COMET.—A telegram from the Lick Observatory to Prof. Krüger, announces that Encke's periodic comet has been observed on its return by Mr. Barnard on August 1·9958 G.M.T., in the position R.A. 3h. 55m. 20·6s., Decl. 29° 59′·1 N.

ON SOME TEST CASES FOR THE MAXWELL-BOLTZMANN DOCTRINE REGARDING DISTRIBUTION OF ENERGY.[1]

(1) MAXWELL, in his article (*Phil. Mag.*, 1860) "On the Collision of Elastic Spheres," enunciates a very remarkable theorem, of primary importance in the kinetic theory of gases, to the effect that, in an assemblage of large numbers of mutually-colliding spheres of two or of several different magnitudes, the mean kinetic energy is the same for equal numbers of the spheres irrespectively of their masses and diameters; or, in other words, the time-averages of the squares of the velocities of individual spheres are inversely as their masses. The mathematical investigation given as a proof of this theorem in that first article on the subject is quite unsatisfactory; but the mere enunciation of it, even if without proof, was a very valuable contribution to science. In a subsequent paper ("Dynamical Theory of Gases," Phil. Trans. for May 1866) Maxwell finds in his equation (34) ("Collected Works," p. 47), as a result of a thorough mathematical investigation, the same theorem extended to include collisions between Boscovich points with mutual forces according to any law of distance, provided only that not more than two points are in collision (that is to say, within the distances of their mutual influence) simultaneously. Tait confirms Maxwell's original theorem for colliding spheres of different

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[1] Paper read at the Royal Society by Sir William Thomson, D.C.L., P.R.S., on June 11, 1891.

magnitudes in an interesting and important examination of the subject in §§ 19, 20, 21 of his paper "On the Foundations of the Kinetic Theory of Gases" (Trans. R.S.E. for May 1866).

(2) Boltzmann, in his "Studien über das Gleichgewicht der lebendigen Kraft zwischen bewegten materiellen Punkten" (*Sitzb. K. Akad. Wien*, October 8, 1868), enunciated a large extension of this theorem, and Maxwell a still wider generalization in his paper "On Boltzmann's Theorem on the Average Distribution of Energy in a System of Material Points" (Cambridge Phil. Soc. Trans., May 6, 1878, republished in vol. ii. of Maxwell's "Scientific Papers," pp. 713–41), to the following effect (p. 716):—

"In the ultimate state of the system, the average kinetic energy of two given portions of the system must be in the ratio of the number of degrees of freedom of those portions."

Much disbelief and doubt has been felt as to the complete truth, or the extent of cases for which there is truth, of this proposition.

(3) For a test case, differing as little as possible from Maxwell's original case of solid elastic spheres, consider a hollow spherical shell and a solid sphere—globule we shall call it for brevity—within the shell. I must first digress to remark that what has hitherto by Maxwell and Clausius and others before and after them been called for brevity an "elastic sphere," is not an elastic solid, capable of rotation and of elastic deformation; and therefore capable of an infinite number of modes of steady vibration, into which, of finer and finer degrees of nodal subdivision and shorter and shorter periods, all translational energy would, if the Boltzmann-Maxwell generalized proposition were true, be ultimately transformed by collisions. The "smooth elastic spheres" are really Boscovich point-atoms, with their translational inertia, and with, for law of force, zero force at every distance between two points exceeding the sum of the radii of the two balls, and infinite repulsion at exactly this distance. We may use Boscovich similarly for the hollow shell with globule in its interior, and so do away with all question as to vibrations due to elasticity of material, whether of the shell or of the globule. Let us simply suppose the mutual action between the shell and the globule to be nothing except at an instant of collision, and then to be such that their relative component velocity along the radius through the point of contact is reversed by the collision, while the motion of their centre of inertia remains unchanged.

(4) For brevity, we shall call the shell and interior globule of § 3, a double molecule, or sometimes, for more brevity, a doublet. The "smooth elastic sphere" of § 3 will be called simply an atom, or a single atom; and the radius or diameter or surface of the atom will mean the radius or diameter or surface of the corresponding sphere. (This explanation is necessary to avoid an ambiguity which might occur with reference to the common expression "sphere of action" of a Boscovich atom.)

(5) Consider now a vast number of atoms and doublets, inclosed in a perfectly rigid fixed surface, having the property of reversing the normal component velocity of approach of any atom or shell or doublet at the instant of contact of surfaces, while leaving unchanged the absolute velocity of the centre of inertia of the two. Let any velocity or velocities in any direction or directions be given to any one or more of the atoms or of the shells or globules constituting the doublets. According to the Boltzmann-Maxwell doctrine, the motion will become distributed through the system, so that ultimately the time-average kinetic energy of each atom, each shell, and each globule shall be equal; and therefore that of each doublet double that of each atom. This is certainly a very marvellous conclusion; but I see no reason to doubt it on that account. After all, it is not obviously more marvellous than the seemingly well-proved conclusion that in a mixed assemblage of colliding single atoms, some of which have a million million times the mass of others, the smaller masses will ultimately average a million times the velocity of the larger. But it is not included in Maxwell's proof for single atoms of different masses [(34) of his "Dynamical Theory of Gases" referred to above]; and the condition that the globules inclosed in the shells are prevented by the shells from collisions with one another violates Tait's condition [(C) of § 18 of "Foundations of K.T. Gases"], "that there is perfectly free access for collision between each pair of particles whether of the same or of different systems." An independent investigation of such a simple and definite case as that of the atoms and doublets defined in §§ 3–5 is desirable as a test, or would be interesting as an illustration were test not needed, for the exceedingly wide generalization set forth in the Boltzmann-Maxwell doctrine.

(6) Next, instead of only a single globule within the shell of § 4, let there be a vast number. To fix ideas let the mass of the shell be equal to a hundred times the sum of the masses of the globules, and let the number of the globules be a hundred million million. Let two such shells be connected by a push-and-pull massless spring. Let all be given at rest, with the spring stretched to any extent; and then left free. According to the Boltzmann-Maxwell doctrine, the motion produced initially by the spring will become distributed through the system, so that ultimately the sum of the kinetic energies of the globules within each shell will be a hundred million million times the average kinetic energy of the shell. The average velocity[1] of the shell will ultimately be a hundred-millionth of the average velocity of the globules. A corresponding proposition in the kinetic theory of gases is that, if two rigid shells, each weighing 1 gram, and containing a centigram of monatomic gas, be attached to the two prongs of a massless perfectly elastic tuning-fork, and set to vibrate, the gas will become heated in virtue of its viscous resistance to the vibration excited in it by the vibration of the shell, until nearly all the initial energy of the tuning-fork is thus spent.

(7) Going back to the double molecules of § 5, suppose the internal globule to be so connected by massless springs with the shell that the globule is urged towards the centre of the shell with a force simply proportional to the distance between the centres of the two. This arrangement, which I gave in my Baltimore Lectures, in 1884, as an illustration for vibratory molecules embedded in ether, would be equivalent to two masses connected by a massless spring, if we had only motions in one line to consider; but it has the advantage of being perfectly isotropic, and giving for all motions parallel to any fixed line exactly the same result as if there were no motion perpendicular to it. When a pair of masses connected by a spring strikes a fixed obstacle or a movable body, with the line of their centres not exactly perpendicular to the tangent plane of contact, it is caused to rotate. No such complication affects our isotropic doublet. An assemblage of such doublets being given moving about within a rigid inclosing surface, will the ultimate statistics be, for each doublet,[2] equal average kinetic energies of motion of centre of inertia, and of relative motion of the two constituents?

(8) If we try to answer this question synthetically, we find a complex and troublesome problem in the details of all but the very simplest case of collision which can occur, which is direct collision between two not previously vibrating doublets, or any collision of one not previously vibrating doublet against a fixed plane. In this case, if the masses of globule and shell are equal, a complete collision consists of two impacts at an interval of time equal to half the period of free vibration of the doublet, and after the second impact there is separation without vibration, just as if we had had single spheres instead of the doublets.

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[1] The "average velocity of a particle," irrespectively of direction, is (in the kinetic theory of gases) a convenient expression for the square root of the time-average of the square of its velocity.

[2] This implies equal average kinetic energies of the two constituents; and, conversely, equal average kinetic energies of the two constituents, except in the case of their masses being equal, implies the equality stated in the text. Let u, u' be absolute component velocities of two masses, m, m', perpendicular to a fixed plane; U the corresponding component velocity of their centre of inertia; and r that of their mutual relative motion. We have

$$u = \mathrm{U} - \frac{m'r}{m+m'}, \qquad u' = \mathrm{U} + \frac{mr}{m+m'}; \quad \ldots\ldots (1)$$

whence

$$mu^2 - m'u'^2 = (m - m')\left[\mathrm{U}^2 - \frac{mm'r^2}{(m+m')^2}\right] - \frac{4mm'}{m+m'}\mathrm{U}r. \quad . \ . (2)$$

Now suppose the time-average of Ur to be zero. In every case in which this is so, we have, by (2),

$$\text{Time-av.}\{mu^2 - m'u'^2\} = (m - m') \times \text{Time-av.}\left\{\mathrm{U}^2 - \frac{mm'r^2}{(m+m')^2}\right\}. \ (3)$$

Hence in any case in which

$$\text{Time-av. } mu^2 = \text{Time-av. } m'u'^2 \quad \ldots\ldots\ldots (4)$$

we have

$$(m - m') \times \text{Time-av.}\left\{\mathrm{U}^2 - \frac{mm'r^2}{(m+m')^2}\right\} = 0, \quad \ldots\ldots (5)$$

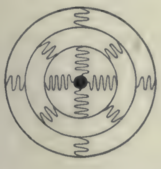
and therefore, except when $m = m'$, we must have

$$\text{Time-av. } (m+m')\mathrm{U}^2 = \text{Time-av. } \frac{mm'r^2}{m+m'} \quad \ldots\ldots (6)$$

which proves the proposition, because, as we readily see from (1), $\frac{1}{2}mm'r^2/(m+m')$ is, in every case, the kinetic energy of the relative motions, $u - \mathrm{U}$, and $\mathrm{U} - u'$.

But in oblique collision between two not previously vibrating doublets, even if the masses of shell and globule are equal, we have a somewhat troublesome problem to find the interval between the two impacts, *when there are two*, and to find the final resulting vibration. When the component relative motion parallel to the tangent plane of the first impact exceeds a certain value depending on the radius of the outer surface of the shell, the period of free vibration of the doublets, and the relative velocity of approach; there is no second impact, and the doublets separate with no relative velocity perpendicular to the tangent plane, but each with the energy of that component of its previous motion converted into vibrational energy. When the mass of the shell is much smaller than the mass of the interior globule, almost every collision will consist of a large number of impacts. It seems exceedingly difficult to find how to calculate true statistics of these chattering collisions, and arrive at sound conclusions as to the ultimate distribution of energy in any of the very simplest cases other than Maxwell's original case of 1860; but, if the Boltzmann-Maxwell generalized doctrine is true, we ought to be able to see its truth as essential, with special clearness in the simplest cases, even without going through the full problem presented by the details. I can find nothing in Maxwell's latest article on the subject (Camb. Phil. Trans., May 6, 1878), or in any of his previous papers, proving an affirmative answer to the question of § 7.

(9) Going back to § 6, let the globules be initially distributed as nearly as may be homogeneously through the hollow; let each globule be connected with neighbours by massless springs; and let all the globules which are near the inner surface of the shell be connected with it also by massless springs. Or let any number of smaller shells be inclosed within our outer shell, and connected by massless springs, as represented by the accompanying diagram, taken from a reprint of my Baltimore Lectures now in progress. Let two such outer shells, given at rest with their systems of globules in equilibrium within them, be connected by massless springs, and be started in motion, as were the shells of § 6. There will not now be the great loss of energy from the vibration of the shells which there was in § 6. On the contrary, the ultimate average kinetic energy of the whole two hundred million million globules will be certainly small in comparison with the ultimate average kinetic energy of the single shell. It may be because each globule of § 6 is free to wander that the energy is lost from the shell in that case, and distributed among them. There is nothing vague in their motion allowing them to take more and more energy, now when they are connected by the massless springs. If we suppose the motions infinitesimal, or if, whatever their ranges may be, all forces are in simple proportion to displacements, the elementary dynamical theorem of *fundamental modes* shows how to find determinately each of the 600 million million and six simple harmonic vibrations, of which the motion resulting from the prescribed initial circumstances is constituted. It tells us that the sum of the potential and kinetic energies of each mode remains always of constant value, and that the time-average of the changing kinetic energy during its period is half of this constant value. Without fully solving the problem for the 600 million million and six co-ordinates, it is easy to see that the gravest fundamental mode of the motion actually produced in the prescribed circumstances differs but little in period and energy from the single simple harmonic vibration which the two shells would take if the globules were rigidly connected to them, or were removed from within them, and the other initial circumstances were those of § 6. But this conclusion depends on the forces being *rigorously* in simple proportion to displacements.



(10)[1] In no real case could they be so, and if there is any deviation from the simple proportionality of force to displacement, the independent superposition of motions does not hold good. We have still a theorem of fundamental modes, although, so far as I know, this theory has not yet been investigated. For any stable system moving with a given sum, E, of potential and kinetic energies, there must in general be *at least as many fundamental modes of rigorously periodic motion as there are freedoms* (or independent variables). But the configuration of each fundamental mode is now not *generally* similar for different values of E; and superposition of different fundamental modes, whether with the same or with different values of E, *has now no meaning*. It seems to me probable that every fundamental mode is essentially unstable. It is so if Maxwell's fundamental assumption[1] "that the system, if left to itself in its actual state of motion, will, sooner or later, pass through every phase which is consistent with the equation of energy" is true. It seems to me quite probable that this assumption *is* true, provided the "actual state of motion" is not exactly, as to position and velocity, a configuration of some one of the fundamental modes of rigorously periodic motion, and provided also that the "system" has not any exceptional character, such as those indicated by Maxwell for cases in which he warns[2] us that his assumption does not hold good.

(11) But, conceding Maxwell's fundamental assumption, I do not see in the mathematical workings of his paper[3] any proof of his conclusion "that the average kinetic energy corresponding to any one of the variables is the same for every one of the variables of the system." Indeed, as a general proposition its meaning is not explained, and seems to me inexplicable. The reduction of the kinetic energy to a sum of squares[4] leaves the several parts of the whole with no correspondence to any defined or definable set of independent variables. What, for example, can the meaning of the conclusion[5] be for the case of a jointed pendulum? (a system of two rigid bodies, one supported on a fixed horizontal axis and the other on a parallel axis fixed relatively to the first body, and both acted on only by gravity). The conclusion is quite intelligible, however (but is it true?), when the kinetic energy is expressible as a sum of squares of rates of change of single co-ordinates each multiplied by a function of all, or of some, of the co-ordinates.[6] Consider, for example, the still easier case of these coefficients constant.

(12) Consider more particularly the easiest case of all, motion of a single particle in a plane; that is, the case of just two independent variables, say x, y; and kinetic energy equal to $\frac{1}{2}(\dot{x}^2 + \dot{y}^2)$. The equations of motion are

$$\frac{d^2x}{dt^2} = -\frac{dV}{dx}, \qquad \frac{d^2y}{dt^2} = -\frac{dV}{dy},$$

where V is the potential energy, which may be any function of x, y, subject only to the condition (required for stability) that it is essentially positive (its least value being, for brevity, taken as zero). It is easily proved that, with any given value, E, for the sum of kinetic and potential energies, there are two determinate modes of periodic motion; that is to say, there are two finite closed curves such that, if m be projected from any point of either with velocity equal to $\sqrt{[2(E-V)]}$ in the direction, either-wards, of the tangent to the curve, its path will be exactly that curve. In a very special class of cases there are only two such periodic motions, but it is obvious that there are more than two in other cases.

(13) Take, for example,

$$V = \tfrac{1}{2}(a^2x^2 + \beta^2y^2 + cx^2y^2).$$

For all values of E we have

$$\left.\begin{aligned} x &= a\cos(at-e) \\ y &= 0 \end{aligned}\right\} \text{ and } \left.\begin{aligned} y &= 0 \\ x &= b\cos(\beta t-f) \end{aligned}\right\}$$

as two fundamental modes. When E is infinitely small we have only these two; but for any finite value of E we have clearly an infinite number of fundamental modes, and *every mode* differs infinitely little from being a fundamental mode. To see this, let m be projected from any point N in OX, in a direction perpendicular to OX, with a velocity equal to $\sqrt{(2E - a^2ON^2)}$.

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[1] Sections 10 to 17 added July 10, 1891.

[1] "Scientific Papers," vol. ii. p. 714.

[2] *Ibid.*, pp. 714, 715.

[3] *Ibid.*, pp. 716–726.

[4] *Ibid.*, p. 722.

[5] Or of Maxwell's "b," in p. 723.

[6] [It may be untrue for one set of co-ordinates, though true for others. Consider, for example, uniform motion in a circle. For all systems of rectilineal rectangular co-ordinates (x, y), time-av. $\dot{x}^2$ = time-av. $\dot{y}^2$; but for polar co-ordinates (r, θ) we have *not* time-av. $\dot{r}^2$ equal to time-av. $r^2\dot{\theta}^2$.—W. T., July 21, 1891.]

After a sufficiently great number of crossings and re-crossings across the line X′OX, the particle will cross this line very nearly at right angles, at some point, N′. Vary the position of N very slightly in one direction or other, and re-project *m* from it perpendicularly and with proper velocity; till (by proper "trial and error" method) a path is found, which, after still the same number of crossings and re-crossings, crosses exactly at right angles at a point N″, very near the point N′. Let *m* continue its journey along this path, and, after just as many more crossings and re-crossings, it will return *exactly* to N, and cross OX there, *exactly* at right angles. Thus the path from N to N″ is exactly half an orbit, and from N″ to N the remaining half.

(14) When $cE/(\alpha^2\beta^2)$ is a small numeric, the part of the kinetic energy expressed by $\frac{1}{2}cx^2y^2$ is very small in comparison with the total energy, E. Hence the path is at every time very nearly the resultant of the two primary fundamental modes formulated in § 13; and an interesting problem is presented, to find (by the method of the "variation of parameters") a, e, b, f, slowly varying functions of t, such that

$$x = a \sin(\alpha t - e), \qquad y = b \sin(\beta t - f),$$
$$\dot{x} = a\alpha \cos(\alpha t - e), \qquad \dot{y} = b\beta \cos(\beta t - f),$$

shall be the rigorous solution, or a practical approximation to it. Careful consideration of possibilities in respect to this case [$cE/(\alpha^2\beta^2)$ very small] seems thoroughly to confirm Maxwell's fundamental assumption quoted in § 11; and that it is correct whether $cE/(\alpha^2\beta^2)$ be small or large seems exceedingly probable, or quite certain.

(15) But it seems also probable that Maxwell's *conclusion*, which for the case of a material point moving in a plane is

$$\text{Time-av. } \dot{x}^2 = \text{Time-av. } \dot{y}^2, \quad \ldots \ldots \ldots (1)$$

is not true when α^2 differs from β^2. It is certainly not proved. No dynamical principle except the equation of energy,

$$\tfrac{1}{2}(\dot{x}^2 + \dot{y}^2) = E - V, \quad \ldots \ldots \ldots (2)$$

is brought into the mathematical work of pp. 722–25, which is given by Maxwell as proof for it. Hence any arbitrarily drawn curve might be assumed for the path without violating the dynamics which enters into Maxwell's investigation; and we may draw curves for the path such as to satisfy (1), and curves not satisfying (1), but all traversing the whole space within the bounding curve

$$\tfrac{1}{2}(\alpha^2x^2 + \beta^2y^2 + cx^2y^2) = E, \quad \ldots \ldots \ldots (3)$$

and all satisfying Maxwell's fundamental assumption (§ 11).

(16) The meaning of the question is illustrated by reducing it to a purely geometrical question regarding the path, thus:—Calling θ the inclination to x of the tangent to the path at any point xy, and q the velocity in the path, we have

$$\dot{x} = q \cos\theta, \qquad \dot{y} = q \sin\theta, \quad \ldots \ldots (4)$$

and therefore, by (2),

$$q = \sqrt{\{2(E - V)\}}. \quad \ldots \ldots \ldots \ldots (5)$$

Hence, if we call s the total length of curve travelled,

$$\int \dot{x}^2 dt = \int q \cos^2\theta \, q dt = \int \sqrt{\{2(E - V)\}} \cos^2\theta \, ds; \quad . (6)$$

and the question of § 15 becomes, Is or is not

$$\frac{1}{S}\int_0^S ds \sqrt{\{2(E - V)\}} \cos^2\theta$$
$$= \frac{1}{S}\int_0^S ds \sqrt{\{2(E - V)\}} \sin^2\theta \,? \quad \ldots (7)$$

where S denotes so great a length of path that it has passed a great number of times very near to every point within the boundary (3), very nearly in every direction.

(17) Consider now separately the parts of the two members of (7) derived from portions of the path which cross an infinitesimal area $d\sigma$ having its centre at (x, y). They are respectively

$$\left.\begin{array}{l} \sqrt{\{2(E - V)\}} d\sigma \int_0^\pi N d\theta \cos^2\theta \\ \text{and} \\ \sqrt{\{2(E - V)\}} d\sigma \int_0^\pi N d\theta \sin^2\theta \end{array}\right\}, \quad \ldots \ldots (8)$$

where $N d\theta$ denotes the number of portions of the path, per unit distance in the direction inclined $\frac{1}{2}\pi + \theta$ to x, which pass eitherwards across the area in directions inclined to x at angles between the values $\theta - \frac{1}{2}d\theta$ and $\theta + \frac{1}{2}d\theta$. The most general possible expression for N is, according to Fourier,

$$\left.\begin{array}{l} N = A_0 + A_1 \cos 2\theta + A_2 \cos 4\theta + \&c. \\ \qquad + B_1 \sin 2\theta + B_2 \sin 4\theta + \&c. \end{array}\right\} \quad \ldots (9)$$

Hence the two members of (8) become respectively

$$\left.\begin{array}{l} \sqrt{\{2(E - V)\}} d\sigma \tfrac{1}{2}\pi(A_0 + \tfrac{1}{2}A_1) \\ \text{and} \\ \sqrt{\{2(E - V)\}} d\sigma \tfrac{1}{2}\pi(A_0 - \tfrac{1}{2}A_1) \end{array}\right\} \quad \ldots \ldots (10)$$

Remarking that A_0 and A_1 are functions of x, y, and taking $d\sigma = dxdy$, we find, from (10), for the two totals of (7) respectively

$$\left.\begin{array}{l} \tfrac{1}{2}\pi \iint dxdy(A_0 + \tfrac{1}{2}A_1) \sqrt{[2(E - V)]} \\ \text{and} \\ \tfrac{1}{2}\pi \iint dxdy(A_0 - \tfrac{1}{2}A_1) \sqrt{[2(E - V)]} \end{array}\right\}, \quad \ldots (11)$$

where $\iint dxdy$ denotes integration over the whole space inclosed by (3). These quantities are equal if and only if $\iint dxdy A_1$ vanishes; it does so, clearly, if $\alpha = \beta$; but it seems improbable that, except when $\alpha = \beta$, it can vanish generally; and unless it does so, our present test case would disprove the Boltzmann-Maxwell general doctrine.

THE INTERNATIONAL GEOGRAPHICAL CONGRESS AT BERNE.

THIS Congress began its proceedings on Monday. Fourteen countries and forty-six Geographical Societies are officially represented. France has sent 73 delegates, Germany 33, Austria-Hungary 21, Switzerland 87, Italy 21, Russia 13, Great Britain 8, and Spain, America, and the Netherlands two each. Egypt, Portugal, Roumania, Greece, Norway, and Sweden are also represented. There are, in addition, 150 Members and Associates who have not yet given in their names.

M. Numa Droz, Swiss Minister for Foreign Affairs, bade the delegates heartily welcome to Berne.

Dr. Gobat, Regierungsrath, Berne, President of the Congress, then delivered his inaugural address. In the name of the Geographical Societies of Switzerland he thanked the *savants* present for responding so cordially to their invitation.

Among the good work already done, Prof. Penck, of Vienna, has proposed the following resolution:—"This Congress on the geographical sciences, held at Berne, resolves to take the initiative in the preparation of a large map of the earth on a scale of one to a million, of which the various sections shall be delimited by latitudes and longitudes; and, with this object, it appoints an international committee to determine the principles upon which the preparation of such map shall proceed. The members of this committee shall arrange that the various States engaged in preparing maps, the societies and periodicals publishing original maps, and all private geographical establishments working in this field shall prepare detached sections of the said map, the sale of which shall also be regulated and arranged for by the committee."

In the course of his address on the subject Prof. Penck paid a high tribute to the services rendered by Mr. Stanley to the cause of geographical science, directing special attention to the fact that each of the explorer's expeditions across Africa had led to the preparation of from 20 to 30 maps.

The proposal was referred to a committee of the Congress, which will report upon it.

The subjects of an initial meridian and universal time, geographical education, orthography of geographical names, lakes and glaciers, cartography, bibliography, meteorology, commercial geography, and voyages and travels are all to be touched upon in the deliberations.

SCIENTIFIC SERIALS.

Journal of the Russian Chemical and Physical Society, vol. xxiii., No. 1.—The chief papers are:—On the molecular weight of albumen, by A. Sabanéeff and N. Alexandroff. Several determinations were made on the method of Raoult, and gave an average of 14,276, the molecular weight thus appearing to be nearly three times as great as that deduced from the formula of

Harnack (4730), and nearly nine times as high as that given in Lieberkühn's formula (1612). The molecule contains nine atoms of sulphur, of which two are easily separated. Submitted to a temperature of 40°, the solution of albumen changes its properties, and its temperature of freezing is lowered.—On the measurement of density of sea-water, by Vice-Admiral Makaroff. This elaborate work gives the results of measurements made on board the corvette *Vityas*. The value of various instruments used during the cruise is discussed in detail, and the following formulæ are given as expressing the results of the observations between the temperatures of 0° and 30°. For distilled water, the density is—

$$S_0 = 0{\cdot}9998795$$
$$= S_t(1 - 0{\cdot}000061398t + 0{\cdot}0000080021t^2 - 0{\cdot}00000004586t^3),$$

maximum density at 3°·972. For sea-water, the density of which at 15° compared with that of distilled water at 4° is = 1·019, the formula is—

$$S_0 = 1{\cdot}0207769$$
$$= S_t(1 + 0{\cdot}000022268t + 0{\cdot}0000069801t^2 - 0{\cdot}00000004761t^3),$$

maximum density at – 1°·570. For sea-water, the density of which, also at 15°, is = 1·026, the formula is—

$$S_0 = 1{\cdot}0280936$$
$$= S_t(1 + 0{\cdot}000050453t + 0{\cdot}0000062833t^2 - 0{\cdot}00000003852t^3),$$

maximum density at – 3°·876. The last two formulæ gave excellent results for temperatures down to – 5°. A comparison between the figures obtained by the *Vityaz* and those obtained by the *Challenger* proved very satisfactory. Finally, the author gives six most valuable tables of corrections. Tables I. and II. contain the corrections to be applied to $S\frac{15}{4}$ for obtaining $S\frac{t}{4}$, and *vice versâ*, from – 5° to + 36°, for both distilled and sea-water. Detailed interpolation tables are also given. Table III. contains the corrections due to the coefficient of dilatation of glass of the areometer being not equal to the normal coefficient 0·000028. The three other tables are for transferring densities $S\frac{17{\cdot}5}{17{\cdot}5}$ into densities $S\frac{15}{4}$.

Bulletin de la Société des Naturalistes de Moscou, 1890, No. 3.—On the *Protopirata centrodon*, Trd., by H. Trautschold (in German). The two Ichthyodornlithes from the Carboniferous of North America, described in J. S. Newberry's capital work upon the "Palæozoic Fishes of North America," Table xxxix., are very much like the Moscow fossils described by the author in the above periodical (1884 and 1886) under the names of *Edectus protopirata*, and later on, of *Protopirata centrodon*.—Geo-botanical notes about the flora of European Russia, by D. I. Litvinoff (in Russian). The common Scotch fir (*Pinus sylvestris*) grows, as known, chiefly on a sandy soil. However, it also appears in the hilly tracts of Europe and Asia, and there it grows upon a rocky soil, chiefly limestone. In the lowlands of Germany and Russia, the appearance of fir upon a rocky ground is extremely rare; but there are some exceptions to this rule—namely, on the chalk hills of the Donets, the Volga mountains, the Middle Russian plateau, and the Silurian limestones of the Baltic provinces; in all those places the fir appears in company with a number of sub-Alpine and Alpine plants which are not met with elsewhere in the Russian plains, and with a number of endemic plants very rare in Russia as a whole. The author considers these rocky islands of fir-growths as survivals from the pre-Glacial period. The paper is full of most interesting botanical data and valuable remarks upon the connection of the glaciation of Russia with its present flora.—The influence of friction upon the rotatory motion of celestial bodies, by Th. Sloudsky (in French). The auxiliary theorems, upon which the principal theorem relative to the effects of friction is based, are demonstrated, the sun being taken as an illustration.—On the origin of endosperm in the embryo-pouch of certain Gymnosperms, by Miss C. Sokolowa (in French, with three plates). Strassburger's researches have proved the similarity between the formation of endosperm and of multicellular albumen, and the partition of cells, especially as regards the Angiosperms. The same researches are pursued by Miss Sokolowa as regards the Gymnosperms, attention being paid to the part played by the nucleus in the formation of partition walls.—Contribution to the morphology and classification of the Chlamydomonads, by Prof. Goroschankin (in German, with two plates).—Preliminary note upon inter-glacial layers about Moscow, by N. Krichtafovitch.

No. 4.—Traces of an inter-glacial period in Central Russia, by N. Krichtafovitch (in German; already analyzed in NATURE).—Remarks upon the function of the nucleus in cells, by J. Gerassimoff (in German), being observations upon cells without a nucleus in *Spirogyra* and *Sirogonium*.—On the molecular weight of the albumen of the egg, by N. Alexandroff (Russian).—Why the relative masses of the brain decrease in proportion to the increase of the weight of the body, in the same type of Vertebrata, by Fernand Lataste (in French).—*Tarentula* (*Lycosa*) *opiphex*, new species, by W. A. Wagner (French, with a plate). This trap-spider inhabits Middle Russia, and is especially numerous in the fields of Orel. Its thin trap, made of one sheet of web with some mould, is even more ingenious, for its shape, than that of the *Ctemiza*.

THE *Nuovo Giornale Botanico Italiano* for July contains two articles of interest to lichenologists: an account of the lichens of Brisbane gathered by Mr. F. M. Bailey, by Herr J. Mueller; and contributions to the lichen-flora of Tuscany, by Signor E. Baroni. Signor E. Tanfani has an important paper on the morphology and histology of the fruit of the Apiaceæ (Umbelliferæ), and Prof. C. Massalongo an account of the galls made by Acari on 45 species of trees, shrubs, and herbaceous plants, as well as of the insects which produce them.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, August 5.—Mr. Frederick Du Cane Godman, F.R.S., President, in the chair.—The President announced the death of Mr. Ferdinand Grut, the Hon. Librarian of the Society, and commented on the valuable services which the deceased gentleman had rendered the Society for many years past.—Dr. D. Sharp, F.R.S., exhibited *Japyx solifugus*, from the Eastern Pyrenees, and stated that in his opinion it was a connecting link between the *Thysanura* and *Dermaptera*. He also exhibited pupæ of *Dytiscus marginalis*; one of these was perfectly developed, with the exception that it retained the larval head: this was owing to the larva having received a slight injury to the head. Dr. Sharp also exhibited specimens of *Ophonus puncticollis* and allied species, and said that Thomson's characters of the three Swedish species, *O. puncticollis*, *O. brevicollis*, and *O. rectangulus*, applied well to our British examples, and separated them in a satisfactory manner. Thomson's nomenclature, however, would, he thought, prove untenable, as the distinguished Swede described our common *puncticollis* as a new species under the name of *rectangulus*.—Mr. F. W. Frohawk exhibited a bleached specimen of *Epinephele janira*, having the right fore-wing of a creamy white, blending into pale smoky brown at the base; also a long and varied series of *E. hyperanthus*, from the New Forest and Dorking. The specimens from the former locality were considerably darker and more strongly marked than those from the chalk. Amongst the specimens was a variety of the female with large lanceolate markings on the under side, taken in the New Forest, and a female from Dorking with large, clearly defined white-pupilled spots on the upper side. Mr. Frohawk further exhibited drawings of varieties of the pupæ of *E. hyperanthus*, and also a large specimen of a variety of the female of *Euchloë cardamines*, bred from ova obtained in South Cork, with the hind wings of an ochreous-yellow colour. Coloured drawings illustrating the life-history of the specimen in all its stages were also exhibited.—M. Sergé Alphéraky communicated a paper entitled "On some cases of Dimorphism and Polymorphism among Palæarctic Lepidoptera."

EDINBURGH.

Royal Society, July 15.—Sir Douglas Maclagan, President, in the chair.—The Prince of Monaco gave an account of the new yacht which he has had fitted out for the study of the sea. He also described the investigations which he has conducted since 1886, first in the Bay of Gascony, and then around the Azores and off Newfoundland. The latter investigations extended over three years, and had as their object the investigation of the direction and speed of the surface currents in the North

Atlantic. Special floats were thrown into the sea in three different places, and their progress was traced from place to place. As a preliminary trial 160 floats were thrown into the sea between the Azores and the Canary Islands. Some of these arrived at the Bermudas eighteen months later. In all 1700 floats were despatched, and the result was that the great ocean currents of the North Atlantic were now fairly well known. The Prince's new yacht is provided with an electric search-light of 10,000 candle-power for illuminating the surface of the sea when investigations are being carried on at night. Soundings can be made to a depth of 8000 metres without much difficulty. —M. le Baron Jules de Guerne, President of the Zoological Society of France, read a paper on the zoological results of the voyages of the *Hirondelle* (the Prince of Monaco's former yacht). He described the work of exploration among the Oceanic Islands, and alluded specially to the new species which had been found.—Mr. J. Y. Buchanan described a cartographic device which is of great use in the treatment of some geographical and telluric problems.—Mr. W. E. Hoyle described a deep-sea tow-net, which, by means of an electrical device, can be opened and closed at definite (arbitrary) instants.—Dr. H. R. Mill exhibited an improved form of his self-locking water-bottle.

July 20.—The Hon. Lord McLaren in the chair.—Some additional observations, by Prof. McIntosh, on the development and life-histories of the marine food-fishes and the distribution of their ova, were communicated. By means of various kinds of tow-nets, an endeavour has been made to ascertain the distribution of the eggs of the food-fishes on our shores. They are found at all depths, at the surface, and at the bottom. The floating eggs of the pilchard and mackerel are chiefly found on the south and south-west shores. On the east coast of Scotland the ova of the cod, whiting, and haddock are abundant. On the west coast, those of the sole, &c., abound.—The Astronomer-Royal for Scotland read a paper on the bright streaks on the moon. When the moon is half full its brilliancy is not nearly one-half so great as its brilliancy when it is quite full. Now at full moon the surface is observed to be covered by bright streaks which originate at the craters. The author believes that the great brightness of the full moon is due to these streaks. He considers them to be convex or concave, and so to be largely invisible under cross light, while they are brilliantly illuminated when the sun shines full upon them. The paper was illustrated by a model in plaster of Paris, with glass beads attached to its surface.—A paper, by Prof. C. G. Knott, on the effect of longitudinal magnetization on the interior volume of iron and nickel tubes, was communicated.—Dr. H. R. Mill read an obituary notice of Prof. C. I. Burton.

PARIS.

Academy of Sciences, August 3.—M. Duchartre in the chair.—Experimental researches on the probable *rôle* of gases at high temperatures and pressures, and in rapid movement, in various geological phenomena, by M. Daubrée. The experiments show how gases at high pressure, and contained in a closed reservoir, may, by a sort of latent action, violently push out rocks into conical or bell-shaped formations without any noise or escape of gas occurring to indicate their gaseous nature. —Heats of combustion and formation of nitrobenzenes, by MM. Berthelot and Matignon. The heats of combustion of ortho-, meta-, and para-dinitrobenzenes are found to be respectively 704·6, 698·1, and 696·5 calories; and the heats of formation 0·5, 6·8, and 8·4 calories. The heats of combustion of the two isomeric trinitrobenzenes examined are 665·9 and 680·6 calories; and the heats of formation +5·5 and −9·2 calories.—On the oldest European Dicotyledons observed in strata at Cercal, Portugal, by M. G. de Saporta.—On some improvements carried out in the manufacture of artificial Seltzer water: the siphon arrangement, by M. de Pietra Santa.—On a new and improved construction of the thermo-cautery of 1876, by M. Paquelin.—Periodic variations of the latitudes of solar prominences, by M. A. Riccò. The author's observations demonstrate that solar prominences, like spots, approach the equator up to the minimum period of activity, and afterwards begin again to appear more numerous in high latitudes.—On induction inclination needles, by M. Ernest Schéring. This is a brief description of a new magnetic inclination needle constructed by the author, and with which it is said to be possible to determine inclination with a probable error of 4″·2.—On the expansion of phosphorus, and its change of volume at the melting-point, by M. A. Leduc. The coefficient of expansion for solid phosphorus between 0° and 44°·1 is found to be 0·000372, whilst for liquid phosphorus between 26° and 50° the coefficient is 0·000560. The expansion is regular up to the melting-point, but an abrupt change of volume then occurs. The relation between the volume of phosphorus in the liquid and solid state is 1·0345.—Study of the chemical neutralization of acids and bases, by means of their electric conductivities, by M. Daniel Berthelot. From the investigation it appears that, when potash is acted on by hydrochloric acid, acetic acid, and phenic acid, compounds are formed having approximately equal electric conductivities. Ammonia, with the first two acids, gives similar stable salts, but with the last acid an unstable compound having a less electric conductivity is produced. Aniline forms with hydrochloric acid a stable compound having good electrical conductivity; and with acetic acid, an unstable body whose conductivity is said to be mediocre.—Action of phenylhydrazine on phenols, by M. Alphonse Seyewetz.—On the development of sponges (*Spongilla fluviatilis*), by M. Yves Delage.—On *Isaria densa*, Link, a parasite of the white worm, by M. Alfred Giard.—The parasite of the cockchafer, by M. Le Moult.—Action of poisons on the germination of the seeds of the plants which furnish them, by M. Ch. Cornevin.—On the resistance of the rabic virus to the action of prolonged cold, by M. Jobert.—Chromoscopic analysis of white light, by M. A. Charpentier.

Erratum.—On line 36, p. 336, instead of 0·1050 and 4·9720, read 1·1050 and 0·9720.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Elementary Science Lessons, Standard III.: W. Hewitt (Longmans).—Elementary Geometry of Conics, 7th edition: Dr. C. Taylor (Bell).—Instructions Météorologiques, 3me édition: A. Angot (Paris, Gauthier-Villars).—Bush Friends in Tasmania: L. A. Meredith (Macmillan).—Illustrations of the C.G.S. System of Units: J. D. Everett (Macmillan).—Elements of the Differential and Integral Calculus: A. Harnack; translation (Williams and Norgate).—Denmark: its Medical Organization, Hygiene, and Demography (Churchill).—Statistical Investigations concerning the Imbeciles in Denmark, 1888-1889 (Churchill).

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THURSDAY, AUGUST 20, 1891.

THE CONGRESS OF HYGIENE.

THE proceedings of this Congress were brought to a close on Monday, it being generally conceded that the importance of the conclusions arrived at and of the discussions on the more important topics were on a level with the numbers and eminence of the men of science taking part in the deliberations.

So far as space permits, we shall endeavour to give an account of some of the most salient subjects touched in the different Sections. To get a general idea of the enormous area of the ground covered, it is only necessary to glance at the resolutions adopted. It will be generally conceded that the members of the Congress are by these resolutions supplied with much food for thought in the interim which will elapse till the next meeting, which has been fixed at Budapest and for 1894.

We note with the greatest pleasure that Her Majesty and the University of Cambridge have shown their appreciation of the honour done to the nation by the presence of so many foreigners; and that other bodies and individuals have not been lacking to render possible gatherings of a less severely scientific character than the Sectional meetings.

Her Majesty's action in inviting many of the most eminent representatives of different nationalities to Osborne—an action, we believe, suggested by the Prince of Wales—has been so well received, that one regrets that the nation has had to wait so long for such a precedent. We regret it, not so much for the sake of men of science, but because the result has been that Royalty here has always lived apart not only from science but from national culture generally. The Queen, indeed, on the present system, never need know anything, except by some happy accident, of Britain's greatest men.

The party which went to Osborne left early by a special train, and were taken over from Portsmouth in the Queen's yacht. They were accompanied by Sir D. Galton, Dr. Poore, Prof. Corfield, and Mr. S. Digby. Luncheon was provided at 2, and Her Majesty later on received the visitors, of whom the following is an official list:—

Austria-Hungary.—Dr. Emil Kusý, Ministerialrath, Sanitats referent, delegated by Minister of the Interior; Hofrath Franz Ritter von Gruber, Professor of Architecture, elegated by Imperial Council of Health; Dr. Ernst Hofrath Ludwig, Professor of Applied Chemistry at Pathological Institute, delegated by Minister of Finance; Dr. J. Fodor, Professor of Hygiene, University of Budapest, delegated by Minister of Public Worship and Education.

Belgium.—M. E. Beco, Secretary-General of the Department of Agriculture, Industry, and Public Works, delegated by Minister of Agriculture and Public Works; D. E. Janssens, Inspecteur en chef de l'Hygiène à Bruxelles, Membre de la Commission Centrale de Statistique de Belgique, de l'Académie Royale de Médecine et du Conseil Supérieur d'Hygiène.

Denmark.—Dr. J. Lehmann, Dean of the Royal Sanitary Council, delegated by the Danish Government; Hans V. Berg, Medical Director of the Navy, delegated by Danish Naval Department; Sur.-Col. Laub, delegated by the Danish Army Department.

Egypt.—Dr. Hassan Pasha Ibrahim, Inspector Sanitary Department, and Professor of Hygiene.

France.—Dr. Etienne Jules Bergeron, Secrétaire perpétuel de l'Académie de Médecine, Vice-Président du Comité Consultatif d'Hygiène Publique, delegated by Ministry of Public Instruction M. le Dr. Brouardel, Doyen de la Faculté de Médecine de Paris, Président du Comité Consultatif d'Hygiène Publique, delegated by French Government; M. le Dr. Auguste Chauveau, Membre de l'Institut, delegated by the Ministry of the Interior, of Public Instruction, and of Agriculture; M. le Dr. Béranger Féraud, Président du Conseil Supérieur de la Marine, delegated by the French Government; Dr. Levraud, Président du Conseil Municipal de la Ville de Paris, delegate of City of Paris; M. Roux, Pasteur Institute, Paris.

Germany.—Dr. Büchner, Professor at Munich University, delegated by the Bavarian Government; Dr. von Coler, delegated by the Army Medical Department, Prussian Army; Prof. Sell, Geheimrath, delegated by the German Empire; Dr. Pistor, Geheim Medicinalrath, delegated by the Prussian Government; Prof. Dr. W. Roth, President of German Committee of the International Congress, Generalarzt des XII. K. S. Armée Corps, delegated by War Ministry of Saxony; Prof. von Koch, delegate of Government of Würtemberg.

Italy.—Dr. Angelo Mosso, Professor at Royal University, Turin, delegated by Italian Government; Dr. A. Corradi, Professor at Royal University, Pavia, delegated by Italian Government.

Japan.—Dr. Shimpei Gotoh, Official Expert in Ministry of Interior, Tokio, delegated by the Government of Japan.

The Netherlands.—Dr. G. van Overbeek de Meyer, Professor of State University, Utrecht, delegate of Government; Dr. W. P. Ruysch, Conseillier pour le Service Sanitaire, Department de l'Intérieur, delegated by Government of the Netherlands.

Roumania.—Dr. J. Felix, Professeur Université de Bucarest, Membre du Conseil Sanitaire Supérieure de Roumanie, Membre en chef de la Ville de Bucarest, delegated by Government of Roumania and City of Bucharest.

Russia.—Prof. Constantin Kowalkowski, Professeur d'Hygiène à l'Université Impériale de Varsovie, delegated by Imperial University, Warsaw.

Spain.—Don Juan Vilanova y Piera, President of Health Section of Royal Academy of Medicine, delegated by Spanish Government.

Sweden and Norway.—Dr. Linroth, Chief Medical Officer, Stockholm, delegated by Swedish Government and by City of Stockholm; Dr. Gotfried E. Bentzen, Director of the Civil Medical Service, Christiana, delegated by Government of Sweden and Norway.

Servia.—Dr. Georgevitch, delegated by Servian Government.

Switzerland.—Dr. Guillaume, Director of the Federal Bureau of Statistics, delegate of the Swiss Government; Col. Dr. Göldlin de Tiefenau, Instructeur en chef des Troupes Sanitaires Suisses, delegate of the Swiss Government.

United States of North America.—Major Alfred Woodhull, Medical Department, United States Army, delegated by United States Government Army Department; Lieut.-Col. Philip S. Wales, Medical Director United States Navy, delegated by United States Government Naval Department; Dr. Salmon, Chief of Bureau of Animal Industry in the United States Department of Agriculture, delegated by Department of Agriculture.

India.—Dr. Simpson, Sanitary Officer of Calcutta; Mancherjee Bhownaggree, C.I.S., member of the Bhavnagar Judicial Council, delegate of Maharajah of Bhavnagar; Dr. Prasanna Kimar Ray, Professor at Presidency College, delegated by Chancellor and Syndicate of Calcutta University.

Ceylon.—Dr. Solomon Fernando, delegate of Government of Ceylon, and of Medical College.

Canada.—Dr. Covernton, delegated by Ontario, Canada.

New South Wales.—Dr. J. Ashburton Thompson, delegated by Government of New South Wales.

Victoria.—Dr. Aubrey Bowen, delegated by Government of Victoria.

The visit to Cambridge took place on Saturday. The University authorities did all in their power to make it an agreeable one. Not only did hospitality abound, but even in the Long Vacation degrees were conferred (this, unfortunately, is impossible at Oxford) on Drs. Brouardel, Corradi, and Fodor.

The speeches made by the Public Orator were as follows:—

DIGNISSIME domine, domine Procancellarie, et tota Academia :—

Nescio quo potissimum exordio hospites nostros, qui de salute publica nuper deliberaverunt, senatus nomine salutare debeam. Ad ipsos conversus, illud unum dixerim :—qui aliorum saluti tam praeclare consuluistis, vosmetipsos omnes iubemus salvere. Ea vero studia, quae vobis cordi sunt, gloriamur in Britannia certe Academiam nostram primam omnium adiuvisse. In salutis publicae ministris nominandis valent plurimum diplomata nostra, valent etiam aliarum Academiarum, quae, exemplo nostro incitatae, laudis cursum eundem sunt ingressae. Hodie vero collegarum vestrorum nonnullos, qui gentium exterarum inter lumina numerantur, diplomate nostro honorifico decorare volumus. Nemini autem mirum sit, quod viros medicinae in scientia illustres iuris potissimum doctores hodie nominamus. Etenim Tullium ipsum in libris quos de Legibus composuit, scripsisse recordamini populi salutem supremam esse legem.

(1) Primum omnium vobis praesento gentis vicinae, gentis nobiscum libertatis bene temperatae amore coniunctæ civem egregium, Parisiorum in Academia medicinae forensis professorem praeclarum, facultatis medicae decanum dignissimum, salutis denique publicae annalium editorem indefessum. Olim Caesar omnes medicinam Romae professos civitate donavit ; nos non omnes certe, sed, habito delectu aliquo, unum e reipublicae Gallicae medicis illustrissimis, qui admirabilem in modum medicinae et iuris studia consociavit, corona nostra ob cives etiam in pace servatos libenter coronamus.

Duco ad vos PAULUM CAMILLUM HIPPOLYTUM BROUARDEL.

(2) Quo maiore dolore Austriae et Germaniae legatos illustres absentes desideramus, eo maiore gaudio Italiae legatum insignem praesentem salutamus. Salutamus Academiae Bononiensis, nobiscum vetere hospitii iure coniunctae, alumnum, tribus deinceps in Academiis, primum Mutinae, deinde Panormi, denique Ticini in ripa professorem, qui medicinae scientiam cum rerum antiquitus gestarum studiis feliciter consociavit, quique in Italiae scriptoribus eximiis, non modo in Boccaccio sed etiam in Torquato Tasso, artis suae argumenta non indigna invenit. Quondam imperator quidam Romanus Roma in ipsa augurium salutis per annos complures omissum repeti ac deinde continuari iussit. Quod autem salutis publicae concilio Londinensi etiam Italia interfuit, velut augurii felicis omen accipimus. Recordamur denique poetam antiquum urbis aeternae de nomine his fere verbis non inepte esse gloriatum :—

> Roma ante Romulum fuit ;
> non ille nomen indidit,
> "sed diva flava et candida,
> Roma, Aesculapi filia." [1]

Duco ad vos Aesculapi ministrum fidelissimum, ALPHONSUM CORRADI.

(3) Quis nescit urbem florentissimam quod Hungariae caput est, nomine bilingui nuncupatam, fluminis Danubii in utraque ripa esse positam. Quis non inde nobis feliciter advectum esse gaudet salutis publicae professorem insignem, virum titulis plurimis cumulatum, qui etiam de Angliae salubritate opus egregium conscripsit. Idem, velut alter Hippocrates, de aëre, aquis et locis praeclare disseruit. Olim Hippocrates ipse corona aurea Atheniensium in theatro donatus est : nos Hippocratis aemulum illustrem laurea nostra qualicunque in hoc templo honoris libenter ornamus.

Duco ad vos bacteriologiae cultorem acerrimum, IOSEPHUM DE FODOR.

The final general meeting of the Congress was held on Monday, under the presidency of Sir Douglas Galton. There was a large attendance, and among those present were nearly all the foreign delegates.

The Chairman, in opening the proceedings, after some preliminary remarks, said :—The success of the Congress, as an international gathering, is due to the fact that we as a nation have many matters of interest to show to foreigners. I think I may say that the chief difference between our hygienic progress and that of our Continental neighbours is that, whilst they are especially fortunate in being able to pursue the theories upon which much of modern hygienic progress is based, with us public opinion has hindered the study of many physiological questions, the solution of which depends upon the examination of living tissue. Hence, we at present are in this respect somewhat behind the Continental schools, and we largely turn our attention to apply their theories to alleviate the wants of life. Hence we can show much of interest in practical hygiene in matters both of construction and administration. Our methods of water supply and drainage, our various plans for refuse disposal or utilization, our isolation hospitals and ambulance systems present many interesting features. The arrangements which are being made to introduce sanitary knowledge and efficiency of workmanship in trades (such as the plumber), upon whom the practical sanitation of parts of our houses largely depends, are deserving of consideration ; and the health administrations of the large cities of Glasgow and Manchester is especially worthy of the study of our visitors. The organization of this Congress has differed from that of former Congresses in the increased number of Sections into which it was divided. In proportion as the study of hygiene and demography becomes more elaborate, the classification must necessarily be more detailed, and the number of Sections must either gradually increase or the Sections must subdivide. Independently of the increased number of Sections, it was found necessary to give two afternoons to the discussion of questions connected with the sanitation of our Indian Empire, which, for the first time in the history of these Congresses, was represented by a large number of delegates. The native Princes of India evinced deep sympathy with the Congress, and I trust that the interest which has been evoked in its object may lead to beneficial results in that great country. . . . A principal object of the Congress is, without doubt, to afford to scientific men in different countries the opportunity of conferring together. But it has another and most important object—viz. to excite the interest of the community at large in the knowledge of the laws of health. Your President the other day asked the pertinent question—Why, if diseases are preventable, are they not prevented? The answer to that question is that, whilst an instructed minority may understand the importance of observing hygienic laws, a very large section of the community is careless of and indifferent to their observance, and consequently the portions of those laws which are individual and personal in their application are left a dead letter. Acts of Parliament are of little avail so long as the people they are framed to guide do not realize their value or importance, and it is quite certain that the only way to stamp out preventable disease is to educate every member of the community to feel the importance of the laws of health. A great international Congress like this brings the subject prominently before the public and has a valuable influence on the country in which it is held. I have already detained you too long. But I must add, as chairman of the organizing committee, that we have endeavoured to make the Congress useful and agreeable to those who have honoured us with their presence. The success which we have had is mainly due to our secretary-general (Dr. Poore), our foreign secretary (Dr. Corfield), and, as far as India is concerned, to the energy of Mr. Digby. The excellence of the social arrangements is entirely due to the organizing power and tact of the secretary of the reception committee, Mr. Malcolm Morris. But you will have an opportunity of thanking the executive before the end of this meeting. If there have been shortcomings, the organizing committee much regret them. The only apology we can offer is that a voluntary organization suddenly created to fulfil the requirements of the moment may have been somewhat strained at first by the number who appeared on Monday morning—a number far in excess of that which former experience led us to anticipate, and I would say in conclusion, in the words of our poet Prior—

> "Be to our virtues very kind,
> Be to our faults a little blind."

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[1] Mariani *Lupercalia*, p. 384 of Baehrens, *Frag. poet. Rom.*

The meeting next discussed the place of the next Congress; we have already stated that Budapest was fixed upon.

Votes of thanks completed the business Among these, Dr. Sell (Germany) moved the following resolution:—

"That His Royal Highness the President be respectfully requested to convey to Her Majesty the Queen the dutiful thanks of this Congress for Her Majesty's gracious act in becoming Patron of the Congress, and for the magnificent hospitality shown by Her Majesty to members of the Congress during their sojourn in England."

Prof. Kusý (Austria) seconded the resolution.

Colonel Woodhall (United States) said that all members of the Congress must desire to express their gratitude for the way in which they had been received by that gracious lady Her Majesty the Queen, whose purity and dignity of life had enabled her to extend her empire of love and respect over even American citizens.

The resolution was unanimously agreed to.

His Excellency M. Gennadius, the Minister for Greece, moved the following resolution:—"That the best thanks of the Congress be dutifully tendered to His Royal Highness, the Prince of Wales, the President of the Congress, for the untiring interest which His Royal Highness has manifested in the Congress, and to which the success of the Congress is to be largely attributed."

Finally, the Chairman proposed a vote of thanks to the officers of the Association, whose unsparing work and indefatigable energy had so largely conduced to the success of the undertaking. He coupled with the vote the names of Dr. G. V. Poore, the hon. secretary-general, Prof. W. H. Corfield, the hon. foreign secretary, and Mr. Malcolm Morris, the hon. secretary of the reception committee.

The vote was warmly received, and was unanimously adopted.

The Permanent International Committee have appointed the following International Sub-Committee to prepare a scheme for the organization of future Congresses. The Sub-Committee consists of Prof. Dr. Brouardel, Hon. LL.D. Cantab. (France), Prof. Dr. Fodor, Hon. LL.D. Cantab. (Hungary), and Prof. Corfield (England), to represent Hygiene; and M. Körösi (Hungary) and Dr. Janssens (Belgium) to represent Demography.

It is understood that the Sub-Committee will consider the advisability of forming Permanent Committees in various country, the plan of having Committees outside the country in which the Congress is held having proved so successful in obtaining Foreign Members for the London Congress, at which it was adopted for the first time.

This week we give an account of the work done in the Section of Preventive Medicine.

In this Section the President, Sir Joseph Fayrer, K.C.S.I, F.R.S., commenced the proceedings by delivering the following inaugural address:—

My first duty on occupying this seat is to make fitting acknowledgment of the honour which has been conferred on me, and to assure those to whom I am indebted for it that, as I appreciate the distinction highly, so, with the aid of my colleagues in this Section, and the support of the many eminent men of science who will take part in its work, I hope to discharge faithfully the important trust reposed in me. My next and most agreeable duty is to offer to all who honour us with their presence, or who propose by co-operation to forward the objects of the Congress, a most hearty welcome and cordial recognition of the interest in it manifested by their presence; to express a hope that the deliberations and conclusions which result from their wisdom and experience may advance our knowledge, and tend to enhance the welfare of the human race. This hope is based upon the universal recognition of the need of, and capacity for, improvement in the conditions upon which physical well-being, immunity from disease, and prolongation of life depend; and this is evinced by the assembling together in this Congress of men of science from all parts of the world, who have devoted themselves to the great international, humanitarian purpose of ameliorating the conditions of mankind everywhere, so far at least as the application of the laws of health, and to some extent those of sociology, can affect this consummation. To all, then, we in this great city, who are interested in the progress of hygiene and demography, offer our cordial greeting, and express an earnest desire that our visitors may derive pleasure and benefit from their sojourn in London, and from the proceedings of the great assembly of which they form so important a part.

Before I invite Dr. Cuningham to open the first subject for discussion, it is right that I should make a few preliminary remarks on the general scope and objects of the work comprised in this section. I do not intend to occupy much of the short and valuable time at our disposal by discussing any special subject, or by anticipating that which those who follow me may have to say, but shall confine myself to a brief notice of the present aspects of preventive medicine, its recent development, how much it has operated and is now operating for the public good, how slowly but surely it is dispelling the cloud of ignorance and prejudice which has overshadowed and impeded the progress of sanitation, and how it is gradually imbuing the public mind with the conviction that prevention is better and often easier than cure, that health may be preserved, disease avoided, and life prolonged by the study and observance of certain well-known laws, which, correlating the individual with his surroundings, determine his well-being when conformed to, deteriorate or prevent it when neglected, and should enforce the maxim, "Venienti occurite morbo." Unprecedented progress in human knowledge characterizes the present century, and has not been wanting in preventive medicine. It is, however, during the last half of it that advance has been most remarkable, whilst it is in a later part of that period, that it has so established itself in the popular mind as to have passed from the region of doubt and speculation into that of certainty. It is now pretty generally understood that about one-fourth of all the mortality in England is caused by preventable disease, that the death-rate of large communities may be reduced much below that at which it has been wont to stand, the average duration of life may be made to approximate nearer to the allotted fourscore, and that the conditions of living may be greatly ameliorated. The chief obstacles to improvement have been ignorance and want of belief; a better knowledge of the laws of life and health, a more rational comprehension of the nature and causes of disease, are gradually but surely entailing improvement in the conditions of living and in the value of life, and the diminution and mitigation, if not extinction, of morbid conditions which have in past times proved so injurious or destructive to life. In short, as Dante says:

"Se' l mondo laggiù ponesse mente
Al fondamento che natura pone,
Seguendo lui avria buona la gente."
"Paradiso," viii., 142.

Such are the subjects contemplated in the work of this Section, and as far as time permits the most interesting of them will be discussed. Those selected are of great importance in their relations to public health; let us hope that observers who have formed their opinions from experience in other countries and under different circumstances may throw new light on them.

In the brief space of time at my disposal it would be impossible to give a continuous outline of the progress of preventive medicine during the past, or to trace its growth and development out of ignorance and superstition to its present well-established foundation on a scientific basis. It is of happy augury for mankind that the subject of public health is now fairly grasped by popular sentiment, and that, though ignorance, opposition, and vested interests still contest the ground, progress is sure, and the light of science is illuminating the dark places. It is now better appreciated than it ever has been, that the causes which induce disease and shorten life are greatly under our own control, and that we have it in our power to restrain and diminish them, and to remove that which has been called "the self-imposed curse of dying before the prime of life." It is, indeed, only recently that the resources of medical science have been specially devoted to the prevention as distinguished from the cure of disease, and how far successfully I hope in a few words to show, whilst I trust the proceedings of the various Sections of this Congress will indicate how much remains to be done. Did time permit, I might illustrate the progress of preventive medicine by contrasting the state of England with its population of more than 29,000,000 during the Victorian with the England of the Elizabethan age with its 4,000,000. I might remind you of the frightful epidemics which had devastated the land, in the forms of black death, sweating sickness, plague, petechial typhus, eruptive fevers, small-pox, influenza, and other diseases, such as leprosy, scurvy, malarial fever, dysentery, &c., of the wretched mode of living, bad and insufficient food, filthy dwellings, and ill-built towns and villages, with a country uncultivated and covered with marshes and stagnant water (according to Defoe, one-fifteenth part of England consisted of standing lakes, stagnant water, and moist places, the land unreclaimed, and with the chill damp of marsh fever pervading all). The homes of the people were wooden or mud houses, small and dirty, without drainage or ventilation, the floors of earth covered with straw or rushes, which remained saturated with filth and emitting noxious miasmata. The streets were narrow and unpaved, with no drains but stagnant gutters and open cesspools, while the food was principally salted meat with little or no vegetable. To this may be added a large amount of intemperance and debauchery. As it is, I can only just allude to them. In such conditions disease found a congenial nidus, and by a process of evolution assumed the various epidemic forms which proved so destructive to life. Some of these have gone, let us hope never to return, and the conditions which fostered if they did not cause them have gone also. Can we venture to hope that it will be the same with those that remain? Our immunity during the last diffusion of cholera gives some ground for thinking it may be so, if, indeed, the Legislature and popular intelligence should be of accord on the subject.

If we turn to the present, we find that great improvements have gradually been made in the mode of living; the houses are better constructed, the drainage and ventilation are more complete, the land is better cultivated, and the subsoil better drained; marsh fever and dysentery, at one period so rife, are unknown, and leprosy has long since disappeared. The death-rate is considerably reduced, and the expectancy of life enhanced. Water is purer, food is more varied and nutritious, clothing is better adapted to the climate, the noxious character of many occupations has been mitigated, and the mental, moral, and physical aspects of the people altogether improved; education is general, a better form of government prevails, and the social conditions are far in advance of what they have been; but still the state of our cities shows that improvement is demanded, and one object of this Congress is to point out why and how this may be effected, not only in this country but throughout the world.

If we inquire into the effects of certain well-known diseases, we find that they are less severe in their incidence, if not less frequent in their recurrence. With regard to small-pox, since the passing of the first Vaccination Act in 1840, the death-rate has diminished from 57·2 to 6·5 per 100,000 for 1880–84, though for the five years 1870–74 it was 42·7, thus showing that there was still much to be learnt about vaccination. Enteric fever was not separated from typhus fever before 1869, but since then the death-rate has decreased from 0·39 to 0·17 per 1000, and it has been shown that this improvement was synchronous in different parts of England with the construction of proper drains. The diminution in the death-rate from typhus fever is quite as striking, and this also is shown to have run parallel with improved sanitation in more than one large town. The death-rate from scarlatina fluctuated between 97 and 72 per 100,000 between the years 1851 and 1880, and though it has diminished considerably of late years (17 per 100,000 in 1886), a corresponding increase in the death-rate from diphtheria has taken place; this may be due in part to a better differentiation of the two diseases. In 1858 it was reported that phthisis killed annually more than 50,000 people; the death-rate from this disease has not decreased very much for England and Wales, but it has done so in some large towns, notably in Liverpool; and Dr. Buchanan and Dr. Bowditch of Massachusetts both showed a striking parallelism between the diminution of the death-rate from this cause and the drying of the soil resulting from the construction of sewerage works. Cholera first appeared in England in 1831, and there were epidemics of it in 1848–49, 1853–54, and 1865–66, but the number of deaths diminished each time it appeared, and though it has been present since, it has never reached the height of an epidemic. This is fairly attributable to local sanitary rather than to coercive measures. Preventable disease still kills yearly about 125,000, and, considering the large number of cases for every death, it has been calculated that 78¼ millions of days of labour are lost annually, which means £7,750,000 per annum; this does not include the days lost by the exhaustion so often induced by the still too numerous unhealthy houses of the poor. Towns, villages, and houses are still built in an insanitary way; the death-rate is still higher and the expectancy of life lower than it should be, and though we have got rid of the terrible plagues of the middle ages, yet in this century, now closing, other epidemics have made their appearance: cholera has four times visited us; fevers, eruptive diseases, and diphtheria have prevailed; influenza has appeared several times, even recently, and after leaving us last year, only to return with renewed virulence, caused in the United States a mortality almost equa to that of the plague. Much has been done, and a great deal of it in what is called the pre-sanitary age, but much remains to be effected. Let us hope that the future may be more prolific of improvement than the past; international philanthropy seems to say it shall be so. That we can exterminate zymotic disease altogether is not to be expected, but there cannot be a doubt that we may diminish its incidence, and though we may never be able to reach the "fons et origo mali," yet we can make the soil upon which its seed is sown so inhospitable as to render it sterile. The scope and objects of preventive medicine are not limited to the removing of conditions which give rise to zymotic disease, nor even of those which compromise otherwise the physical welfare of mankind, but should extend as well to a consideration of the best means of controlling or obviating those which, attending the strain and struggle for existence, involve over-competition in various occupations, whether political, professional, or mercantile, by which wealth or fame is acquired or even a bare livelihood is obtained, and under the pressure of which so many succumb, if not from complete mental alienation, from breakdown and exhaustion of the nervous system, which give rise to many forms of neurotic disease and add largely to the numbers of those laid aside and rendered unfitted to take their due share in the natural and inevitable struggle for existence. Or I might point to the recrudescence of those psychical phenomena manifested by the so-called hypnotism or Braidism, morbid conditions arising out of the influence of one mind upon another; this is a subject which demands not only further investigation, but great precaution as to its application, and claims the watchful notice of preventive medicine on account of the dangerous consequences which may ensue from it.

Again, the abuse of alcohol, opium, chloral, and other stimulants and narcotics, and the evil consequences which may result therefrom, is also a subject worthy of consideration, and will, no doubt, receive it in a communication which is to be brought before this Section.

The possible deleterious influence of mistaken notions of education, as evinced in the over-pressure which is exercised upon the young, the predominance of examinations, their increasing multiplication and severity, and the encouragement of the idea that they are the best test of knowledge, whilst true mental culture is in danger of being neglected, and physical training, if not ignored, left so much to individual inclination—this is another subject which demands the jealous scrutiny of preventive medicine, whose duty it is to safeguard the human race from all avoidable causes of either physical or mental disease.

Though preventive medicine in some form has been practised since the days of Moses, yet it has received but little recognition until a comparatively recent period; when science developed and observation extended, medical men and others became impressed with the influence of certain conditions in producing disease, and thus it was forced upon the public conscience that something must be done; and when philanthropists like John Howard devoted life and property to the amelioration of such awful conditions as existed—*e.g.* in our gaols, where the prisoners not only died of putrid fever, the result of ochletic causes, but actually infected the judges before whom they came reeking with the contagion of the prisons—rude sanitary measures gradually came into operation and partially obviated these evil conditions, but it was not before the middle of this century that any scientific progress was made; it was when Chadwick, Parkes, and others initiated the work by which they have earned the lasting gratitude of the human race that preventive medicine became a distinct branch of medical science. The sanitary condition of towns and communities is not dependent on the views or exertions of individuals alone, for they are and have been for the last fifty years largely cared for by the Legislature, and a variety of Acts have been passed which deal with questions concerning the public health; indeed, were all the provisions enforced, little would remain to be desired on the part of the executive Government, but as many of them are permissive, not compulsory, the benefit is less complete than it might be. The old difficulty of prejudice combined with ignorance still too often stands in the way, and, despite evidence which on any other subject would be conclusive, the most obvious sanitary requirements are often ignored or neglected. Many thousands of lives have been saved by the Sanitary Acts now in force; but there is little doubt that more thorough organization under State control, as under a Minister of Public Health, would have most beneficial results, and would save a great many more. We must acknowledge, however, that we are much indebted to the action of the Local Government Board, under whose able administration the most crying evils are gradually being rectified. Through the wise precautions enacted by it against the importation and diffusion of epidemic disease, when other parts of Europe were affected by cholera, this country escaped, or so nearly so as to suggest that it was to sanitary measures we owed our immunity. That there is something in the nature of epidemics which brings them under the dominion of a common law as to their extension seems certain; that there is much about them we do not yet grasp is equally true, but it is as surely the case that local sanitation is the preventive remedy as it is that coercive measures to arrest their progress are unavailing.

Under the improved system of sanitary administration which now obtains, and is gradually developing to a greater state of perfection, the sanitary administration of every district in the country is intrusted to the care of duly qualified health officers—a system from which excellent results have already accrued, and from which better still may be anticipated. The records of the past fifty years prove the influence exerted by sanitary measures on vital statistics. The first reliable tables from which the expectancy of life may be derived show that in 1838 to 1854 it was for males 39·91 years, for females 41·85 years; by the tables of 1871 to 1880 it had increased to 41·35 for males and 44·66 for females. It is shown also that the expectation of life increases every year up to the fourth year, and decreases after that age. For males up to nineteen years it is higher by the last tables, but after that age it is higher by the old table; for females it is greater by the new table up to forty-five, but after that age it is less. The improved sanitation saves more children's lives, but the conditions of gaining a living are harder than they were at the time of the first table, which accounts for the expectancy of life for adult men being less. Women remain more at home, where the better sanitation tells, and are not subject to quite the same conditions as men, so that their expectancy of life is greater than by the old tables up to the age of forty-five. A further proof of the effects of sanitary work is a decreased death-rate. Let us compare the death-rates of England during past times with the present; whether they be equally significant for other countries I cannot say, but these, at all events, sufficiently prove the point in question:—

DEATH-RATE.

1660–79	...	80 per 1000	1870–75	...	20 9 per 1000
1681–90	...	42·1 ,,	1875–80	...	20·0 ,,
1746–55	...	35·5 ,,	1880–85	...	19·3 ,,
1846–55	...	24·9 ,,	1885–88	...	18·7 ,,
1866–70	...	22·4 ,,	1889	...	17·85 ,,

In some parts of England, where the main object is the recovery or maintenance of health, the death-rate is down to 9 per 1000, while in others, where the main object is manufacture and money-making, it is as high as 30 per 1000. Nowhere, I think, have the beneficial results of sanitary work been better illustrated than in India during the past thirty years. A Royal Commission was appointed after the Crimean war to inquire into the sanitary condition of the British Army, and this in 1859 was extended to India. The European army was the special subject of it, but the native troops were referred to incidentally. Here the inquiry had to deal with a large body of men, concerning whom, their conditions of existence being well known, reliable information was accessible. It was ascertained that up to that time the annual death-rate over a long period had stood at 69 per 1000. The inquiry resulted in certain changes and improvements in the housing, clothing, food, and occupation of the soldier. Since those have been carried out there has been a steady decline in the death-rate, and the annual reports of the Sanitary Commissioners to the Government of India give the rates as: in 1886, 15·18 per 1000; 1887, 14·20 per 1000; 1888, 14·84 per 1000. During some years it has been even lower, down to 10 per 1000, whilst the general efficiency of the troops has increased. It is not easy to estimate the money equivalent of this, but if we take the rough standard which values each soldier at £100, a simple calculation will show how great is the gain, and who can estimate the value of lives saved and suffering avoided? As to native soldiers with whom the European troops may be compared, I find that the death-rate was: in 1886, 13·27 per 1000; 1887, 11·68 per 1000; 1888, 12·84 per 1000. Famine, cholera, and other epidemic visitations in some years disturb the regularity of the death-rate; under less favourable conditions of living, as in the case of prisoners in the gaols, it is somewhat higher. In the Indian gaols, for example, it was: in 1886, 31·85 per 1000; 1887, 34·15 per 1000; 1888, 35·57 per 1000.

On the whole, all this indicates improvement,[1] and as regards the civil population progress also is being made; but here, from so many disturbing causes, the figures are neither so easily obtained nor so reliable. The comparatively large mortality is due to neglect of the common sanitary laws added to extremes of climate, which favour the incidence and diffusion of epidemic disease, and intensify it when it has once appeared. A Sanitary Department has existed in India since 1866, and every effort is made by Government, at no small cost, to give effect to sanitary laws; there can be little doubt that the results, so far, are good, that disease generally is diminishing, and that life is of longer duration. An important result of the observations of the able medical officers of the Sanitary Service of India has been to show that cholera is to be prevented or diminished by sanitary proceedings alone, and that all coercive measures of quarantine or forcible isolation are futile and hurtful. Here I may say that, large as may appear the death-rate from cholera in India (*i.e.* in 1888, 1·99 per 1000 for the European army and 1·35 for the civil population), it is small compared with that of fevers, which caused in 1889 4·48 per 1000 in the European army and 17·09 in the civil population; but there is every reason to believe that these also are becoming less fatal under the influence of sanitary measures. In preventive as in curative medicine, knowledge of causation is essential. It is obvious that any rational system of proceeding must have this for its basis. A certain empirical knowledge may be useful as a guide, but no real advance can be expected without the exactitude which results from careful scientific observation and induction; the spirit of experimental research, however, is now dominant, and progress is inevitable. How much we owe to it is already well known, whilst under its guidance the reproach of uncertainty which attaches to medicine as a science is disappearing. Recent advances in physiology, chemistry, histology, and pharmacology, have done much to throw light on the nature and causes of, and also on the means of preventing or of dealing with, disease. It is impossible to exaggerate the value of the scientific researches which have led to antiseptic methods of preventing the morbific action of micro-organic life, whether the toxic effects produced by them, or those induced autogenetically in the individual. Theory has here been closely followed by its practical application in prevention and treatment of disease, whilst the study of bacteriology, which is of such remarkable pre-eminence at the present time, is opening out sources from which may flow results of incalculable importance in their bearing on life and health. That the conclusions arrived at are always to be depended on I doubt, and it seems that scientific zeal may perhaps sometimes outrun discretion. That it might be wiser to postpone generalization has, I think, been more than once apparent, whilst the expediency of further investigation before arriving at conclusions which may subsequently prove to be erroneous should not be lost sight of; but it has probably ever been so in the course of scientific progress, that in the enthusiasm of research, which is rewarded by such brilliant results, early generalization has too often been followed by disappointment, and it may be by temporary discouragement of hopes which seemed so promising.

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[1] "It is to be noticed with regret that during the last five years there has been a tendency to revert to a higher death-rate and percentage of sickness. Let us hope this will prove only transitory; the attention of sanitary authorities both at home and in India is anxiously directed towards the removal of whatever may be the cause of it. It is shown both by the vital statistics and the history of the chief diseases that there is in India an enormous amount of preventable sickness and death," but "that the local insanitary conditions or local disease causes are well known and widespread." —A. S. C.'s Reports for 1889.

It would be well to bear in mind a caution recently given by the Duke of Argyll, "that we should be awake to the retarding effect of a superstitious dependence on the authority of great men, and to the constant liability of even the greatest observers to found fallacious generalizations on a few selected facts" (*Nineteenth Century*, April 1891). Still, it is in the region of scientific research by experiment that we look for real progress, and we can only deplore the mistaken sentiment, the false estimate, and the misconstruction of its aspirations and purposes, which have placed an embargo on experiment on living animals, rendering the pursuit of knowledge in this direction well nigh impossible, if not criminal; whilst for any other purpose, whether of food, clothing, ornament, or sport, a thousandfold the pain may be inflicted without question. The inconsistency of the sentiment which finds unwarrantable suffering in an operation performed on a rabbit, when the object is to preserve human or animal life or prevent suffering, but which raises no objection to the same animal being slowly tortured to death in a trap, or hunted or worried by a dog, needs no comment; whilst the spirit which withholds from the man of science what it readily concedes to the hunter is, to say the least, as much to be regretted as it is to be deprecated.

It must be remembered that, important as are the researches into microbiology, there are other factors to reckon with before we can hope to gain a knowledge of the ultimate causation of disease. It is not by any one path, however closely or carefully it may be followed, that we shall arrive at a full comprehension of all that is concerned in its etiology and prevention, for there are many conditions, dynamical and material, around and within us which have to be considered in their mutual relations and bearings before we can hope to do so; still, I believe we may feel satisfied that the causes of disease are now being more thoroughly sought out than they ever have been—all honour to those who are prosecuting the research so vigorously—and that though individual predilection may seem sometimes to dwell too exclusively on specific objects, yet the tendency is to investigate everything that bears upon the subject, and to emphasize all that is implied in the aphorism, *Salus populi, suprema lex.*

The morning sitting of the Section and most of the afternoon sitting was devoted to papers and a discussion on "The Mode of preventing the Spread of Epidemic Disease from one Country to another."

The chair was occupied successively by the President, Professeur Brouardel of Paris, and Prof. da Silva Amado of Lisbon.

Surgeon-General Cuningham, of London, opened the discussion, and said the modes of prevention of spread of disease from one country to another were three in number, (1) quarantine, (2) medical inspection, (3) sanitary improvements. In his remarks he dealt chiefly with cholera, and he held that the chief factor of cholera, being carried by atmospheric currents, cannot be excluded from any country, and where it has been distributed over any area it excites the disease directly in many persons who are predisposed to it, and forms foci of it whenever it finds localities suitable for its increase; these are often very limited in extent, not embracing more than a single house, or even a portion of a house, or ship; the mortality among the steerage passengers in the latter is often very great, while the cabin passengers and all the crew have scarcely a case. Such foci are always badly ventilated, and the emanations arising in them acquire much greater density than in the open air; as a natural consequence the clothing of those who reside in them absorbs an amount of the emanation sufficient to produce cholera in susceptible persons outside until it has been dissipated by exposure; those so affected, however, and the others who have contracted the complaint apart from such foci, do not seem to have any such influence, it being not the body but the emanations from the locality which generate the disease. Cholera, therefore, cannot be excluded from any country by general quarantine. All that can be done is by hygienic measures to improve the health of the population, and to remove the conditions which favour the formation of foci. The placing ships which arrive with cholera on board under observation, removing their crews and passengers to suitable localities on shore until the disease ceases among them, are very proper precautions, and may prevent a small amount of the disease among the surrounding population, but can never prevent an epidemic if the necessary factors be in progress.

Inspector-General Lawson then followed with a paper on "The Communicability of Cholera from one Country to another."

To draw up a plan to prevent the extension of a disease, say cholera, from one country to another, with any prospect of success, it is necessary to have a general acquaintance at least with the different factors which contribute to the result, and of their mode of operation. The existing information on these points falls far short of these requirements, and its increase has been enormously impeded by the belief that man himself is the chief agent in diffusing the disease; and by interpreting the evidence obtained from various sources with an undue bias in favour of the theory. There has been, in short, and still remains, a most serious error in assuming that personal communication is the principal factor; and a no less extensive error in the methods and reasoning by which the central idea of diffusion by man was advocated.

The character and causes of cholera must be derived from a critical examination of all the evidence Nature presents, and from a study of the methods she herself adopts, instead of from our *a priori* deductions. Cholera occurs in two different forms: simple cholera or cholera nostras, of little severity, and attributed to local causes; and Asiatic epidemic, or malignant cholera, always a serious disease, and by many attributed to a poison given off by those labouring under it to others, and so diffused until it becomes epidemic.

Since 1832, when cholera visited Europe in the epidemic form, cholera nostras has been observed to fluctuate every few years, and with the milder cases occur a certain number presenting all the characters of the malignant disease; these cases occur singly or in small groups, but in every instance they accompany epidemics of varying severity, at no very great distance off, and are under the same "epidemic influence."

Those who support the theory that man diffuses cholera are, necessarily, required to show that persons under the disease must arrive at points where it has not yet appeared, before it commences in these latter, and that the first attacks in the new locality have been in persons exposed to the imported cases: but there are now a good many instances of epidemics springing up in localities at a distance from where the disease was already prevailing, and without any trace of importation, and where those first attacked had resided in the country for many months in succession without communication with any previous case. Such were the outbreaks at Southampton in 1865, at New Orleans in 1873, and at Toulon and the south of France in 1884, all of which were most carefully investigated on the spot. The only other conclusion open was that the necessary factors were supplied by epidemic influence; and if supplied in one instance, supplied in all: where there appeared to have been importation at the commencement of the outbreak, it must not be assumed that the disease was communicated by man unless the epidemic influence could be excluded, as at present it could not. It seemed probable that the exciting factors were conveyed by the air, whether fully or only partially developed, and consequently it was not in our power to exclude them; but much might be done by hygienic and other local means to limit their development in the localities they reached, and so to avoid excessive mortality.

Dr. Ashburton Thompson, official delegate of the Government of New South Wales, followed with a paper entitled "Quarantine in Australasia: Theory and Practice." He said that the amount of traffic which had to be dealt with was an important consideration in all questions of practical quarantine. The Australasian Sanitary Conference of Sydney, N.S.W., 1884, was attended by delegates of each of the six Governments, and by the speaker. Their resolutions were unanimous, accepted by each Government, and presented to each Parliament. They had not been modified since 1884, and were therefore those received in Australasia at the present day. Limited quarantine, medical inspection, the outcome of England's local conditions, was exactly suited to them, but not necessarily suitable, therefore, where local conditions differed from England's. The *first* proposition of the Conference was that *the degree of protection which quarantine measures can afford varies inversely with the ease of communication between the infected country and the country to be defended.* The difference between English and

Australasian conditions was described. The Conference rejected ancient quarantine as a principle of action, and on account of easy and daily interchange of population between the six territories decided to regard Australasia as constituting one epidemiological tract, and consequently to relinquish all quarantine as against each other. Then, before adopting resolutions which would affect others, they put themselves in order by declaring in a *second* proposition that *quarantine can yield protection commensurate with its costs only to countries whose internal sanitation is good;* and they recognized defects inherent in all quarantine measures by declaring, in a *third* proposition, that *the function of quarantine is not to exclude infection, but to lessen the entering number of foci of infection*, and thus made it clear that exclusive reliance was not placed by them on quarantine as a defence against imported disease. Having thus indicated what should be refrained from, it proceeded to say what should be done. *Nations whose internal sanitary organization was not perfect cannot afford to refer the observation of suspects to the country at large.* It was decided consequently that limited quarantine should be employed against ships actually carrying cases of exotic disease—that was, that vessels and equipment should be cleansed forthwith and held for delivery to owners at earliest possible date, but that the ship's company should be detained in isolation for periods slightly in excess of recognized clinical incubation periods. Medical inspection was thus rejected as a principle of action not less than ancient quarantine, but still not inconsiderately; when imported disease was one already familiar ashore, the circumstances were seen to resemble England's, and then medical inspection must (not might or could) be used. Accordingly, in case of scarlatina or the like, patients were removed to ordinary isolation hospital (not quarantine), the quarters cleansed, and the ship discharged in the usual way after five or six hours' detention. These principles were strictly adhered to by the Government of New South Wales since 1884. If not quite so closely by the other five Governments, the reason was probably political rather than commercial or scientific.

Dr. Rochard, of Paris (whose communication was read by Dr. Jules Bergeron), said that the means of preventing the transmission of epidemic diseases, such as the plague, yellow fever, and cholera, were threefold—namely, isolation, disinfection—and sanitation. The first was the simplest and the most radical. It was also the most difficult to use, because it required the intervention of public enactments, and the existence of an *entente internationale*. It was the system of quarantine and of the sanitary cordons. The second was more modern, and was the result of the development of contemporary science. The third rested on the progress of urban hygiene. It was probable that when we had sanitary towns we could brave epidemics. England had spent five millions since the commencement of the century, and it did not fear cholera during the last epidemic. Some of England's resistance to the cholera must be ascribed to its great distance from the source of cholera. M. Rochard next proceeded to detail the means taken at the frontier by the French authorities during the last cholera epidemic in Spain, and expressed the belief that it was necessary to persevere in the employment of those measures which responded to the necessities of the moment and to our present knowledge, until the future developed some better remedy.

Dr. Stékoulis, of Constantinople, after mentioning the methods quarantine and inspection, detailed by previous speakers, said that Turkey was like numerous other countries, one in which sanitary organization had yet to be carried out. If cholera has entered Turkey in these last years by Basjorah (Persian Gulf) and by Camaran (Red Sea) it was that the lazarets are not in accord with the progress of sanitary science. The pilgrimage of the Mussulmans to Mecca is also a great source of danger to the country. The lazarets of Turkey ought to be made sanitary, and there would be a great danger removed.

Dr. Hewitt, of Minnesota, U.S.A., said they had very little to do in his State with disease properly called epidemic except that of small-pox. Cholera had but once obtained something of a lodgment, and then it came directly from the port of New York. Small-pox came to them directly through emigration from the ports of England, and most of it came through the Gulf of St. Lawrence. Only the other day cases came from Liverpool to Minnesota. He mentioned one case in which infection was carried in the clothing of a woman who did not have the disease herself, but had been exposed on shipboard to it. The epidemic resulted in 300 deaths. For interior States like Minnesota the demand was that there should be complete sanitary central organization, with local organization in direct relation thereto, and that this organization should stand in direct relation to the quarantine service, which should be bound to give notice to the interior authorities of the presence of disease or infection, and that they should all co-operate for its control.

Dr. Simpson, of Calcutta, stated that the real source of cholera epidemics in Europe was, in his opinion, from emigrants and pilgrims coming over land and in ships to Mecca, where there was a focus 2000 miles nearer Europe than any Indian port.

Dr. Leduc, of Nantes, agreed with Dr. Cuningham as to the need of improved sanitary conditions in our towns, but he strongly disagreed with him when he proposed the suppression of quarantine. Modern science teaches us that contagious diseases are spread by wandering germs: isolation must therefore be a preventive to the spread of the disease, and quarantine presents us with the best means of isolation, so that to propose the suppression of quarantine was to propose a measure at once irrational and contrary to the principles of modern science.

Dr. Thorne Thorne, of London, spoke of the need of sanitary reform in towns, and deprecated the so-called protection of a country by means of cordons, quarantine, &c. The sixteen days' quarantine decided at Constantinople in 1866 failed, the ten days' quarantine decided at Vienna failed, and yet the five days' suggested at Rome is to succeed. The contention is altogether illogical.

Prof. Stokvis, of Amsterdam, said that at the International Medical Congress at Amsterdam there was a discussion on quarantine, in which the same arguments for and against were used as now. He then had no steadfast conviction. Now he had, and it was, that the only way to prevent the spread of epidemic diseases, and especially of cholera, was to make sanitary improvements. He had arrived at this conclusion by the study of the history of cholera in India, where cholera diminishes as sanitation improves. In the Dutch Indian Archipelago, where quarantine is of no consequence, the following figures show the great diminution in the death-rate which ensued on sanitary improvement. From 1864–78 the death-rate in the European army was 15 per 1000. In 1878 artesian wells, &c., were made. In 1879–83 the death-rate fell to 6·4 per 1000; and in 1884–88 to 3·5 per 1000. These figures are very striking, and lead one to hope that the saying of the late Prof. De Chaumont will come true, that the time will arrive when cholera will only be an historical curiosity.

The following gentlemen also took part in the discussion: Dr. Felkin of Edinburgh, Prof. Brouardel of Paris, Sir Joseph Fayrer, Surgeon-Major Pringle, Surgeon-General Cook, Dr. Robert Grieve of British Guiana, Dr. Ruijsch of the Hague, Brigade-Surgeon Staples, Surgeon-Generals Cayley, Ewart, and Beatson, Señor Vicente Cabello, and Brigade-Surgeon McGann.

In the afternoon, Sir John Banks, K.C.B., in the chair, Dr. Manson read an elaborate paper on "The Geographical Distribution, Pathological Relations, and Life-history of *Filaria sanguinis hominis diurna* and *Filaria sanguinis hominis perstans* in connection with Preventive Medicine." The paper was illustrated by numerous microscopical specimens.

Dr. Manson said that the discovery of the blood-worms herein named *Filaria sanguinis hominis diurna* and *Filaria sanguinis hominis perstans* suggests an investigation into their possible pathological relations, and into their life-histories, with the view to intervention in respect to them of preventive medicine.

The facts that these parasites and the disease known as negro lethargy, or sleeping sickness of the Congo, are endemic in the same region, the West Coast of Africa; that neither can be acquired unless in this particular region; and that sleeping sickness may declare itself many years after the endemic region has been quitted, and that these filariæ continue to live for many years after the negro has left Africa; suggest a possible relationship between these parasites and this disease.

A papulo-vesicular skin disease called craw-craw is endemic in the sleeping sickness region, and sleeping sickness is often accompanied by a similar papulo-vesicular skin disease, probably the same. O'Neil found a filaria-like parasite in the vesicles of craw-craw. Nielly considers a disease he calls *dermatose parasitaire*, which he found in a lad in France, the same as the African craw-craw; he discovered in the vesicles of the skin in this case the same or a similar parasite to O'Neil's. Nielly, at the same time, found an embryo filaria in his patient's blood which was undoubtedly an earlier form of the skin worm. From this the inference may be drawn that, in certain cases, at all events, of sleeping sickness a filaria embryo is present in the blood.

Filaria s. h. diurna and *Filaria s. h. perstans* have both been found in a case of sleeping sickness.

These facts taken together amount to a presumptive case against one or other of these parasites as the cause of sleeping sickness.

The probable life-histories of these worms is then indicated, the *Filaria loa* being considered the parental form, and an insect, called the mangrove fly, the intermediary host of *Filaria s. h. diurna*. The parental form of *Filaria s. h. perstans* is not known, but, assuming that the worm of craw-craw, sleeping sickness, and *dermatose parasitaire* is the same, and that the skin form is an advanced stage of the embryo filaria found in the blood, then, arguing from the analogy to what happens in the case of the embryo of *Filaria medinensis*, which closely resembles this skin parasite, the probable intermediary host of *Filaria s. h. perstans* is a freshwater animal, possibly a cyclops.

Provided the hypotheses as regards these parasites and the diseases they produce are correct, both disease and parasites may be avoided by securing a pure water supply to which the intermediary hosts of the parasites do not get access.

Travellers, missionaries, and others in Africa are appealed to for assistance in clearing up the subject, and for further information.

An appendix to the paper contains directions for demonstrating in the surest, most rapid, and most effective way the presence or absence of filaria embryos in blood, and of making collections of slides of blood for storage and future examination.

Dr. Sonsino, of Pisa, made a few remarks on Dr. Manson's paper. The meeting then adjourned.

On Wednesday, August 12, the chair was occupied successively by Sir Joseph Fayrer, Dr. Pistor of Berlin, and Surgeon-General Roth of the Saxon Army.

DISCUSSION ON DIPHTHERIA.

Dr. Edward Seaton, of London, opened a discussion on "Diphtheria, with special reference to its distribution and to the need for comprehensive and systematic inquiry into the causes of its prevalence in certain countries and parts of countries, with a view to its prevention."

Dr. Seaton said that he should confine himself in introducing this subject to leading statements, showing the necessity for comprehensive and systematic inquiry to be promoted by Government into the causes of the prevalence of diphtheria in certain countries and parts of countries, with a view to its prevention. He first of all pointed to the special prevalence of the disease, as shown by Dr. Longstaff, in Norfolk and Wales, and the comparative freedom of Devonshire, Cornwall, and the Midlands. He then dwelt on the facts that the disease prevailed more in rural than urban districts, although it has shown of late years an increasing preference for urban populations, especially that of London. He showed the independence of the disease of what are ordinarily called sanitary conditions, and illustrated this by a table taken from Dr. Thorne Thorne's recent lectures at the Royal College of Physicians, showing the fall in enteric fever mortality in England and Wales which had synchronized with a rise in the mortality from diphtheria. He further illustrated the independence of diphtheria prevalence of what are usually termed sanitary conditions by experiences gathered from a large manufacturing town in the Midlands, and from certain parts of the metropolis in which he had special opportunities for observation as a medical officer of health, as well as in connection with the work of the Metropolitan Asylums Board, into whose hospitals cases of diphtheria had been received during the last three years. He also gave a recent experience of a Surrey village, in which the disease had prevailed in an epidemic form, shortly after the replacement of the old insanitary cesspool system by a new and elaborately constructed sewerage system. The occurrence of the disease under these circumstances gave rise to the suspicion that there might be a connection between diphtheria and conditions of soil, which needed to be investigated in a comprehensive and systematic manner. In conclusion, he pointed out the importance of these main considerations, viz.: (1) the prevalence of the disease in strikingly different degree in countries in the same latitude and with similar climatic conditions and also in parts of countries close to each other, (2) the fact that it has not apparently been influenced favourably by the adoption of sanitary measures which have been generally found effective in reducing the death-rate, prove the necessity for a comprehensive inquiry by our own Government as well as those of other countries, into the causes which determine the prevalence of diphtheria. Such an inquiry should take into account what has already been ascertained with regard to the occasional causation and spread of the disease by milk, and the influence which schools have on its production and spread, and also the subsidiary influence of dampness, dirt, overcrowding, &c.; but its main object would be to ascertain the local conditions and circumstances which account for the growth of the disease. To ascertain these the inquiries must, of course, be made in countries marked by freedom from the disease as well as in those which suffer from it specially.

Dr. Schrevens, of Tournai, followed with a paper entitled "Contribution à l'étude des causes favorisant les endemies diphthéritiques," of which the following is an abstract.

By investigating carefully how the ravages committed by diphtheria are distributed over the different districts, one can attain more easily to a precise knowledge of the external conditions which favour the harbouring of diphtheritic germs, and which result in such germs being brought into a locality. Investigations were made by the author in Belgium with this object. Thanks to the figures kindly furnished by Dr. Kuborn, the distribution of diphtheria throughout the different provinces of Belgium for the ten years from 1871 to 1880 has been determined. The same having been done for typhoid fever, it was noticed that where this latter disease committed the greatest ravages the same fact was observable in the case of diphtheria; and that where diphtheria secured its smallest number of victims the number of deaths caused by typhoid fever diminished equally. This parallel rise and fall of the mortality caused by typhoid fever and diphtheria is shown in two diagrams placed near each other on the same sheet; in the first, the parallelism is less evident, because one province, East Flanders, forms an exception to the rule I have just laid down; in the second diagram this province is omitted, and the parallel march of diphtheria and typhoid fever stands out clearly. On what does this relation, this agreement rest? On this fact, that these two diseases must be considered as fœcal diseases, as B. Russell, of Glasgow, has remarked. The bacilli of Löffler, like the bacilli of Eberth, develop admirably, prosper, and extend wherever filth and rubbish of all kinds are stored up or spread out; there exists, however, this slight difference between the conditions which are severally favourable to them: impurities on the *surface* of the soil suit the bacilli of Löffler in a special degree, while impurities of the *subsoil* please the bacilli of Eberth better.

Even the exception formed by East Flanders tends to confirm this rule, inasmuch as it is perfectly clear that its surface ought to be more easily cleared of all impurities by reason of the numerous watercourses which furrow it. A further proof that it is, in a special degree, impurities of the surface which serve to harbour diphtheritic germs in certain localities, is the exaggeration of mortality from diphtheria in country districts compared to what obtains in towns; density of the population is not of the least influence on the increase of the mortality due to diphtheria; but the surface of the soil is much better protected in towns against impurities of all kinds.

Another circumstance which may foster diphtheria in a locality is the breeding of certain species of animals presenting a great receptivity for diphtherogenic germs: for example, Italian fowls and game-cocks. The transmission of diphtheria to man by these animals is so well established by the observations collected by the author for several years past that he feels persuaded of the need of further attention being paid to this subject. Finally, a third condition which necessarily fosters diphtheria in a locality is the negligence exercised in the application of measures of disinfection and isolation.

Every case of diphtheria must be notified to the local authority, who will see to it immediately that all the children of the sick person's family be kept away from school as long as any danger of contagion exists. In every case disinfection must be rigorously attended to and performed by special agents. Notification and disinfection ought to be obligatory.

The altitude of the locality does not probably exercise any very great influence. One would suppose that diphtheria would be specially prevalent in low, damp places. Recent observations by the author on the progress of diphtheria in three contiguous parishes of the district of Ath (Œudeghien, Ostiches, and Mainvault), show that in each of these parishes there was a principal seat of the malady, and that in the three parishes this seat was in precisely the most elevated hamlet of all, a fact which from the first appears somewhat strange. One may, perhaps, conclude that Löffler's bacillus does not like too much damp, and that it is in this respect that its character differs from the bacillus of Eberth.

Dr. Hewitt, Secretary and Executive Officer of the State Board of Health of Minnesota, U.S.A., said that his experience covered eighteen years of sanitary service with the disease in an interior State of the American Union with a very complete public health service, consisting of 1575 local boards of health, with a State Board. Notification of infectious disease by physicians, householders, hotel and inn keepers, has been obligatory since 1883 with penalty, as is also isolation and disinfection by the local boards of health. The facts believed to be proven in Minnesota were that the disease is very infectious, that it is communicable by persons and things, that the infection lives and grows outside the body and below the body temperature, that it is very tenacious of life as against measures of disinfection, and lives for long periods in clothing and bedding and on floors and walls. Isolation and systematic disinfection, with the most perfect sanitary regulation, are most efficient at present in the control of the disease. Since these had been in efficient use the prevalence had assumed a family character, limiting itself to one or more associated families, and rarely going beyond, except by evasion of the law on the part of an infected person. What was needed now was more careful collection of the facts of each outbreak with a view to a more accurate knowledge of the disease, not neglecting the preventive and controlling measures now found to be most efficient, as above.

Dr. Jules Bergeron, of Paris, followed with a paper entitled "Note sur la Prophylaxie de la Diphthérie." Dr. Bergeron said that the measures to be taken against diphtheria were disinfection and isolation: disinfection of all clothing, &c., contaminated with secretions from the affected parts; isolation of all cases and of all doubtful cases, such as those of a herpetic character, which are difficult to distinguish from diphtheria in the early stage of the disease. An important question to be answered is, How long ought isolation to continue; how long, in fact, does contagion last? Dr. Bergeron says that he adopts six weeks' isolation as the maximum, and that he has never observed a case of transmission of the disease when a case has been isolated for this period.

Dr. Gibert, of Havre, spoke of diphtheria in Havre. He said that diphtheria appeared in Havre about 1860, and was limited to the Graville Quartier. In 1864, there was an epidemic close to Eryonville. From this date the number of deaths constantly increased, and the disease, which at first was confined to only a few localities, spread throughout the town. The severity of the disease increased until 1885, when a *brigade de salubrité* was formed as an annexe to the Bureau d'Hygiène. The dwellings occupied by diphtheritic patients having been regularly disinfected, the mortality curve has since decreased to such an extent as to justify the hope of its total extinction, provided all the medical men of the town furnish accurate information to the Bureau d'Hygiène.

Dr. S. W. Abbott, of Boston, U.S.A., read a paper on "Diphtheria in Massachusetts from 1871–88." From his observations he concludes that diphtheria is an eminently contagious disease, that it is infectious, not only by direct exposure of the sick to the well, but also through indirect media, such as clothing and other articles that have come in contact with the sick; that the infection is not so great as in the case of some of the other infectious diseases, notably small-pox and scarlet fever. Dr. Abbott also concludes that overcrowding, &c., favours the spread of the disease; but that its transmission through the water supply is not proved. Its transmission is favoured by soil-moisture and damp houses; and the poison may remain infective in houses for a long period.

Mr. Matthew A. Adams, of Maidstone, read a paper on "The Relationship between the Occurrence of Diphtheria and the Movement of the Subsoil Water." The conclusions he arrived at were that the organism of diphtheria inhabits organically polluted surface-soil, and that, subject to suitable conditions of environment, especially as respects moisture, temperature, and food, it thrives and multiplies in the soil, the micro-organism thus produced being liable to displacement from the interstices of the polluted surface-soil, and to dispersal into the superincumbent air; in this manner determining outbreaks of the disease. So that, given the existence of the pathogenic organism, two sets of factors at least are engaged in the production of a state of affairs that culminate in an outbreak of diphtheria. First, those that promote and support the growth of the germ in the soil, such, for instance, as moisture, temperature, air, food, and so on. Secondly, agents of dispersal, by which the germs already existing in the soil are driven out and distributed into the atmosphere, and so come to be breathed by man and animals; for example, sudden rainfall, rise of subsoil water, lowering of barometric pressure.

Mr. Charles E. Paget, of Salford, followed with a paper on "A Local Examination of the Difference in Susceptibility between Old and New Residents."

The general conclusion at which he arrived as the result of an examination of the statistics of Salford was, that a shorter average period of residence before an attack of diphtheria was observed where the general mortality rate was highest and *vice versâ*; that, in fact, the relative incidence of diphtheria during an epidemic period, in respect of length of residence, was dependent to no small extent on general sanitary circumstances.

Prof. D'Espine, of Geneva, followed in the discussion. He drew attention to the great value in the prophylaxis of diphtheria in the systematic washing out of the mouth and pharynx by antiseptic solutions, corrosive sublimate (1 in 10,000), salicylic acid (1 in 2000), and lime-juice. In his practice he used salicylic acid in the strength of 1½ to 2 per 1000.

Dr. Tripe, of Hackney, who followed, said he had had large experience of this disease, as he had been 35 years Medical Officer of Health in Hackney. During that time all deaths had been investigated, and lately all cases, with the result that there was no evidence that insanitary conditions of houses caused the disease, although they might predispose to it. He believed that closing playgrounds in schools is as effectual in checking the disease as closing the schools; that prompt removal to hospital and disinfection of clothing and rooms, burning of infected rags, &c., are the best methods for checking the disease.

Dr. Thursfield, of Shrewsbury, agreed with Dr. Hewitt that dampness had a great deal to do with the etiology of diphtheria; he had himself stated so thirteen years ago in a series of papers on the subject. He thought Dr. Adams's conclusion regarding the connection of the rise and fall of the subsoil water with outbreaks of diphtheria a somewhat hasty generalization.

Dr. Günther of Dresden, Dr. Janssens of Brussels, Dr. Hubert of Louvain, Dr. Escherich of Graz, Dr. Jules Felix of Brussels, and Dr. P. Sonsino of Pisa, also took part in the discussion; many of the speakers emphasizing the need of local antiseptic measures in the prophylaxis of diphtheria.

At the end of the discussion, the following recommendation was unanimously adopted by the Section:—

"That this Section urges the European Governments to make a comprehensive and systemative inquiry into the causes of diphtheria."

On Tuesday afternoon, Sir John Banks, K.C.B., and Overlaege Bentzen, Christiania, occupied the chair.

DISCUSSION OF THE PREVENTABILITY OF PHTHISIS.

Dr. Arthur Ransome, F.R.S., read a paper "On the Need of Special Measures for the Prevention of Consumption." He said, that consumption is both curable and preventable will be acknowledged at once by all medical men who have had any experience of modern methods of dealing with the disease.

Its curability is attested (1) by the reports of many pathologists as to the presence of evidence of healed phthisis in a large proportion of bodies examined in public institutions. Many thousands of such examinations have now been made, and the results show that from 25 to 50 per cent. of persons dying from other diseases than phthisis give signs of spontaneous cure of tubercular disease. (2) The testimony of all the most eminent modern physicians is to the same effect, that consumption is distinctly curable.

With regard to the preventability of the disease we have also a strong basis for our faith.

(1) In the marvellous results that followed the improved drainage and ventilation of the barracks of the British army in all parts of the world. Before the year 1854, the mortality from lung disease amongst the picked population of these dwellings was a scandal to the nation, and was enormously greater than that of the ordinary inhabitants of our towns, especially in the battalions sent to warm climates, such as those of India, Ceylon, the West Indies, the Mediterranean, &c.

Thanks to the above-mentioned measures, it now stands at from one-third to one-tenth of its former rates.

(2) The influence of improved drainage has been shown by Dr. Buchanan, in his table of towns, contrasting the mortality by phthisis and other diseases before and after the introduction of improvements in this direction; and lastly, by the reduction of the general phthisis rate of the country from 2500 per 1,000,000 in 1867, to 1500 per 1,000,000 in 1889.

My own observation in Manchester and Salford, and those of Dr. Irwin in Oldham, and of Dr. Flick in Philadelphia, point to the existence in towns of tubercular areas and infected houses.

Under these circumstances it seems to me that the duty of sanitary authorities is clear. They should regard phthisis as a disease to be dealt with on precisely the same lines as the analogous diseases, typhoid fever, cholera, and leprosy—diseases, namely, which are slightly, if at all, directly contagious, but which spread by material thrown off from the bodies of the patients. The means to be employed to this end would also be very similar: (1) notification of cases; (2) disinfection; (3) hospital accommodation; and (4) general sanitary measures, such as ventilation, drainage, and reconstruction of unhealthy areas.

(1) *Notification.*—At first it may sound somewhat novel to demand that a slowly progressing ailment like phthisis should be notified as if it were liable to become an epidemic disease; but, after all, we may fairly inquire whether the purpose of notification is not the prevention of any disease that could be arrested by early intelligence of its existence being sent to the health officer, nor would there be much difficulty in obtaining the notification of phthisis. Although phthisis is not directly contagious, there would be nothing unreasonable in classing it with other diseases that need special measures to prevent its spread.

(2) *Disinfection.*—After receiving notice of a case of tuberculosis, the next step to be taken by a local authority would be to ascertain whether proper care is or can be taken to prevent injury to the public health. In the case of well-to-do persons the information given by the medical attendant would be sufficient, but where the case is that of a poor person it should be visited, and the local authority should see to the regular cleansing and whitewashing of the premises, and to the disposal of excretions, especially of the expectorated matter. If necessary, disinfection by sulphur and the steaming of clothes should be carried out. Paper spittoons that can be burnt should be insisted upon. After death, also, measures should be taken for the cleansing and disinfection of house, bedding, and clothes.

(3) *Hospital Accommodation.*—There would next come the question of the propriety or possibility of removing the sick person to hospital. So long as he (or she) could work, and so long as he would consent to use the necessary means for destroying the infective material, it would be unnecessary to do more than I have already indicated; but when the patient becomes unable to follow his employment, and the family are obliged to seek for assistance from the parish, he has a claim to be received into the workhouse hospital, and such an asylum should be offered him, and should be made as little humiliating and as free from ignominy as possible.

(4) But it is probably to general sanitary measures that we must look for any large reduction in the rate of mortality from tubercle. It has been found that deep and thorough drainage of the subsoil will greatly diminish this mortality. In the case of Salisbury, as you are probably aware, it was reduced by one-half, and similar reports have come from other towns; and though the same result has not always been obtained elsewhere, there can be no doubt as to the importance both of draining and concreting the foundations of dwelling-houses, so as to prevent organic vapours from rising along with the ground air into living-rooms.

It is for this reason that I have ventured to suggest that where consumption is prevalent there must exist some special nutriment which either (1) serves to prolong the life of the bacillus of tubercle, or (2) which may even increase its virulent properties, this special element in foul air being either the organic matter exhaled from human bodies, or the emanations from polluted ground air from badly drained subsoils. I should imagine that either of these hypotheses might account for the result, and certainly in the few experiments which I have carried out to find the conditions that modify the virulence of the bacillus it was proved that foul air caused the organism to retain its power for evil much longer than when it was exposed to some fresh air and light.

It is possible that these may be regarded as somewhat strong proposals, but at least they have the merit that they may all be put in force without any material increase in the powers now possessed by local authorities. The only thing needed to enable them to be carried out in their entirety is a powerful public opinion to back them up. When people generally, and especially the working classes, realize that a large part of their sickness and consequent loss of time and money is due to their neglect, they will unquestionably be on our side. The undertaking possesses, moreover, the further merit that not only will all this sanitary improvement prevent consumption and other tubercular diseases by doing away with the sources of infection, but it will also prevent them by raising the general standard of health amongst town dwellers. It will so strengthen those who are already predisposed to the disease that they will more readily throw off any stray germs of tubercle that may find an entrance into their bodies. It will conduce to spontaneous cure, will prevent recurrence of the disease, and will ward off attacks from those who are now healthy.

Prof. Finkelnburg, of Bonn, read a paper "On the Influence of Soil on the Spread of Tuberculous Diseases."

He showed on a large map of Germany that the localities where phthisis was most prevalent were those in which there was a moory soil with stagnating and high-standing ground water; such as some districts in the north-western provinces, in the Rhenish province, in Upper Bavaria, and in some parts of Silesia. These facts agree with the conclusions of Bowditch and Buchanan. Overcrowding did not appear to have much influence on the spread of phthisis.

Dr. J. Edward Squire, of London, read a paper entitled, "To what extent can Legislation assist in diminishing the Prevalence of Consumption and other Tubercular Diseases."

Dr. Squire considered that the danger of infection increased with the close crowding of the sick and healthy, and with deficient ventilation; and that by sanitary improvements this danger might be obviated. There ought also to be a proper supervision of food (meat and milk) obtained from tuberculous cattle. Trades in relation to phthisis were also discussed.

Dr. Gibert, of Havre, followed with a paper entitled "De la distribution géographique de la Phthisie pulmonaire dans la ville de Havre: Rapports de la Phthisie avec la densité de la population, avec l'alcoolisme, et avec la misère." Dr. Gibert thought from his observations that overcrowding was a great factor in the etiology of phthisis: but that alcoholism played a much greater part, and poverty was also a factor. He showed on a map the distribution of phthisis in Havre.

Sir John Banks, of Dublin, who spoke in the discussion, mentioned that the sanitary improvements undertaken in Dublin had produced a great diminution of disease. Practice both in hospital and private had demonstrated this to him.

Mr. Weaver, of London, and Dr. B. O'Connor also took part in the discussion.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Aërial Roots of the Mangrove.

IN your note on a recent meeting of the Royal Botanic Society (July 30, p. 304), it is stated that the only explanation yet offered of the erect aërial roots of *Avicennia nivea* is that of detaining the *débris* and preventing the soil from being washed away. Without in any way detracting from the ingenuity and probability of Mr. Sowerby's explanation, it can hardly be admitted that this is the only explanation that has as yet been proposed. The peculiarities, both structural and physiological, of the mangrove-vegetation of the swamps of the Malayan Archipelago have been, during recent years, a special subject of investigation by botanists located at the Botanical Laboratory at Buitenzorg; the most recent and most important addition to its literature being comprised in the 22nd Heft of Luerssen and Haenlein's

"Bibliotheca Botanica," illustrated by eleven fine plates, by Herr G. Karsten. Herr Karsten points out that, in addition to the obvious mechanical function of these roots, serving as a supporting organ to attach the trees more firmly to the very loose soil in which they grow—this is especially observable in *Rhizophora mangle*—there is another important function performed by them, at least in a large number of the trees which make up the mangrove-vegetation, though I do not recollect that *Avicennia nivea* is especially mentioned. In the species examined by Karsten, these aërial roots possess very large intercellular spaces, which serve to promote the interchange of gases; and he considers it unquestionable that their chief function is to assist respiration. He therefore proposes for them the term "pneumatophores." It would be interesting to examine the structure of the trees at the Botanic Garden in this respect. All mangrove-trees also contain large quantities of tannin, which is probably serviceable in preventing rotting.

August 1. ALFRED W. BENNETT.

The Tasman Sea.

I SEND you the inclosed copy of a letter from the Secretary of the Admiralty, in case you should consider the matter of sufficient interest for notice in your columns.

A. LIVERSIDGE, Permanent Hon. Sec.
Australasian Association for the Advancement of Science.
The University, Sydney, July 4.

Admiralty, May 19, 1891.

SIR,—With reference to your letter of March 17, forwarding copy of a resolution passed by the Australasian Association for the Advancement of Science at the meeting held at Christchurch, New Zealand, that the name of Tasman Sea should be given to the sea between New Zealand and the islands of the north-west of New Zealand on the one hand and Australia and Tasmania on the other, I am commanded by my Lords Commissioners of the Admiralty to acquaint you that the name will be inserted in Admiralty charts and other publications.

I am, Sir,
Your obedient servant,
EVAN MACGREGOR.

To Prof. Liversidge, M.A., F.R.S.,
The University, Sydney.

Reduplication of Seasonal Growth.

LAST summer I sent you a note on the occurrence of apple-blossoms and the blossoms of the mountain ash in July. Before me now, as I write, is a simple but elegant bouquet containing a beautiful and fragrant corymb of the latter tree in full flower, side by side with one of the ripe scarlet fruit, which the blackbirds have begun to devour. These were cut from one and the self-same tree this morning at the top of my garden; while from an adjoining tree was gathered a twig carrying four pinnate leaves from which all the chlorophyll has disappeared; the phenomena which mark the beginning and the end of the season thus appearing side by side. These trees grows on the Upper Bagshot Sands, and I have no doubt that this reduplication of seasonal growth is due to the later rains developing some centres of flowering energy in the plant, which had remained dormant during the spring owing to deficiency of moisture and warmth.

Wellington College, Berks, August 17. A. IRVING.

Rain-gauges.

I HAVE been using the ordinary Symonds pattern rain-gauge, but find that the percentage of rain collected varies in proportion to the strength of the wind; when this is moderately strong, almost the whole of the rain passes across the top, striking and being retained by vertical surfaces only.

The present method of estimating the rainfall is far from being either correct or uniform, and I should like to ascertain if any gauge has been made with a correctly-proportioned inverted cone, which will collect and compensate for side drive; and, if so, what are the correct proportions. It would appear that either this, or a funnel mounted on gimbals and balanced to face the wind at the correct angle, must be the only correct method to ascertain the actual rainfall. The present apparatus would appear to be crude, untrustworthy, and incapable under any conditions in practice of giving results which are at all trustworthy. THOS. FLETCHER.

Grappenhall House, Grappenhall, near Warrington,
August 17.

THE BRITISH ASSOCIATION.

(FROM OUR CORRESPONDENT.)

CARDIFF, *Wednesday Morning*.

THE preparations of the Local Committee are now in an advanced state, and members of the Association are beginning to arrive in considerable numbers.

A change has been made in the position of the Reception Room, which is now located entirely in the Drill Hall, the Town Hall having had to be abandoned for that purpose owing to the impossibility of making adequate provision for the accommodation of the large number of guests expected. The Drill Hall is a large building, and has been divided into two parts by a screen, which also serves the purpose of a notice-board. On the entrance side are the offices for various purposes, post and excursions; and at a central oval counter, all other requirements relating to tickets, reserved seats, publications, and lodgings are attended to by a numerous staff of clerks.

Beyond the screen the hall has been fitted up as a drawing-room, and from this lead off smaller rooms for ladies, the press, and smokers. Separated from the drawing-room by a passage is the gun-room, from which everything has been removed, and tables laid down so as to convert it into a dining-room.

The President's address will be given in the Park Hall, this evening, and for the half-hour of waiting before the business commences Mr. T. E. Aylward will give a recital upon the fine organ in that hall. It is understood that Lord Bute, as Mayor of Cardiff, will at the outset welcome the Association in the name of the town of Cardiff.

The *conversazioni* will also be given in the same hall, and from 8.30 to 9 p.m., Lord Bute, as Chairman of the Local Committee, accompanied by Lady Bute, will receive the guests. At 9.30 p.m. an exhibition of views will be given by the lime-light, amongst them some fine ones, by Mr. M. Stirrup, of the limestone region of Languedoc. Amongst other attractions will be taking impressions of finger-tips, by Sergeant Randall (Mr. F. Galton's assistant); a model of the moon, shown by the Astronomer-Royal of Scotland; drawings in black and white of the Himalayas, by Col. Tanner; a collection of old local maps and atlases, by Mr. O. H. Jones; the Eisteddfod concert given at Swansea transmitted by telephone, by Mr. Gavey; and numerous other objects of interest.

Arrangements have been made for military and vocal music.

No alteration has been made in the Section rooms from that mentioned in our former article.

The publications of the Local Committee are ready for distribution, and comprise the local hand-book of 240 pages dealing with the archæology of the land of Morgan, the education, botany, geology, industries, and topography of Cardiff; the excursions-guide containing a map of the district on a scale of four miles to the inch, and two maps on a larger scale, one of the Bute Docks, and the other of the Barry Dock. The excursions number twenty in all—twelve are arranged for Saturday, the 22nd, and eight for Thursday, the 27th; and moderately detailed descriptions of each are given in the guide to the excursions.

The local programme, and the list of lodgings and hotels, are the remaining publications of the Committee. The total number of members of all classes who have taken out tickets for the meeting was, at 6 p.m. yesterday, over 900.

The President's address is as follows:—

Inaugural Address by William Huggins, Esq., D.C.L. (Oxon.), LL.D. (Cantab., Edin., et Dubl.), Ph.D. (Lugd. Bat.), F.R.S., F.R.A.S., Hon. F.R.S.E., &c., Correspondant de l'Institut de France, President.

It is now many years since this Association has done honour to the science of Astronomy in the selection of its President.

Since Sir George Airy occupied the chair in 1851, and the late Lord Wrottesley nine years later, in 1860, other sciences have been represented by the distinguished men who have presided over your meetings.

The very remarkable discoveries in our knowledge of the heavens which have taken place during this period of thirty years—one of amazing and ever-increasing activity in all branches of science—have not passed unnoticed in the addresses of your successive Presidents; still it seems to me fitting that I should speak to you to-night chiefly of those newer methods of astronomical research which have led to those discoveries, and which have become possible by the introduction since 1860 into the observatory of the spectroscope and the modern photographic plate.

In 1866 I had the honour of bringing before this Association, at one of the evening lectures, an account of the first-fruits of the novel and unexpected advances in our knowledge of the celestial bodies which followed rapidly upon Kirchhoff's original work on the solar spectrum and the interpretation of its lines.

Since that time a great harvest has been gathered in the same field by many reapers. Spectroscopic astronomy has become a distinct and acknowledged branch of the science, possessing a large literature of its own and observatories specially devoted to it. The more recent discovery of the gelatine dry plate has given a further great impetus to this modern side of astronomy, and has opened a pathway into the unknown of which even an enthusiast thirty years ago would scarcely have dared to dream.

In no science, perhaps, does the sober statement of the results which have been achieved appeal so strongly to the imagination, and make so evident the almost boundless powers of the mind of man. By means of its light alone to analyze the chemical nature of a far distant body; to be able to reason about its present state in relation to the past and future; to measure within an English mile or less per second the otherwise invisible motion which it may have towards or from us; to do more, to make even that which is darkness to our eyes light, and from vibrations which our organs of sight are powerless to perceive to evolve a revelation in which we see mirrored some of the stages through which the stars may pass in their slow evolutional progress—surely the record of such achievements, however poor the form of words in which they may be described, is worthy to be regarded as the scientific epic of the present century.

I do not purpose to attempt a survey of the progress of spectroscopic astronomy from its birth at Heidelberg in 1859, but to point out what we do know at present, as distinguished from what we do not know, of a few only of its more important problems, giving a prominent place, in accordance with the traditions of this chair, to the work of the last year or two.

In the spectroscope itself advances have been made by Lord Rayleigh by his discussion of the theory of the instrument, and by Prof. Rowland in the construction of concave gratings.

Lord Rayleigh has shown that there is not the necessary connection, sometimes supposed, between dispersion and resolving power, as besides the prism or grating other details of construction and of adjustment of a spectroscope must be taken into account.

The resolving power of the prismatic spectroscope is proportional to the length of path in the dispersive medium. For the heavy flint glass used in Lord Rayleigh's experiments, the thickness necessary to resolve the sodium lines, came out 1·02 cm. If this be taken as a unit, the resolving power of a prism of similar glass will be in the neighbourhood of the sodium lines equal to the number of centimetres of its thickness. In other parts of the spectrum the resolving power will vary inversely as the third power of the wave-length, so that it will be eight times as great in the violet as in the red. The resolving power of a spectroscope is therefore proportional to the total thickness of the dispersive material in use, irrespective of the number, the angles, or the setting of the separate prisms into which, for the sake of convenience, it may be distributed.

The resolving power of a grating depends upon the total number of lines on its surface, and the order of spectrum in use; about 1000 lines being necessary to resolve the sodium lines in the first spectrum.

As it is often of importance in the record of observations to state the efficiency of the spectroscope with which they were made, Prof. Schuster has proposed the use of a unit of purity as well as of resolving power, for the full resolving power of a spectroscope is realized in practice only when a sufficiently narrow slit is used. The unit of purity also is to stand for the separation of two lines differing by one-thousandth of their own wave-length; about the separation of the sodium pair at D.

A further limitation may come in from the physiological fact that, as Lord Rayleigh has pointed out, the eye, when its full aperture is used, is not a perfect instrument. If we wish to realize the full resolving power of a spectroscope, therefore, the emergent beam must not be larger than about one-third of the opening of the pupil.

Up to the present time the standard of reference for nearly all spectroscopic work continues to be Ångström's map of the solar spectrum, and his scale based upon his original determinations of absolute wave-length. It is well known, as was pointed out by Thalén in his work on the spectrum of iron, in 1884, that Ångström's figures are slightly too small, in consequence of an error existing in a standard metre used by him. The corrections for this have been introduced into the tables of the wave-lengths of terrestrial spectra collected and revised by a Committee of this Association from 1885 to 1887. Last year the Committee adedd a table of corrections to Rowland's scale.

The inconvenience caused by a change of standard scale is, for a time at least, considerable; but there is little doubt that in the near future Rowland's photographic map of the solar spectrum, and his scale based on the determinations of absolute wave-length by Pierce and Bell, or the Potsdam scale based on original determinations by Müller and Kempf, which differs very slightly from it, will come to be exclusively adopted.

The great accuracy of Rowland's photographic map is due chiefly to the introduction by him of concave gratings, and of a method for their use by which the problem of the determination of relative wave-lengths is simplified to measures of coincidences of the lines in different spectra by a micrometer.

The concave grating and its peculiar mounting, in which no lenses or telescope are needed, and in which all the spectra are in focus together, formed a new departure of great importance in the measurement of spectral lines. The valuable method of photographic sensitizers for different parts of the spectrum has enabled Prof. Rowland to include in his map the whole visible solar spectrum, as well as the ultra-violet portion as far as it can get through our atmosphere. Some recent photographs of the solar spectrum, which include A, by Mr. George Higgs, are of great technical beauty.

During the past year the results of three independent researches have appeared, in which the special object of the observers has been to distinguish the lines which are due to our atmosphere from those which are truly solar—the maps of M. Thollon, which, owing to his lamented death just before their final completion, have assumed the character of a memorial of him; maps by Dr. Becker; and sets of photographs of a high and a low sun by Mr. McClean.

At the meeting of this Association in Bath, M. Janssen gave an account of his own researches on the terrestrial lines of the solar spectrum which owe their origin to the oxygen of our atmosphere. He discovered the remarkable fact that, while one class of bands varies as the density of the gas, other diffuse bands vary as the square of the density. These observations are in accordance with the work of Egoroff and of Olszewski, and of Liveing and Dewar on condensed oxygen. In some recent experiments Olszewski, with a layer of liquid oxygen 30 millimetres thick, saw, as well as four other bands, the band coincident with Fraunhofer's A; a remarkable instance of the persistence of absorption through a great range of temperature. The light which passed through the liquid oxygen had a light blue colour resembling that of the sky.

Of not less interest are the experiments of Knut Ångström, which show that the carbonic acid and aqueous vapour of the atmosphere reveal their presence by dark bands in the invisible infra-red region, at the positions of bands of emission of these substances.

It is now some thirty years since the spectroscope gave us for the first time certain knowledge of the nature of the heavenly bodies, and revealed the fundamental fact that terrestrial matter is not peculiar to the solar system, but is common to all the stars which are visible to us.

In the case of a star such as Capella, which has a spectrum almost identical with that of the sun, we feel justified in concluding that the matter of which it is built up is similar, and that its temperature is also high, and not very different from the solar temperature. The task of analyzing the stars and nebulæ becomes, however, one of very great difficulty when we have to do with spectra differing from the solar type. We are thrown back upon the laboratory for the information necessary to enable us to interpret the indications of the spectroscope as to the chemical nature, the density and pressure, and the temperature of the celestial masses.

What the spectroscope immediately reveals to us are the waves which were set up in the ether filling all interstellar space, years or hundreds of years ago, by the motions of the molecules of the celestial substances. As a rule, it is only when a body is gaseous and sufficiently hot that the motions within its molecules can produce bright lines and a corresponding absorption. The spectra of the heavenly bodies are, indeed, to a great extent absorption spectra, but we have usually to study them through the corresponding emission spectra of bodies brought into the gaseous form and rendered luminous by means of flames or of electric discharges. In both cases, unfortunately, as has been shown recently by Profs. Liveing and Dewar, Wüllner, E. Wiedemann, and others, there appears to be no certain direct relation between the luminous radiation as shown in the spectroscope and the temperature of the flame, or of the gaseous contents of the vacuum tube—that is, in the usual sense of the term as applied to the mean motion of all the molecules. In both cases, the vibratory motions within the molecules to which their luminosity is due are almost always much greater than would be produced by encounters of molecules having motions of translation no greater than the average motions which characterize the temperature of the gases as a whole. The temperature of a vacuum tube through which an electric discharge is taking place may be low, as shown by a thermometer, quite apart from the consideration of the extreme smallness of the mass of gas, but the vibrations of the luminous molecules must be violent in whatever way we suppose them to be set up by the discharge; if we take Schuster's view that comparatively few molecules are carrying the discharge, and that it is to the fierce encounters of these alone that the luminosity is due, then if all the molecules had similar motions, the temperature of the gas would be very high.

So in flames where chemical changes are in progress, the vibratory motions of the molecules which are luminous may be, in connection with the energy set free in these changes, very different from those corresponding to the mean temperature of the flame.

Under the ordinary conditions of terrestrial experiments, therefore, the temperature or the mean *vis viva* of the molecules may have no direct relation to the total radiation, which, on the other hand, is the sum of the radiation due to each luminous molecule.

These phenomena have recently been discussed by Ebert from the standpoint of the electro-magnetic theory of light.

Very great caution is therefore called for when we attempt to reason by the aid of laboratory experiments to the temperature of the heavenly bodies from their radiation, especially on the reasonable assumption that in them the luminosity is not ordinarily associated with chemical changes or with electrical discharges; but is due to a simple glowing from the ultimate conversion into molecular motion of the gravitational energy of shrinkage.

In a recent paper Stas maintains that electric spectra are to be regarded as distinct from flame spectra, and from researches of his own, that the pairs of lines of the sodium spectrum other than D are produced only by disruptive electric discharges. As these pairs of lines are found reversed in the solar spectrum, he concludes that the sun's radiation is due mainly to electric discharges. But Wolf and Diacon, and later, Watts, observed the other pairs of lines of the sodium spectrum when the vapour was raised above the ordinary temperature of the Bunsen flame. Recently, Liveing and Dewar saw easily, besides D, the citron and green pairs, and sometimes the blue pair and the orange pair, when hydrogen charged with sodium vapour was burning at different pressures in oxygen. In the case of sodium vapour, therefore, and presumably in all other vapours and gases, it is a matter of indifference whether the necessary vibratory motion of the molecules is produced by electric discharges or by flames. The presence of lines in the solar spectrum which we can only produce electrically, is an indication, however, as Stas points out, of the high temperature of the sun.

We must not forget that the light from the heavenly bodies may consist of the combined radiations of different layers of gas at different temperatures, and possibly be further complicated to an unknown extent by the absorption of cooler portions of gas outside.

Not less caution is needed if we endeavour to argue from the broadening of lines and the coming in of a continuous spectrum as to the relative pressure of the gas in the celestial atmospheres. On the one hand, it cannot be gainsaid that in the laboratory the widening of the lines in a Plücker's tube follows upon increasing the density of the residue of hydrogen in the tube, when the vibrations are more frequently disturbed by fresh encounters, and that a broadening of the sodium lines in a flame at ordinary pressure is produced by an increase of the quantity of sodium in the flame; but it is doubtful if pressure, as distinguished from quantity, does produce an increase of the breadth of the lines. An individual molecule of sodium will be sensibly in the same condition, considering the relatively enormous number of the molecules of the other gases, whether the flame is scantily or copiously fed with the sodium salt. With a small quantity of sodium vapour the intensity will be feeble except near the maximum of the lines; when, however, the quantity is increased, the comparative transparency on the sides of the maximum will allow the light from the additional molecules met with in the path of the visual ray to strengthen the radiation of the molecules farther back, and so increase the breadth of the lines.

In a gaseous mixture it is found, as a rule, that at the same pressure or temperature, as the encounters with similar molecules become fewer, the spectral lines will be affected as if the body were observed under conditions of reduced quantity or temperature.

In their recent investigation of the spectroscopic behaviour of flames under various pressures up to forty atmospheres, Profs. Liveing and Dewar have come to the conclusion that, though the prominent feature of the light emitted by flames at high pressure appears to be a strong continuous spectrum, there is not the slightest indication that this continuous spectrum is produced by the broadening of the lines of the same gases at low pressure. On the contrary, photometric observations of the brightness of the continuous spectrum, as the pressure is varied, show that it is mainly produced by the mutual action of the molecules of a gas. Experiments on the sodium spectrum were carried up to a pressure of forty atmospheres without producing any definite effect on the width of the lines which could be ascribed to the pressure. In a similar way the lines of the spectrum of water showed no signs of expansion up to twelve atmospheres; though more intense than at ordinary pressure, they remained narrow and clearly defined.

It follows, therefore, that a continuous spectrum cannot be considered, when taken alone, as a sure indication of matter in the liquid or the solid state. Not only, as in the experiments already mentioned, such a spectrum may be due to gas when under pressure, but, as Maxwell pointed out, if the thickness of a medium, such as sodium vapour, which radiates and absorbs different kinds of light, be very great, and the temperature high, the light emitted will be of exactly the same composition as that emitted by lamp-black at the same temperature, for the radiations which are feebly emitted will be also feebly absorbed, and can reach the surface from immense depths. Schuster has shown that oxygen, even in a partially exhausted tube, can give a continuous spectrum when excited by a feeble electric discharge.

Compound bodies are usually distinguished by a banded spectrum; but, on the other hand, such a spectrum does not necessarily show the presence of compounds—that is, of molecules containing different kinds of atoms—but simply of a more complex molecule, which may be made up of similar atoms, and be, therefore, an allotropic condition of the same body. In some cases—for example, in the diffuse bands of the absorption spectrum of oxygen—the bands may have an intensity proportional to the square of the density of the gas, and may be due either to the formation of more complex molecules of the gas with increase of pressure, or it may be to the constraint to which the molecules are subject during their encounter with one another.

It may be thought that at least in the coincidences of bright lines we are on the solid ground of certainty, since the length of the waves set up in the ether by a molecule, say of hydrogen, is the most fixed and absolutely permanent quantity in nature, and is so of physical necessity, for with any alteration the molecule would cease to be hydrogen.

Such would be the case if the coincidence were certain; but an absolute coincidence can be only a matter of greater or less probability, depending on the resolving power employed, on the number of the lines which correspond, and on their characters. When the coincidences are very numerous, as in the case of iron and the solar spectrum, or the lines are characteristically grouped, as in the case of hydrogen and the solar spectrum, we may regard the coincidence as certain; but the progress of science has been greatly retarded by resting important conclusions upon the apparent coincidence of single lines, in spectroscopes of very small resolving power. In such cases, unless other reasons supporting the coincidence are present, the probability of a real coincidence is almost too small to be of any importance, especially in the case of a heavenly body which may have a motion of approach or of recession of unknown amount.

But even here we are met by the confusion introduced by multiple spectra, corresponding to different molecular groupings of the same substance; and, further, to the influence of substances in vapour upon each other; for when several gases are present together, the phenomena of radiation and reversal by absorption are by no means the same as if the gases were free from each other's influence, and especially is this the case when they are illuminated by an electric discharge.

I have said as much as time will permit, and I think indeed sufficient, to show that it is only by the laborious and slow process of most cautious observation that the foundations of the science of celestial physics can be surely laid. We are at present in a time of transition, when the earlier, and, in the nature of things, less precise, observations are giving place to work of an order of accuracy much greater than was formerly considered attainable with objects of such small brightness as the stars.

The accuracy of the earlier determinations of the spectra of the terrestrial elements are in most cases insufficient for modern work on the stars as well as on the sun. They fall much below the scale adopted in Rowland's map of the sun, as well as below the degree of accuracy attained at Potsdam by photography in a part of the spectrum for the brighter stars. Increase of resolving power very frequently breaks up into groups, in the spectra of the sun and stars, the lines which had been regarded as single, and their supposed coincidences with terrestrial lines fall to the ground. For this reason many of the early conclusions, based on observation as good as it was possible to make at the time with the less powerful spectroscopes then in use, may not be found to be maintained under the much greater resolving power of modern instruments.

The spectroscope has failed as yet to interpret for us the remarkable spectrum of the Aurora Borealis. Undoubtedly in this phenomenon portions of our atmosphere are lighted up by electric discharges: we should expect, therefore, to recognize the spectra of the gases known to be present in it. As yet we have not been able to obtain similar spectra from these gases artificially, and especially we do not know the origin of the principal line in the green, which often appears alone, and may have, therefore, an origin independent of that of the other lines. Recently the suggestion has been made that the aurora is a phenomenon produced by the dust of meteors and falling stars, and that near positions of certain auroral lines or flutings of manganese, lead, barium, thallium, iron, &c., are sufficient to justify us in regarding meteoric dust in the atmosphere as the origin of the auroral spectrum. Liveing and Dewar have made a conclusive research on this point, by availing themselves of the dust of excessive minuteness thrown off from the surface of electrodes of various metals and meteorites by a disruptive discharge, and carried forward into the tube of observation by a more or less rapid current of air or other gas. These experiments prove that metallic dust, however fine, suspended in a gas will not act like gaseous matter in becoming luminous with its characteristic spectrum in an electric discharge similar to that of the aurora. Prof. Schuster has suggested that the principal line may be due to some very light gas which is present in too small a proportion to be detected by chemical analysis or even by the spectroscope in the presence of the other gases near the earth, but which at the height of the auroral discharges is in a sufficiently greater relative proportion to give a spectrum. Lemström, indeed, states that he saw this line in the silent discharge of a Holtz machine on a mountain in Lapland. The lines may not have been obtained in our laboratories from the atmospheric gases on account of the difficulty of reproducing in tubes with sufficient nearness the conditions under which the auroral discharges take place.

In the spectra of comets the spectroscope has shown the presence of carbon presumably in combination with hydrogen, and also sometimes with nitrogen; and in the case of comets approaching very near the sun, the lines of sodium, and other lines which have been supposed to belong to iron. Though the researches of Prof. H. A. Newton and of Prof. Schiaparelli leave no doubt of the close connection of comets with corresponding periodic meteor swarms, and therefore of the probable identity of cometary matter with that of meteorites, with which the spectroscopic evidence agrees, it would be perhaps unwise at present to attempt to define too precisely the exact condition of the matter which forms the nucleus of the comet. In any case the part of the light of the comet which is not reflected solar light can scarcely be attributed to a high temperature produced by the clashing of separate meteoric stones set up within the nucleus by the sun's disturbing force. We must look rather to disruptive electric discharges, produced probably by processes of evaporation due to increased solar heat, which would be amply sufficient to set free portions of the occluded gases into the vacuum of space. May it be that these discharges are assisted, and indeed possibly increased, by the recently discovered action of the ultra-violet part of the sun's light? Lenard and Wolfe have shown that ultra-violet light can produce a discharge from a negatively electrified piece of metal, while Hallwachs and Righi have shown further that ultra-violet light can even charge positively an unelectrified piece of metal. Similar actions on cometary matter, unscreened as it is by an absorptive atmosphere, at least of any noticeable extent, may well be powerful when a comet approaches the sun, and help to explain an electrified condition of the evaporated matter which would possibly bring it under the sun's repulsive action. We shall have to return to this point in speaking of the solar corona.

A very great advance has been made in our knowledge of the constitution of the sun by the recent work at the Johns Hopkins University by means of photography and concave gratings, in comparing the solar spectrum, under great resolving power, directly with the spectra of the terrestrial elements. Prof. Rowland has shown that the lines of thirty-six terrestrial elements at least are certainly present in the solar spectrum, while eight others are doubtful. Fifteen elements, including nitrogen as it shows itself under an electric discharge in a vacuum tube, have not been found in the solar spectrum. Some ten other elements, inclusive of oxygen, have not yet been compared with the sun's spectrum.

Rowland remarks that of the fifteen elements named as not found in the sun, many are so classed because they have few strong lines, or none at all, in the limit of the solar spectrum as compared by him with the arc. Boron has only two strong lines. The lines of bismuth are compound and too diffuse. Therefore even in the case of these fifteen elements there is little evidence that they are really absent from the sun.

It follows that if the whole earth were heated to the temperature of the sun, its spectrum would resemble very closely the solar spectrum.

Rowland has not found any lines common to several elements, and in the case of some accidental coincidences, more accurate investigation reveals some slight difference of wave-length or a common impurity. Further, the relative strength of the lines in the solar spectrum is generally, with a few exceptions, the same as that in the electric arc, so that Rowland considers that his experiments show "very little evidence" of the breaking up of the terrestrial elements in the sun.

Stas in a recent paper gives the final results of eleven years of research on the chemical elements in a state of purity, and on the possibility of decomposing them by the physical and chemical forces at our disposal. His experiments on calcium, strontium, lithium, magnesium, silver, sodium, and thallium, show that these substances retain their individuality under all conditions, and are unalterable by any forces that we can bring to bear upon them.

Prof. Rowland looks to the solar lines which are unaccounted for as a means of enabling him to discover such new terrestrial elements as still lurk in rare minerals and earths, by confronting their spectra directly with that of the sun. He has already resolved yttrium spectroscopically into three components, and actually into two. The comparison of the results of this independent analytical method with the remarkable but different conclusions to which M. Lecoq de Boisbaudran and Mr. Crookes have been led respectively, from spectroscopic observation of these bodies when glowing under molecular bombardment in a vacuum tube, will be awaited with much interest. It is worthy of remark that, as our knowledge of the spectrum of hydrogen in its complete form came to us from the stars, it is now from the sun that chemistry is probably about to be enriched by the discovery of new elements.

In a discussion in the Bakerian Lecture for 1885 of what we knew up to that time of the sun's corona, I was led to the conclusion that the corona is essentially a phenomenon similar in the cause of its formation to the tails of comets—namely, that it consists for the most part probably of matter going from the sun under the action of a force, possibly electrical, which varies as the surface, and can therefore in the case of highly attenuated matter easily master the force of gravity even near the sun. Though many of the coronal particles may return to the sun, those which form the long rays or streamers do not return; they separate and soon become too diffused to be any longer visible, and may well go to furnish the matter of the zodiacal light, which otherwise has not received a satisfactory explanation. And further, if such a force exist at the sun, the changes of terrestrial magnetism may be due to direct electric action, as the earth moves through lines of inductive force.

These conclusions appear to be in accordance broadly with the lines along which thought has been directed by the results of subsequent eclipses. Prof. Schuster takes an essentially similar view, and suggests that there may be a direct electric connection between the sun and the planets. He asks further whether the sun may not act like a magnet in consequence of its revolution about its axis. Prof. Bigelow has recently treated the coronal forms by the theory of spherical harmonics, on the supposition that we see phenomena similar to those of free electricity, the rays being lines of force, and the coronal matter discharged from the sun, or at least arranged or controlled by these forces. At the extremities of the streams for some reasons the repulsive power may be lost, and gravitation set in, bringing the matter back to the sun. The matter which does leave the sun is persistently transported to the equatorial plane of the corona; in fact, the zodiacal light may be the accumulation at great distances from the sun along this equator of such like material. Photographs on a larger scale will be desirable for the full development of the conclusions which may follow from this study of the curved forms of the coronal structure. Prof. Schaeberle, however, considers that the coronal phenomena may be satisfactorily accounted for on the supposition that the corona is formed of streams of matter ejected mainly from the spot zones with great initial velocities, but smaller than 382 miles per second. Further that the different types of the corona are due to the effects of perspective on the streams from the earth's place at the time relatively to the plane of the solar equator.

Of the physical and the chemical nature of the coronal matter we know very little. Schuster concludes, from an examination of the eclipses of 1882, 1883, and 1886, that the continuous spectrum of the corona has the maximum of actinic intensity displaced considerably towards the red when compared with the spectrum of the sun, which shows that it can only be due in small part to solar light scattered by small particles. The lines of calcium and of hydrogen do not appear to form part of the normal spectrum of the corona. The green coronal line has no known representative in terrestrial substances, nor has Schuster been able to recognize any of our elements in the other lines of the corona.

The spectra of the stars are almost infinitely diversified, yet they can be arranged with some exceptions in a series in which the adjacent spectra, especially in the photographic region, are scarcely distinguishable, passing from the bluish-white stars like Sirius, through stars more or less solar in character, to stars with banded spectra, which divide themselves into two apparently independent groups, according as the stronger edge of the bands is towards the red or the blue. In such an arrangement the sun's place is towards the middle of the series.

At present a difference of opinion exists as to the direction in the series in which evolution is proceeding, whether by further condensation white stars pass into the orange and red stages, or whether these more coloured stars are younger and will become white by increasing age. The latter view was suggested by Johnstone Stoney in 1867.

About ten years ago Ritter in a series of papers discussed the behaviour of gaseous masses during condensation, and the probable resulting constitution of the heavenly bodies. According to him, a star passes through the orange and red stages twice: first during a comparatively short period of increasing temperature, which culminates in the white stage, and a second time during a more prolonged stage of gradual cooling. He suggested that the two groups of banded stars may correspond to these different periods: the young stars being those in which the stronger edge of the dark band is towards the blue, the other banded stars, which are relatively less luminous and few in number, being those which are approaching extinction through age.

Recently a similar evolutional order has been suggested, which is based upon the hypothesis that the nebulæ and stars consist of colliding meteoric stones in different stages of condensation.

More recently the view has been put forward that the diversified spectra of the stars do not represent the stages of an evolutional progress, but are due for the most part to differences of original constitution.

The few minutes which can be given to this part of the address are insufficient for a discussion of these different views. I purpose, therefore, to state briefly, and with reserve, as the subject is obscure, some of the considerations from the characters of their spectra which appeared to me to be in favour of the evolutional order in which I arranged the stars from their photographic spectra in 1879. This order is essentially the same as Vogel had previously proposed in his classification of the stars in 1874, in which the white stars, which are most numerous, represent the early adult and most persistent stage of stellar life; the solar condition that of full maturity and of commencing age; while in the orange and red stars with banded spectra we see the setting in and advance of old age. But this statement must be taken broadly, and not as asserting that all stars, however different in mass and possibly to some small extent in original constitution, exhibit one invariable succession of spectra.

In the spectra of the white stars the dark metallic lines are relatively inconspicuous, and occasionally absent, at the same time that the dark lines of hydrogen are usually strong, and more or less broad, upon a continuous spectrum, which is remarkable for its brilliancy at the blue end. In some of these stars the hydrogen and some other lines are bright, and sometimes variable.

As the greater or less prominence of the hydrogen lines, dark or bright, is characteristic of the white stars as a class, and diminishes gradually with the incoming and increase in strength of the other lines, we are probably justified in regarding it as due to some conditions which occur naturally during the progress of stellar life, and not to a peculiarity of original constitution.

To produce a strong absorption-spectrum a substance must be at the particular temperature at which it is notably absorptive; and, further, this temperature must be sufficiently below that of the region behind from which the light comes for the gas to appear, so far as its special rays are concerned, as darkness upon it. Considering the high temperature to which hydrogen must be raised before it can show its characteristic emission and absorption, we shall probably be right in attributing the relative feebleness or absence of the other lines, not to the paucity of the metallic vapours, but rather to their being so hot relatively to the substances behind them as to show feebly, if at all, by reversion. Such a state of things would more probably be found, it seems to me, in conditions anterior to the solar stage. A considerable cooling of the sun would probably give rise to banded spectra due to compounds, or to more complex molecules, which might form near the condensing points of the vapours.

The sun and stars are generally regarded as consisting of glowing vapours surrounded by a photosphere where condensation is taking place, the temperature of the photospheric layer from which the greater part of the radiation comes being constantly renewed from the hotter matter within.

At the surface the convection currents would be strong, producing a considerable commotion, by which the different gases would be mixed and not allowed to retain the inequality of proportions at different levels due to their vapour densities.

Now the conditions of the radiating photosphere and those of the gases above it, on which the character of the spectrum of a star depends, will be determined, not alone by temperature, but also by the force of gravity in these regions; this force will be fixed by the star's mass and its stage of condensation, and will become greater as the star continues to condense.

In the case of the sun the force of gravity has already become so great at the surface that the decrease of the density of the gases must be extremely rapid, passing in the space of a few miles from atmospheric pressure to a density infinitesimally small; consequently the temperature-gradient at the surface, if determined solely by expansion, must be extremely rapid. The gases here, however, are exposed to the fierce radiation of the sun, and unless wholly transparent would take up heat, especially if any solid or liquid particles were present from condensation or convection currents.

From these causes, within a very small extent of space at the surface of the sun, all bodies with which we are acquainted should fall to a condition in which the extremely tenuous gas could no longer give a visible spectrum. The insignificance of the angle subtended by this space as seen from the earth should cause the boundary of the solar atmosphere to appear defined. If the boundary which we see be that of the sun proper, the matter above it will have to be regarded as in an essentially dynamical condition—an assemblage, so to speak, of gaseous projectiles for the most part falling back upon the sun after a greater or less range of flight. But in any case it is within a space of relatively small extent in the sun, and probably in the other solar stars, that the reversion which is manifested by dark lines is to be regarded as taking place.

Passing backward in the star's life, we should find a gradual weakening of gravity at the surface, a reduction of the temperature-gradient so far as it was determined by expansion, and convection currents of less violence producing less interference with the proportional quantities of gases due to their vapour densities, while the effects of eruptions would be more extensive.

At last we might come to a state of things in which, if the star were hot enough, only hydrogen might be sufficiently cool relatively to the radiation behind to produce a strong absorption. The lower vapours would be protected, and might continue to be relatively too hot for their lines to appear very dark upon the continuous spectrum; besides, their lines might be possibly to some extent effaced by the coming in under such conditions in the vapours themselves of a continuous spectrum.

In such a star the light radiated towards the upper part of the atmosphere may have come from portions lower down of the atmosphere itself, or at least from parts not greatly hotter. There may be no such great difference of temperature of the low and less low portions of the star's atmosphere as to make the darkening effect of absorption of the protected metallic vapours to prevail over the illuminating effect of their emission.

It is only by a vibratory motion corresponding to a very high temperature that the bright lines of the first spectrum of hydrogen can be brought out, and by the equivalence of absorbing and emitting power that the corresponding spectrum of absorption should be produced; yet for a strong absorption to show itself, the hydrogen must be cool relatively to the source of radiation behind it, whether this be condensed particles or gas. Such conditions, it seems to me, should occur in the earlier rather than in the more advanced stages of condensation.

The subject is obscure, and we may go wrong in our mode of conceiving of the probable progress of events, but there can be no doubt that in one remarkable instance the white-star spectrum is associated with an early stage of condensation.

Sirius is one of the most conspicuous examples of one type of this class of stars. Photometric observations combined with its ascertained parallax show that this star emits from forty to sixty times the light of our sun, even to the eye, which is insensible to ultra-violet light, in which Sirius is very rich, while we learn from the motion of its companion that its mass is not much more than double that of our sun. It follows that, unless we attribute to this star an improbably great emissive power, it must be of immense size, and in a much more diffuse and therefore an earlier condition than our sun; though probably at a later stage than those white stars in which the hydrogen lines are bright.

A direct determination of the relative temperature of the photospheres of the stars might possibly be obtained in some cases from the relative position of maximum radiation of their continuous spectra. Langley has shown that through the whole range of temperature on which we can experiment, and presumably at temperatures beyond, the maximum of radiation-power in solid bodies gradually shifts upwards in the spectrum from the infra-red through the red and orange, and that in the sun it has reached the blue.

The defined character, as a rule, of the stellar lines of absorption suggests that the vapours producing them do not at the same time exert any strong power of general absorption. Consequently, we should probably not go far wrong, when the photosphere consists of liquid or solid particles, if we could compare select parts of the continuous spectrum between the stronger lines, or where they are fewest. It is obvious that, if extended portions of different stellar spectra were compared, their true relation would be obscured by the line-absorption.

The increase of temperature, as shown by the rise in the spectrum of the maximum of radiation, may not always be accompanied by a corresponding greater brightness of a star as estimated by the eye, which is an extremely imperfect photometric instrument. Not only is the eye blind to large regions of radiation, but even for the small range of light that we can see the visual effect varies enormously with its colour. According to Prof. Langley, the same amount of energy which just enables us to perceive light in the crimson at A would in the green produce a visual effect 100,000 times greater. In the violet the proportional effect would be 1600, in the blue 62,000, in the yellow 28,000, in the orange 14,000, and in the red 1200. Captain Abney's recent experiments make the sensitiveness of the eye for the green near F to be 750 times greater than for the red about C. It is for this reason, at least in part, that I suggested in 1864, and have since shown by direct observation, that the spectrum of the nebula in Andromeda, and presumably of similar nebulæ, is, in appearance, only wanting in the red.

The stage at which the maximum radiation is in the green, corresponding to the eye's greatest sensitiveness, would be that in which it could be most favourably measured by eye-photometry. As the maximum rose into the violet and beyond, the star would increase in visual brightness, but not in proportion to the increase of energy radiated by it.

The brightness of a star would be affected by the nature of the substance by which the light was chiefly emitted. In the laboratory, solid carbon exhibits the highest emissive power. A stellar stage in which radiation comes, to a large extent, from a photosphere of the solid particles of this substance, would be favourable for great brilliancy. Though the stars are built up of matter essentially similar to that of the sun, it does not follow that the proportion of the different elements is everywhere the same. It may be that the substances condensed in the photospheres of different stars may differ in their emissive powers, but probably not to a great extent.

All the heavenly bodies are seen by us through the tinted medium of our atmosphere. According to Langley, the solar stage of stars is not really yellow, but, even as gauged by our imperfect eyes, would appear bluish-white if we could free ourselves from the deceptive influences of our surroundings.

From these considerations it follows that we can scarcely infer the evolutional stages of the stars from a simple comparison of their eye-magnitudes. We should expect the white stars to be, as a class, less dense than the stars in the solar stage. As great mass might bring in the solar type of spectrum at a relatively earlier time, some of the brightest of these stars may be very massive, and brighter than the sun—for example, the brilliant star Arcturus. For these reasons the solar stars should not only be dense than the white stars, but perhaps, as a class, surpass them in mass and eye-brightness.

It has been shown by Lane that, so long as a condensing gaseous mass remains subject to the laws of a purely gaseous body, its temperature will continue to rise.

The greater or less breadth of the lines of absorption of hydrogen in the white stars may be due to variations of the depth of the hydrogen in the line of sight, arising from the causes which have been discussed. At the sides of the lines the absorption and emission are feebler than in the middle, and would come out more strongly with a greater thickness of gas.

The diversities among the white stars are nearly as numerous as the individuals of the class. Time does not permit me to do more than to record that, in addition to the three sub-classes into which they have been divided by Vogel, Scheiner has recently investigated minor differences as suggested by the character of the third line of hydrogen near G. He has pointed out, too, that so far as his observations go the white stars in the constellation of Orion stand alone, with the exception of Algol, in possessing a dark line in the blue which has apparently the same position as a bright line in the great nebula of the same constellation; and Pickering finds in his photographs of the spectra of these stars dark lines corresponding to the principal lines of the bright-line stars, and the planetary nebulæ with the exception of the chief nebular line. The association of white stars with nebular matter in Orion, in the Pleiades, in the region of the Milky Way, and in other parts of the heavens, may be regarded as falling in with the view that I have taken.

In the stars possibly further removed from the white class than our sun, belonging to the first division of Vogel's third class, which are distinguished by absorption bands with their stronger edge towards the blue, the hydrogen lines are narrower than in the solar spectrum. In these stars the density-gradient is probably still more rapid, the depth of hydrogen may be less, and possibly the hydrogen molecules may be affected by a larger number of encounters with dissimilar molecules. In some red stars with dark hydrocarbon bands, the hydrogen lines have not been certainly observed; if they are really absent, it may be because the temperature has fallen below the point at which hydrogen can exert its characteristic absorption; besides, some hydrogen will have united with the carbon. The coming in of the hydrocarbon bands may indicate a later evolutional stage, but the temperature may still be high, as acetylene can exist in the electric arc.

A number of small stars more or less similar to those which are known by the names of their discoverers, Wolf and Rayet, have been found by Pickering in his photographs. These are remarkable for several brilliant groups of bright lines, including frequently the hydrogen lines and the line D_3, upon a continuous spectrum strong in blue and violet rays, in which are also dark lines of absorption. As some of the bright groups appear in his photographs to agree in position with corresponding bright lines in the planetary nebulæ, Pickering suggests that these stars should be placed in one class with them, but the brightest nebular line is absent from these stars. The simplest conception of their nature would be that each star is surrounded by a nebula, the bright groups being due to the gaseous matter outside the star. Mr. Roberts, however, has not been able to bring out any indication of nebulosity by prolonged exposure. The remarkable star η Argûs may belong to this class of the heavenly bodies.

In the nebulæ, the elder Herschel saw portions of the fiery mist or "shining fluid" out of which the heavens and the earth had been slowly fashioned. For a time this view of the nebulæ gave place to that which regarded them as external galaxies, cosmical "sand-heaps," too remote to be resolved into separate stars; though indeed, in 1858, Mr. Herbert Spencer showed that the observations of nebulæ up to that time were really in favour of an evolutional progress.

In 1864, I brought the spectroscope to bear upon them; the bright lines which flashed upon the eye showed the source of the light to be glowing gas, and so restored these bodies to what is probably their true place, as an early stage of sidereal life.

At that early time our knowledge of stellar spectra was small. For this reason partly, and probably also under the undue influence of theological opinions then widely prevalent, I unwisely wrote in my original paper in 1864, "that in these objects we no longer have to do with a special modification of our own type of sun, but find ourselves in presence of objects possessing a distinct and peculiar plan of structure." Two years later, however, in a lecture before this Association, I took a truer position. "Our views of the universe," I said, "are undergoing important changes; let us wait for more facts, with minds unfettered by any dogmatic theory, and therefore free to receive the teaching, whatever it may be, of new observations."

Let us turn aside for a moment from the nebulæ in the sky to the conclusions to which philosophers had been irresistibly led by a consideration of the features of the solar system. We have before us in the sun and planets obviously not a haphazard aggregation of bodies, but a system resting upon a multitude of relations pointing to a common physical cause. From these considerations Kant and Laplace formulated the nebular hypothesis, resting it on gravitation alone, for at that time the science of the conservation of energy was practically unknown. These philosophers showed how, on the supposition that the space now occupied by the solar system was once filled by a vaporous mass, the formation of the sun and planets could be reasonably accounted for.

By a totally different method of reasoning, modern science traces the solar system backward step by step to a similar state of things at the beginning. According to Helmholtz, the sun's heat is maintained by the contraction of his mass, at the rate of about 220 feet a year. Whether at the present time the sun is getting hotter or colder we do not certainly know. We can reason back to the time when the sun was sufficiently expanded to fill the whole space occupied by the solar system, and was reduced to a great glowing nebula. Though man's life, the life of the race perhaps, is too short to give us direct evidence of any distinct stages of so august a process, still the probability is great that the nebular hypothesis, especially in the more precise form given to it by Roche, does represent broadly, notwithstanding some difficulties, the succession of events through which the sun and planets have passed.

The nebular hypothesis of Laplace requires a rotating mass of fluid which at successive epochs became unstable from excess of motion, and left behind rings, or more probably perhaps lumps, of matter from the equatorial regions.

The difficulties to which I have referred have suggested to some thinkers a different view of things, according to which it is not necessary to suppose that one part of the system gravitationally supports another. The whole may consist of a congeries of discrete bodies even if these bodies be the ultimate molecules of matter. The planets may have been formed by the gradual accretion of such discrete bodies. On the view that the material of the condensing solar system consisted of separate particles or masses, we have no longer the fluid pressure which is an essential part of Laplace's theory. Faye, in his theory of evolution from meteorites, has to throw over this fundamental idea of the nebular hypothesis, and he formulates instead a different succession of events, in which the outer planets were formed last; a theory which has difficulties of its own.

Prof. George Darwin has recently shown, from an investigation of the mechanical conditions of a swarm of meteorites, that on certain assumptions a meteoric swarm might behave as a coarse gas, and in this way bring back the fluid pressure exercised by one part of the system on the other, which is required by Laplace's theory. One chief assumption consists in supposing that such inelastic bodies as meteoric stones might attain the effective elasticity of a high order which is necessary to the theory through the sudden volatilization of a part of their mass at an encounter, by which what is virtually a violent explosive is introduced between the two colliding stones. Prof. Darwin is careful to point out that it must necessarily be obscure as to how a small mass of solid matter can take up a very large amount of energy in a small fraction of a second.

Any direct indications from the heavens themselves, however slight, are of so great value, that I should perhaps in this connection call attention to a recent remarkable photograph, by Mr. Roberts, of the great nebula in Andromeda. On this plate we seem to have presented to us some stage of cosmical evolution on a gigantic scale. The photograph shows a sort of whirlpool disturbance of the luminous matter which is distributed in a plane inclined to the line of sight, in which a series of rings of bright matter separated by dark spaces, greatly foreshortened by perspective, surround a large undefined central mass. We are ignorant of the parallax of this nebula, but there can be little doubt that we are looking upon a system very remote, and therefore of a magnitude great beyond our power of adequate comprehension. The matter of this nebula, in whatever state it may be, appears to be distributed, as in so many other nebulæ, in rings or spiral streams, and to suggest a stage in a succession of evolutional events not inconsistent with that which the nebular hypothesis requires. To liken this object more directly to any particular stage in the formation of the solar system would be "to compare things great with small," and might be indeed to introduce a false analogy; but, on the other hand, we should err through an excess of caution if we did not accept the remarkable features brought to light by this photograph as a presumptive indication of a progress of events in cosmical history following broadly upon the lines of Laplace's theory.

The old view of the original matter of the nebulæ, that it consisted of a "fiery mist,"

> "a tumultuous cloud
> Instinct with fire and nitre,"

fell at once with the rise of the science of thermodynamics. In 1854, Helmholtz showed that the supposition of an original fiery condition of the nebulous stuff was unnecessary, since in the mutual gravitation of widely separated matter we have a store of potential energy sufficient to generate the high temperature of the sun and stars. We can scarcely go wrong in attributing the light of the nebulæ to the conversion of the gravitational energy of shrinkage into molecular motion.

The idea that the light of comets and of nebulæ may be due to a succession of ignited flashes of gas from the encounters of meteoric stones was suggested by Prof. Tait, and was brought to the notice of this Association in 1871 by Sir William Thomson in his Presidential Address.

The spectrum of the bright-line nebulæ is certainly not such a spectrum as we should expect from the flashing by collisions of meteorites similar to those which have been analyzed in our laboratories. The strongest lines of the substances which in the case of such meteorites would first show themselves, iron, sodium, magnesium, nickel, &c., are not those which distinguish the nebular spectrum. On the contrary, this spectrum is chiefly remarkable for a few brilliant lines, very narrow and defined, upon a background of a faint continuous spectrum, which contains numerous bright lines, and probably some lines of absorption.

The two most conspicuous lines have not been interpreted; for though the second line falls near, it is not coincident with a strong double line of iron. It is hardly necessary to say that though the near position of the brightest line to the bright double line of nitrogen, as seen in a small spectroscope in 1864, naturally suggested at that early time the possibility of the presence of this element in the nebulæ, I have been careful to point out, to prevent misapprehension, that in more recent years the nitrogen line and subsequently a lead line have been employed by me solely as fiducial points of reference in the spectrum.

The third line we know to be the second line of the first spectrum of hydrogen. Mr. Keeler has seen the first hydrogen line in the red, and photographs show that this hydrogen spectrum is probably present in its complete form, or nearly so, as we first learnt to know it in the absorption spectrum of the white stars.

We are not surprised to find associated with it the line D_3, near the position of the absent sodium lines, probably due to the atom of some unknown gas, which in the sun can only show itself in the outbursts of highest temperature, and for this reason does not reveal itself by absorption in the solar spectrum.

It is not unreasonable to assume that the two brightest lines, which are of the same order, are produced by substances of a similar nature, in which a vibratory motion corresponding to a very high temperature is also necessary. These substances, as well as that represented by the line D_3, may be possibly some of the unknown elements which are wanting in our terrestrial chemistry between hydrogen and lithium, unless indeed D_3 be on the lighter side of hydrogen.

In the laboratory we must have recourse to the electric discharge to bring out the spectrum of hydrogen; but in a vacuum-tube, though the radiation may be great, from the relative fewness of the luminous atoms or molecules or from some other cause, the temperature of the gas as a whole may be low.

On account of the large extent of the nebulæ, a comparatively small number of luminous molecules or atoms would probably be sufficient to make the nebulæ as bright as they appear to us. On such an assumption the average temperature may be low, but the individual particles, which by their encounters are luminous, must have motions corresponding to a very high temperature, and in this sense be extremely hot.

In such diffuse masses, from the great mean length of free path, the encounters would be rare but correspondingly violent, and tend to bring about vibrations of comparatively short period, as appears to be the case if we may judge by the great relative brightness of the more refrangible lines of the nebular spectrum.

Such a view may perhaps reconcile the high temperature which the nebular spectrum undoubtedly suggests with the much lower mean temperature of the gaseous mass, which we should expect at so early a stage of condensation, unless we assume a very enormous mass; or that the matter coming together had previously considerable motion, or considerable molecular agitation.

The inquisitiveness of the human mind does not allow us to remain content with the interpretation of the present state of the cosmical masses, but suggests the question—

> "What see'st thou else
> In the dark backward and abysm of time?"

What was the original state of things? how has it come about that by the side of ageing worlds we have nebulæ in a relatively younger stage? Have any of them received their birth from dark suns, which have collided into new life, and so belong to a second or later generation of the heavenly bodies?

During the short historic period, indeed, there is no record of such an event; still it would seem to be only through the collision of dark suns, of which the number must be increasing, that a temporary rejuvenescence of the heavens is possible, and by such ebbings and flowings of stellar life that the inevitable end to which evolution in its apparently uncompensated progress is carrying us can, even for a little, be delayed.

We cannot refuse to admit as possible such an origin for nebulæ.

In considering, however, the formation of the existing nebulæ we must bear in mind that, in the part of the heavens within our ken, the stars still in the early and middle stages of evolution exceed greatly in number those which appear to be in an advanced condition of condensation. Indeed, we find some stars which may be regarded as not far advanced beyond the nebular condition.

It may be that the cosmical bodies which are still nebulous owe their later development to some conditions of the part of space where they occur, such as, conceivably, a greater original homogeneity, in consequence of which condensation began less early. In other parts of space condensation may have been still further delayed, or even have not yet begun. It is worthy of remark that these nebulæ group themselves about the Milky Way, where we find a preponderance of the white-star type of stars, and almost exclusively the bright-line stars which Pickering associates with the planetary nebulæ. Further, Dr. Gill concludes, from the rapidity with which they impress themselves upon the plate, that the fainter stars of the Milky Way also, to a large extent, belong to this early type of stars. At the same time other types of stars occur also over this region, and the red hydrocarbon stars are found in certain parts; but possibly these stars may be before or behind the Milky Way, and not physically connected with it.

If light matter be suggested by the spectrum of these nebulæ, it may be asked further, as a pure speculation, whether in them we are witnessing possibly a later condensation of the light matter which had been left behind, at least in a relatively greater proportion, after the first growth of worlds into which the heavier matter condensed, though not without some entanglement of the lighter substances. The wide extent and great diffuseness of this bright-line nebulosity over a large part of the constellation of Orion may be regarded perhaps as pointing in this direction. The diffuse nebulous matter streaming round the Pleiades may possibly be another instance, though the character of its spectrum has not yet been ascertained.

In the planetary nebulæ, as a rule, there is a sensible increase of the faint continuous spectrum, as well as a slight thickening of the bright lines towards the centre of the nebula, appearances which are in favour of the view that these bodies are condensing gaseous masses.

Prof. G. Darwin, in his investigation of the equilibrium of a rotating mass of fluid, found, in accordance with the independent researches of Poincaré, that when a portion of the central body becomes detached through increasing angular velocity, the portion should bear a far larger ratio to the remainder than is observed in the planets and satellites of the solar system, even taking into account heterogeneity from the condensation of the parent mass.

Now this state of things, in which the masses though not equal are of the same order, does seem to prevail in many nebulæ, and to have given birth to a large class of binary stars. Mr. See has recently investigated the evolution of bodies of this class, and points out their radical differences from the solar system in the relatively large mass-ratios of the component bodies, as well as in the high eccentricities of their orbits brought about by tidal friction, which would play a more important part in the evolution of such systems.

Considering the large number of these bodies, he suggests that the solar system should perhaps no longer be regarded as representing celestial evolution in its normal form—

> "A goodly Paterne to whose perfect mould
> He fashioned them . . ."—

but rather as modified by conditions which are exceptional.

It may well be that in the very early stages condensing masses are subject to very different conditions, and that condensation may not always begin at one or two centres, but sometimes set in at a large number of points, and proceed in the different cases along very different lines of evolution.

Besides its more direct use in the chemical analysis of the heavenly bodies, the spectroscope has given to us a great and unexpected power of advance along the lines of the older astronomy. In the future, a higher value may, indeed, be placed upon this indirect use of the spectroscope than upon its chemical revelations.

By no direct astronomical methods could motions of approach or of recession of the stars be even detected, much less could they be measured. A body coming directly towards us or going directly from us appears to stand still. In the case of the stars we can receive no assistance from change of size or of brightness. The stars show no true disks in our instruments, and the nearest of them is so far off that if it were approaching us at the rate of a hundred miles in a second of time, a whole century of such rapid approach would not do more than increase its brightness by the one-fortieth part.

Still it was only too clear that, so long as we were unable to ascertain directly those components of the stars' motions which lie in the line of sight, the speed and direction of the solar motion in space, and many of the great problems of the constitution of the heavens, must remain more or less imperfectly known. Now the spectroscope has placed in our hands this power, which, though so essential, appeared almost in the nature of things to lie for ever beyond our grasp; it enables us to measure directly, and under favourable circumstances to within a mile per second, or even less, the speed of approach or of recession of a heavenly body. This method of observation has the great advantage for the astronomer of being independent of the distance of the moving body, and is therefore as applicable and as certain in the case of a body on the extreme confines of the visible universe, so long as it is bright enough, as in the case of a neighbouring planet.

Doppler had suggested as far back as 1841 that the same principle, on which he had shown that a sound should become sharper or flatter if there were an approach or a recession between the ear and the source of the sound, would apply equally to light; and he went on to say that the difference of colour of some of the binary stars might be produced in this way by their motions. Doppler was right in that the principle is true in the case of light, but he was wrong in the particular conclusion which he drew from it. Even if we suppose a star to be moving with a sufficiently enormous velocity to alter sensibly its colour to the eye, no such change would actually be seen, for the reason that the store of invisible light beyond both limits of the visible spectrum, the blue and the red, would be drawn upon, and light-waves invisible to us would be exalted or degraded so as to take the place of those raised or lowered in the visible region, and the colour of the star would remain unchanged. About eight years later Fizeau pointed out the importance of considering the individual wave-lengths of which white light is composed. As soon, however, as we had learned to recognize the lines of known substances in the spectra of the heavenly bodies, Doppler's principle became applicable as the basis of a new and most fruitful method of investigation. The measurement of the small shift of the celestial lines from their true positions, as shown by the same lines in the spectrum of a terrestrial substance, gives to us the means of ascertaining directly in miles per second the speed of approach or of recession of the heavenly body from which the light has come.

An account of the first application of this method of research to the stars, which was made in my observatory in 1868, was given by Sir Gabriel Stokes from this chair at the meeting at Exeter in 1869. The stellar motions determined by me were shortly after confirmed by Prof. Vogel in the case of Sirius, and in the case of other stars by Mr. Christie, now Astronomer-Royal, at Greenwich; but, necessarily, in consequence of the inadequacy of the instruments then in use for so delicate an inquiry, the amounts of these motions were but approximate.

The method was shortly afterwards taken up systematically at Greenwich and at the Rugby Observatory. It is to be greatly regretted that, for some reasons, the results have not been sufficiently accordant and accurate for a research of such exceptional delicacy. On this account probably, as well as that the spectroscope at that early time had scarcely become a familiar instrument in the observatory, astronomers were slow in availing themselves of this new and remarkable power of investigation. That this comparative neglect of so truly wonderful a method of ascertaining what was otherwise outside our powers of observation has greatly retarded the progress of astronomy during the last fifteen years, is but too clearly shown by the brilliant results which within the last couple of years have followed fast upon the recent masterly application of this method by photography at Potsdam, and by eye with the needful accuracy at the Lick Observatory. At last this use of the spectroscope has taken its true place as one of the most potent methods of astronomical research. It gives us the motions of approach and of recession, not in angular measures, which depend for their translation into actual velocities upon separate determinations of parallactic displacements, but at once in terrestrial units of distance.

This method of work will doubtless be very prominent in the astronomy of the near future, and to it probably we shall have to look for the more important discoveries in sidereal astronomy which will be made during the coming century.

In his recent application of photography to this method of determining celestial motions, Prof. Vogel, assisted by Dr. Scheiner, considering the importance of obtaining the spectrum of as many stars as possible on an extended scale without an exposure inconveniently long, wisely determined to limit the part of the spectrum on the plate to the region for which the ordinary silver-bromide gelatine plates are most sensitive—namely, to a small distance on each side of G—and to employ as the line of comparison the hydrogen line near G, and recently also certain lines of iron. The most minute and complete mechanical arrangements were provided for the purpose of securing the absolute rigidity of the comparison spectrum relatively to that of the star, and for permitting temperature adjustments and other necessary ones to be made.

The perfection of these spectra is shown by the large number of lines, no fewer than 250 in the case of Capella, within the small region of the spectrum on the plate. Already the motions of about fifty stars have been measured with an accuracy, in the case of the larger number of them, of about an English mile per second.

At the Lick Observatory it has been shown that observations can be made directly by eye with an accuracy equally great. Mr. Keeler's brilliant success has followed in great measure from the use of the third and fourth spectra of a grating 14,438 lines to the inch. The marvellous accuracy attainable in his hands on a suitable star is shown by observations on three nights of the star Arcturus, the largest divergence of his measures being not greater than six tenths of a mile per second, while the mean of the three nights' work agreed with the mean of five photographic determinations of the same star at Potsdam to within one-tenth of an English mile. These are determinations of the motions of a sun so stupendously remote that even the method of parallax practically fails to fathom the depth of intervening space, and by means of light-waves which have been according to Elkin's nominal parallax, nearly 200 years upon their journey.

Mr. Keeler, with his magnificent means, has accomplished a task which I attempted in vain in 1874, with the comparatively poor appliances at my disposal, of measuring the motions in the line of sight of some of the planetary nebulæ. As the stars have considerable motions in space, it was to be expected that nebulæ should possess similar motions, for the stellar motions must have belonged to the nebulæ out of which they have been evolved. My instrumental means, limiting my power of detection to motions greater than twenty-five miles per second, were insufficient. Mr. Keeler has found in the examination of ten nebulæ motions varying from two miles to twenty-seven miles, with one exceptional motion of nearly forty miles.

For the nebula of Orion, Mr. Keeler finds a motion of recession of about ten miles a second. Now this motion agrees closely with what it should appear to have from the drift of the solar system itself, so far as it has been possible at present to ascertain the probable velocity of the sun in space. This grand nebula, of vast extent and of extreme tenuity, is probably more nearly at rest relatively to the stars of our system than any other celestial object we know; still it would seem more likely that even here we have some motion, small though it may be, than that the motions of the matter of which it is formed were so absolutely balanced as to leave this nebula in the unique position of absolute immobility in the midst of whirling and drifting suns and systems of suns.

The spectroscopic method of determining celestial motions in the line of sight has recently become fruitful in a new but not altogether unforeseen direction, for it has, so to speak, given us a separating power far beyond that of any telescope the glass-maker and the optician could construct, and so enabled us to penetrate into mysteries hidden in stars apparently single, and altogether unsuspected of being binary systems. The spectroscope has not simply added to the list of the known binary stars, but has given to us for the first time a knowledge of a new clas of stellar systems, in which the components are in some cases of nearly equal magnitude, and in close proximity, and are revolving with velocities greatly exceeding the planetary velocities of our system.

The K line in the photographs of Mizar, taken at the Harvard College Observatory, was found to be double at intervals of fifty-two days. The spectrum was therefore not due to a single source of light, but to the combined effect of two stars moving periodically in opposite directions in the line of sight. It is obvious that if two stars revolve round their common centre of gravity in a plane not perpendicular to the line of sight, all the lines in a spectrum common to the two stars will appear alternately single or double.

In the case of Mizar and the other stars to be mentioned, the spectroscopic observations are not as yet extended enough to furnish more than an approximate determination of the elements of their orbits.

Mizar especially, on account of its relatively long period—about 105 days—needs further observations. The two stars are moving each with a velocity of about fifty miles a second, probably in elliptical orbits, and are about 143 millions of miles apart. The stars, of about equal brightness, have together a mass about forty times as great as that of our sun.

A similar doubling of the lines showed itself in the Harvard photographs of β Aurigæ at the remarkably close interval of almost exactly two days, indicating a period of revolution of about four days. According to Vogel's later observations, each star has a velocity of nearly seventy miles a second, the distance between the stars being little more than seven and a half millions of miles, and the mass of the system 4·7 times that of the sun. The system is approaching us at the speed of about sixteen miles a second.

The telescope could never have revealed to us double stars of this order. In the case of β Aurigæ, combining Vogel's distance with Pritchard's recent determination of the star's parallax, the greatest angular separation of the stars as seen from the earth would be 1/200 part of a second of arc, and therefore very far too small for the highest powers of the largest telescopes. If we take the relation of aperture to separating power usually accepted, an object-glass of about 80 feet in diameter would be needed to resolve this binary star. The spectroscope, which takes no note of distance, magnifies, so to speak, this minute angular separation 4000 times; in other words, the doubling of the lines, which is the phenomenon that we have to observe, amounts to the easily measurable quantity of twenty seconds of arc.

There were known, indeed, variable stars of short period, which it had been suggested might be explained on the hypothesis of a dark body revolving about a bright sun in a few days, but this theory was met by the objection that no such systems of closely revolving suns were known to exist.

The Harvard photographs of which we have been speaking, were taken with a slitless form of spectroscope, the prisms being placed, as originally by Fraunhofer, before the object-glass of the telescope. This method, though it possesses some advantages, has the serious drawback of not permitting a direct comparison of the star's spectrum with terrestrial spectra. It is obviously unsuited to a variable star like Algol, where one star only is bright, for in such a case there would be no doubling of the lines, but only a small shift to and fro of the lines of the bright star as it moved in its orbit alternately towards and from our system, which would need for its detection the fiducial positions of terrestrial lines compared directly with them.

For such observations the Potsdam spectrograph was well adapted. Prof. Vogel found that the bright star of Algol did pulsate backwards and forwards in the visual direction in a period corresponding to the known variation of its light. The explanation which had been suggested for the star's variability, that it was partially eclipsed at regular intervals of 68·8 hours by a dark companion large enough to cut off nearly five-sixths of its light, was therefore the true one. The dark companion, no longer able to hide itself by its obscureness, was brought out into the light of direct observation by means of its gravitational effects.

Seventeen hours before minimum, Algol is receding at the rate of about 24½ miles a second, while seventeen hours after minimum it is found to be approaching with a speed of about 28½ miles. From these data, together with those of the variation of its light, Vogel found, on the assumption that both stars have the same density, that the companion, nearly as large as the sun, but with about one-fourth his mass, revolves with a velocity of about fifty-five miles a second. The bright star, of about twice the size and mass, moves about the common centre of gravity with the speed of about twenty-six miles a second. The system of the two stars, which are about 3¼ millions of miles apart, considered as a whole, is approaching us with a velocity of 2·4 miles a second. The great difference in luminosity of the two stars, not less than fifty times, suggests rather that they are in different stages of condensation, and dissimilar in density.

It is obvious that if the orbit of a star with an obscure companion is inclined to the line of sight, the companion will pass above or below the bright star, and produce no variation of its light. Such systems may be numerous in the heavens. In Vogel's photographs, Spica, which is not variable, by a small shifting of its lines reveals a backward and forward periodical pulsation due to orbital motion. As the pair whirl round their common centre of gravity, the bright star is sometimes advancing, at others receding. They revolve in about four days, each star moving with a velocity of about fifty-six miles a second in an orbit probably nearly circular, and possess a combined mass of rather more than two and a half times that of the sun. Taking the most probable value for the star's parallax, the greatest angular separation of the stars would be far too small to be detected with the most powerful telescopes.

If in a close double star the fainter companion is of the white-star type, while the bright star is solar in character, the composite spectrum would be solar with the hydrogen lines unusually strong. Such a spectrum would in itself afford some probability of a double origin, and suggest the existence of a companion star.

In the case of a true binary star the orbital motions of the pair would reveal themselves in a small periodical swaying of the hydrogen lines relatively to the solar ones.

Prof. Pickering considers that his photographs show ten stars with composite spectra; of these, five are known to be double. The others are: τ Persei, ζ Aurigæ, δ Sagittarii, 31 Ceti, and β Capricorni. Perhaps β Lyræ should be added to this list.

In his recent classical work on the rotation of the sun, Dunér has not only determined the solar rotation for the equator but for different parallels of latitude up to 75°. The close accordance of his results shows that these observations are sufficiently accurate to be discussed with the variation of the solar rotation for different latitudes which had been determined by the older astronomical methods from the observations of the solar spots.

Though I have already spoken incidentally of the invaluable aid which is furnished by photography in some of the applications of the spectroscope to the heavenly bodies, the new power which modern photography has put into the hands of the astronomer is so great, and has led already, within the last few years, to new acquisitions of knowledge of such vast importance, that it is fitting that a few sentences should be specially devoted to this subject.

Photography is no new discovery, being about half a century old; it may excite surprise, and indeed possibly suggest some apathy on the part of astronomers, that though the suggestion of the application of photography to the heavenly bodies dates from the memorable occasion when, in 1839, Arago, announcing to the Académie des Sciences the great discovery of Niepce and Daguerre, spoke of the possibility of taking pictures of the sun and moon by the new process, yet that it is only within a few years that notable advances in astronomical methods and discovery have been made by its aid.

The explanation is to be found in the comparative unsuitability of the earlier photographic methods for use in the observatory. In justice to the earlier workers in astronomical photography, among whom Bond, De la Rue, J. W. Draper, Rutherfurd, Gould, hold a foremost place, it is needful to state clearly that the recent great successes in astronomical photography are not due to greater skill, nor, to any great extent, to superior instruments, but to the very great advantages which the modern gelatine dry plate possesses for use in the observatory over the methods of Daguerre, and even over the wet collodion film on glass, which, though a great advance on the silver plate, went but a little way towards putting into the hands of the astronomer a photographic surface adapted fully to his wants.

The modern silver-bromide gelatine plate, except for its grained texture, meets the needs of the astronomer at all points. It possesses extreme sensitiveness; it is always ready for use;

it can be placed in any position; it can be exposed for hours; lastly, it does not need immediate development, and for this reason can be exposed again to the same object on succeeding nights, so as to make up by several instalments, as the weather may permit, the total time of exposure which is deemed necessary.

Without the assistance of photography, however greatly the resources of genius might overcome the optical and mechanical difficulties of constructing large telescopes, the astronomer would have to depend in the last resource upon his eye. Now we cannot by the force of continued looking bring into view an object too feebly luminous to be seen at the first and keenest moment of vision. But the feeblest light which falls upon the plate is not lost, but is taken in and stored up continuously. Each hour the plate gathers up 3600 times the light-energy which it received during the first second. It is by this power of accumulation that the photographic plate may be said to increase, almost without limit, though not in separating power, the optical means at the disposal of the astronomer for the discovery or the observation of faint objects.

Two principal directions may be pointed out in which photography is of great service to the astronomer. It enables him within the comparatively short time of a single exposure to secure permanently with great exactness the relative positions of hundreds or even of thousands of stars, or the minute features of nebulæ or other objects, or the phenomena of a passing eclipse, a task which by means of the eye and hand could only be accomplished, if done at all, after a very great expenditure of time and labour. Photography puts it in the power of the astronomer to accomplish in the short span of his own life, and so enter into their fruition, great works which otherwise must have been passed on by him as a heritage of labour to succeeding generations.

The second great service which photography renders is not simply an aid to the powers the astronomer already possesses. On the contrary, the plate, by recording light-waves which are both too small and too large to excite vision in the eye, brings him into a new region of knowledge, such as the infra-red and the ultra-violet parts of the spectrum, which must have remained for ever unknown but for artificial help.

The present year will be memorable in astronomical history for the practical beginning of the Photographic Chart and Catalogue of the Heavens, which took their origin in an International Conference which met in Paris in 1887, by the invitation of M. l'Amiral Mouchez, Director of the Paris Observatory.

The richness in stars down to the ninth magnitude of the photographs of the comet of 1882 taken at the Cape Observatory under the superintendence of Dr. Gill, and the remarkable star charts of the Brothers Henry which followed two years later, astonished the astronomical world. The great excellence of these photographs, which was due mainly to the superiority of the gelatine plate, suggested to these astronomers a complete map of the sky, and a little later gave birth in the minds of the Paris astronomers to the grand enterprise of an International Chart of the Heavens. The actual beginning of the work this year is in no small degree due to the great energy and tact with which the Director of the Paris Observatory has conducted the initial steps, through the many delicate and difficult questions which have unavoidably presented themselves in an undertaking which depends upon the harmonious working in common of many nationalities, and of no fewer than eighteen observatories in all parts of the world. The three years since 1887 have not been too long for the detailed organization of this work, which has called for several elaborate preliminary investigations on special points in which our knowledge was insufficient, and which have been ably carried out by Profs. Vogel and Bakhuyzen, Dr. Trépied, Dr. Scheiner, Dr. Gill, the Astronomer-Royal, and others. Time also was required for the construction of the new and special instruments.

The decisions of the Conference in their final form provide for the construction of a great photographic chart of the heavens with exposures corresponding to forty minutes' exposure at Paris, which it is expected will reach down to stars of about the fourteenth magnitude. As each plate is to be limited to four square degrees, and as each star, to avoid possible errors, is to appear on two plates, over 22,000 photographs will be required. For the more accurate determination of the positions of the stars, a *réseau* with lines at distances of 5 mm. apart is to be previously impressed by a faint light upon the plate, so that the image of the *réseau* will appear together with the images of the stars when the plate is developed. This great work will be divided, according to their latitudes, among eighteen observatories provided with similar instruments, though not necessarily constructed by the same maker. Those in the British dominions and at Tacubaya have been constructed by Sir Howard Grubb.

Besides the plates to form the great chart, a second set of plates for a catalogue is to be taken, with a shorter exposure, which will give stars to the eleventh magnitude only. These plates, by a recent decision of the Permanent Committee, are to be pushed on as actively as possible, though as far as may be practicable plates for the chart are to be taken concurrently. Photographing the plates for the catalogue is but the first step in this work, and only supplies the data for the elaborate measurements which have to be made, which are, however, less laborious than would be required for a similar catalogue without the aid of photography.

Already Dr. Gill has nearly brought to conclusion, with the assistance of Prof. Kapteyn, a preliminary photographic survey of the southern heavens.

With an exposure sufficiently long for the faintest stars to impress themselves upon the plate, the accumulating action still goes on for the brighter stars, producing a great enlargement of their images from optical and photographic causes. The question has occupied the attention of many astronomers, whether it is possible to find a law connecting the diameters of these more or less over-exposed images with the relative brightness of the stars themselves. The answer will come out undoubtedly in the affirmative, though at present the empirical formulæ which have been suggested for this purpose differ from each other. Captain Abney proposes to measure the total photographic action, including density as well as size, by the obstruction which the stellar image offers to light.

A further question follows as to the relation which the photographic magnitudes of stars bear to those determined by eye. Visual magnitudes are the physiological expression of the eye's integration of that part of the star's light which extends from the red to the blue. Photographic magnitudes represent the plate's integration of another part of the star's light—namely, from a little below where the power of the eye leaves off in the blue to where the light is cut off by the glass, or is greatly reduced by want of proper corrections when a refracting telescope is used. It is obvious that the two records are taken by different methods in dissimilar units of different parts of the star's light. In the case of certain coloured stars the photographic brightness is very different from the visual brightness; but in all stars, changes, especially of a temporary character, may occur in the photographic or the visual region, unaccompanied by a similar change in the other part of the spectrum. For these reasons it would seem desirable that the two sets of magnitudes should be tabulated independently, and be regarded as supplementary of each other.

The determination of the distances of the fixed stars from the small apparent shift of their positions when viewed from widely separated positions of the earth in its orbit is one of the most refined operations of the observatory. The great precision with which this minute angular quantity—a fraction of a second only—has to be measured, is so delicate an operation with the ordinary micrometer, though, indeed, it was with this instrument that the classical observations of Sir Robert Ball were made, that a special instrument, in which the measures are made by moving the two halves of a divided object-glass, known as a heliometer, has been pressed into this service, and quite recently, in the skilful hands of Dr. Gill and Dr. Elkin, has largely increased our knowledge in this direction.

It is obvious that photography might be here of great service, if we could rely upon measurements of photographs of the same stars taken at suitable intervals of time. Prof Pritchard, to whom is due the honour of having opened this new path, aided by his assistants, has proved by elaborate investigations that measures for parallax may be safely made upon photographic plates, with, of course, the advantages of leisure and repetition; and he has already by this method determined the parallax for twenty-one stars with an accuracy not inferior to that of values previously obtained by purely astronomical methods.

The remarkable successes of astronomical photography, which depend upon the plate's power of accumulation of a very feeble light acting continuously through an exposure of several hours, are worthy to be regarded as a new revelation. The first chapter opened when, in 1880, Dr. Henry Draper obtained a picture of the nebula of Orion; but a more important advance was made in 1883, when Dr. Common, by his photographs, brought to our knowledge details and extensions of this nebula hitherto unknown. A further disclosure took place in 1885, when the Brothers Henry showed for the first time in great detail the spiral nebulosity issuing from the bright star Maia of the Pleiades, and, shortly afterwards, nebulous streams about the other stars of this group. In 1886, Mr. Roberts, by means of a photograph to which three hours' exposure had been given, showed the whole background of this group to be nebulous. In the following year Mr. Roberts more than doubled for us the great extension of the nebular region which surrounds the trapezium in the constellation of Orion. By his photographs of the great nebula in Andromeda he has shown the true significance of the dark canals which had been seen by the eye. They are in reality spaces between successive rings of bright matter, which appeared nearly straight owing to the inclination in which they lie relatively to us. These bright rings surround an undefined central luminous mass. I have already spoken of this photograph.

Some recent photographs by Mr. Russell show that the great rift in the Milky Way in Argus, which to the eye is void of stars, is in reality uniformly covered with them. Also, quite recently, Mr. George Hale has photographed the prominences by means of a grating, making use of the lines H and K.

The heavens are richly but very irregularly inwrought with stars, the brighter stars cluster into well-known groups upon a background formed of an enlacement of streams and convoluted windings and intertwined spirals of fainter stars, which becomes richer and more intricate in the irregularly rifted zone of the Milky Way.

We, who form part of the emblazonry, can only see the design distorted and confused; here crowded, there scattered, at another place superposed. The groupings due to our position are mixed up with those which are real.

Can we suppose that each luminous point has no relation to the others near it than the accidental neighbourship of grains of sand upon the shore, or of particles of the wind-blown dust of the desert? Surely every star, from Sirius and Vega down to each grain of the light-dust of the Milky Way, has its present place in the heavenly pattern from the slow evolving of its past. We see a system of systems, for the broad features of clusters and streams and spiral windings which mark the general design are reproduced in every part. The whole is in motion, each point shifting its position by miles every second, though from the august magnitude of their distances from us and from each other, it is only by the accumulated movements of years or of generations that some small changes of relative position reveal themselves.

The deciphering of this wonderfully intricate constitution of the heavens will be undoubtedly one of the chief astronomical works of the coming century. The primary task of the sun's motion in space, together with the motions of the brighter stars, has been already put well within our reach by the spectroscopic method of the measurement of star-motions in the line of sight.

From other directions information is accumulating: from photographs of clusters and parts of the Milky Way, by Roberts in this country, Barnard at the Lick Observatory, and Russell at Sydney; from the counting of stars, and the detection of their configurations, by Holden and by Backhouse; from the mapping of the Milky Way by eye, at Parsonstown; from photographs of the spectra of stars, by Pickering at Harvard and in Peru; and from the exact portraiture of the heavens in the great international star chart which begins this year.

I have but touched some only of the problems of the newer side of astronomy. There are many others which would claim our attention if time permitted. The researches of the Earl of Rosse on lunar radiation, and the work on the same subject and on the sun, by Langley. Observations of lunar heat with an instrument of his own invention by Mr. Boys; and observations of the variation of the moon's heat with its phase by Mr. Frank Very. The discovery of the ultra-violet part of the hydrogen spectrum, not in the laboratory, but from the stars. The confirmation of this spectrum by terrestrial hydrogen in part by H. W. Vogel, and in its all but complete form by Cornu, who found similar series in the ultra-violet spectra of aluminium and thallium. The discovery of a simple formula for the hydrogen series by Balmer. The important question as to the numerical spectral relationship of different substances, especially in connection with their chemical properties; and the further question as to the origin of the harmonic and other relations between the lines and the groupings of lines of spectra; on these points contributions during the past year have been made by Rudolf v. Kövesligethy, Ames, Hartley, Deslandres, Rydberg, Grünwald, Kayser and Runge, Johnstone Stoney, and others. The remarkable employment of interference phenomena by Prof. Michelson for the determination of the size, and distribution of light within them, of the images of objects which when viewed in a telescope subtend an angle less than that subtended by the light-wave at a distance equal to the diameter of the objective. A method applicable not alone to celestial objects, but also to spectral lines, and other questions of molecular physics.

Along the older lines there has not been less activity; by newer methods, by the aid of larger or more accurately constructed instruments, by greater refinement of analysis, knowledge has been increased, especially in precision and minute exactness.

Astronomy, the oldest of the sciences, has more than renewed her youth. At no time in the past has she been so bright with unbounded aspirations and hopes. Never were her temples so numerous, nor the crowd of her votaries so great. The British Astronomical Association formed within the year numbers already about 600 members. Happy is the lot of those who are still on the eastern side of life's meridian!

Already, alas! the original founders of the newer methods are falling out—Kirchhoff, Ångström, D'Arrest, Secchi, Draper, Becquerel; but their places are more than filled; the pace of the race is gaining, but the goal is not and never will be in sight.

Since the time of Newton our knowledge of the phenomena of Nature has wonderfully increased, but man asks, perhaps more earnestly now than in his days, What is the ultimate reality behind the reality of the perceptions? Are they only the pebbles of the beach with which we have been playing? Does not the ocean of ultimate reality and truth lie beyond?

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY PROF. OLIVER J. LODGE, D.SC., LL.D., F.R.S., PRESIDENT OF THE SECTION.

DURING the past year three or four events call for special mention in an annual deliverance of this kind by a physicist.

One is the Faraday centenary, which was kept in a happy and simple manner by a cosmopolitan gathering in the place so long associated with his work, and by discourses calling attention to the modern development of discoveries made by him.

Another is the decease of the veteran Wilhelm Weber, one of the originators of that absolute system of measurement which, though still ungrasped in its simplicity and completeness by the majority of men engaged in practice, nor even, I fear, wholly understood by some of those engaged in University teaching, has yet done so much, and is destined to do still more, for the unification of physical science, and for a thorough comprehension of its range and its limitations.

A third event of importance during the year is the discovery in America of a binary system of stars, revolving round each other with grotesque haste, and with a proximity to each other such as to render their ordinary optical separation quite impossible. Ideas concerning the future of such systems, if, as seems probable, their revolution period is shorter than their axial period, will readily suggest themselves, in accordance with the principles elaborated by Prof. George Darwin. The subject more properly belongs to our President, but I may parenthetically exclaim at the singular absurdity of the notion which was once propounded by a philosopher, that motion of stars in our line of sight must for ever remain unknown to us; when the mere time of revolution of a satellite, compared with its distance from its central body, is theoretically sufficient to give us information on this head. As a matter of pedagogy it is convenient to observe that the principle called Doppler's, which is generally known to apply to the periodic disturbances called Light and Sound, applies equally to all periodic occurrences; and that the explanation of anomalies of Jupiter's first satellite by Roemer may be regarded as an instance of Doppler's principle.[1] Any discrepancy between the observed and the calculated times of revolution of stars round each other can possibly be explained by a relative motion between us and the pair of bodies along the line of sight.

If our text-books clearly recognized this, we should not so often find examination candidates asserting that the apparent time of revolution of a satellite of Jupiter depends on the distance of the earth from that planet, instead of on the speed. I should indeed be sorry to be judged by the performance of my own students, but I fear that many of the less obvious mistakes made by reasonably trained examination candidates are more directly traceable to their teachers than some of us as teachers would like to admit.

The change in the refrangibility of light by reason of the motion of its source, though commonplace enough now, was at first regarded as too small to be observed, and one or two attempts directed to detecting the effect of this principle on the spectra of the stars, or sometimes on sunlight reflected by a 45° mirror into the line of the earth's motion (which is not a possible method), wholly failed. I take pleasure in remembering that this effect was clearly observed for the first time by the gentleman we this year honour as our President; and that it is by this very means that the latest sensational discovery in astronomy of the rapidly revolving twin star β-Aurigæ, by Prof. Pickering and the staff connected with the Draper Memorial, was made.

The funds for the investigation that led to this result were provided by Mrs. Draper, as a memorial to her late husband; and if β-Aurigæ does not constitute a satisfactory memorial, I am at a loss to conceive the kind of tombstone which the relations of a man of science would prefer.

The fourth event to which it behoves me to refer is the practical discovery of a physical method for colour photography. When I say practical I do not mean commercial, nor do I know that it will ever become applicable to the ordinary business of the photographer. Whether it does or not, it is a sound achievement by physical means of a result which the chemical means hitherto tried failed, some think necessarily failed, to produce. I say practical, because already it had been suggested as possible theoretically; and a step toward it, indeed very near it, had been actually made. The first suggestion of the method, so far as I know, was made by Lord Rayleigh in the course of a mathematical paper on the reflection of light, and with reference to some results of Becquerel obtained on a totally different plan. He said in a note that if by normal reflection waves of light were converted into stationary waves, they could shake out silver in strata half a wave-length apart, and that such strata would give selective reflection and show iridescence.

The colour of certain crystals of chlorate of potash, described in a precise manner by Sir George Stokes (Proc. Roy. Soc., February 1885), and also the colours of opal and ancient glass, had been elaborately and completely explained by Lord Rayleigh on this theory of a periodic structure (the laminated structure in the case of chlorate of potash being caused by twinning) (Phil. Mag., September 1888, pp. 256 and 241); and he subsequently illustrated it with sound and a series of muslin disks one behind the other on a set of lazy-tongs. Each membrane reflected an inappreciable amount, but successive equidistant membranes reinforced each other's action, and the entire set reflected distinctly one definite note, of wave-length twice the distance between adjacent muslins. So also with any series of equidistant strata each very slightly reflecting. They should give selective reflection, and the spectrum of their reflected beam should show a single line or narrow band, corresponding to a wave-length twice the distance of the strata apart.[2]

Independently of all this, Herr Otto Wiener, imitating Hertz's experiments with ordinary light, in 1889 reflected a beam directly back on itself, and, by interposing a very thin collodion film at extraordinarily oblique incidence, succeeded in the difficult experiment of so magnifying by the cosine of inclination the half wave-length, as to get the silver deposited in strata of visible width, and thus to photograph the interference nodes themselves at the places where they were cut by the plane of the film (*Wiedemann's Annalen*, vol. xl., 1890).

Then M. Lippmann, using a thicker film, not put obliquely but normal to the light, obtained the strata within the thickness of the film itself—hundreds of layers; and so, employing incidence light of definite wave-length, was able to produce a stratified deposit, which reflected back at appropriate incidences the same wave-length as produced it; thus reproducing, of course, the definite colour.

It is probable that the silver is first shaken out at the ventral segments, but that the strata so formed are thick and blurry. I conjecture that by over-exposure this deposit is nearly all mopped up again, traces being left only at the nodes, where the action is very feeble and takes a long time to occur; but that these residual strata, being fairly sharp and definite, will be likely to give much better effects. And so I suppose that these are what are actually effective in obtaining M. Lippmann's very interesting, though not yet practically useful, result.

I now leave the retrospect of what has been done, although many other topics might usefully detain us, and I proceed to glance forward at the progress ahead and at the means we have for effectively grappling with our due share of it.

There is a subject which has long been in my mind, and which I determined to bring forward whenever I had a cathedral opportunity of doing so; and now, if ever, is a suitable occasion. It is to call attention to the fact that the further progress of physical science in the somewhat haphazard and amateur fashion in which it has been hitherto pursued in this country is becoming increasingly difficult, and that the quantitative portion especially should be undertaken in a permanent and publicly-supported physical laboratory on a large scale. If such an establishment were to weaken the sinews of private enterprise and individual research it should be strenuously opposed; but, in my opinion, it would have the opposite effect, by relieving the private worker of much which he can only with great difficulty, sacrifice, and expense, undertake. To illustrate more precisely what I mean, it is sufficient to recall the case of astronomy. The amateur astronomer has much work lying ready to his hand, and he grapples with it manfully. To him is left the striking out of new lines and the guerilla warfare of science. Skirmishing and brilliant cavalry evolutions are his natural field, he should not be called upon to take part in the general infantry advance. It is wasting his energies, and he could not do it in the long run well. What, for instance, would have been the state of astronometry—the nautical almanac department of astronomy—without the consecutive and systematic work of the National Observatory at Greenwich? It may be that some enthusiastic amateurs would have devoted their lives to this routine kind of work, and here at one time and there at another a series of accurate observations would have been kept for several years. Pursued in that way, however, not only would the effort be spasmodic and temporary, but the energy and enthusiasm of those amateurs would have been diverted from the pioneering more suited to them, and have been cramped in the groove of routine, eminently adapted to a permanent official staff, but not wholesome for an individual.

Long-continued consecutive observations may be made by a leader of science, as functions may be tabulated by an eminent mathematician; but if the work can be done almost equally well (some would say better) by a professional observer or computator, how great an economy results.

Now all this applies equally to physics. The ohm has been determined with 4-figure, perhaps with 5-figure, accuracy; but think of the list of eminent men to whose severe personal labour we owe this result, and ask if the spoil is worth the cost. Perhaps in this case it is, as a specimen of a well-conducted determination. We must have a few specimens, and our leaders must show us the way to do things. But let us not continue to use them for such purposes much longer. The quest of the fifth or sixth decimal is a very legitimate, and may become a very absorbing, quest, but there are plenty of the rank and file who can undertake it if properly generalled and led: not as isolated individuals, but as workers in a National Laboratory under a competent head and a governing committee. By this means work far greater in quantity, and in the long run more exact in quality, can be turned out, by patient and conscientious labour without much genius, by the gradual improvement of instrumental means, by the skill acquired by practice, and by the steady drudgery of routine. Paris has long had one form of such an institution, in the Conservatoire des Arts et Métiers, and has been able to impose the metric system on the civilised world in consequence. It can also point to the classical determinations of Regnault as the fruits of just such a system. Berlin is now starting a similar or a more ambitious scheme for a permanent national physical institute. Is it not time that England, who in physical science, I venture to think, may in some sort claim a leading place, should be thinking of starting the same movement?

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[1] Dr. Huggins has just pointed out to me a perfectly clear statement to the above effect in Professor Tait's little book on Light.

[2] The footnote of Lord Rayleigh on page 158, *Phil. Mag.*, 1887, vol. xxiv., is brief and forcible enough to quote in full:—"A detailed experimental examination of the various cases in which a laminated structure leads to a powerful but highly selected reflection would be of value. The most frequent examples are met with in the organic world. It has occurred to me that Becquerel's reproduction of the spectrum in natural colours upon silver plates may perhaps be explicable in this manner. The various parts of the film of subchloride of silver with which the metal is coated may be conceived to be subjected during exposure to *stationary* luminous waves of nearly definite wave-length, the effect of which might be to impress upon the substance a periodic structure occurring at intervals equal to *half* the wave-length of light; just as a sensitive flame exposed to stationary sonorous waves is influenced at the loops, but not at the nodes (*Phil. Mag.*, March 1879, p. 153). In this way the operation of any kind of light would be to produce just such a modification of the film as would cause it to reflect copiously that particular kind of light. I abstain at present from developing this suggestion, in the hope of soon finding an opportunity of making myself experimentally acquainted with the subject."

The Meteorological and Magnetic Observatory at Kew (in the inauguration of which this Association took so large a part) is a step, and much useful quantitative work is done there. The new Electric Standardizing Laboratory of the Board of Trade is another and, in some respects perhaps, a still closer appproximation to the kind of thing I advocate. But what I want to see is a much larger establishment erected on the most suitable site, limited by no speciality of aim nor by the demands of the commercial world, furnished with all appropriate appliances, to be amended and added to as time goes on and experience grows, and invested with all the dignity and permanence of a national institution: a Physical Observatory, in fact, precisely comparable to the Greenwich Observatory, and aiming at the very highest quantitative work in all departments of physical science. That the arts would be benefited may be assumed without proof. It is largely the necessity of engineers that has inspired the amount of accuracy in electrical matters already attained. The work and appliances of the mechanical engineer eclipse the present achievements of the physicist in point of accuracy, and it is by the aid of the mechanician and optician that precision even in astronomy has reached so high a stage. There is no reason why physical determinations should be conducted in an amateur fashion, with comparatively imperfect instruments, as at present they mostly are. Discoveries lie along the path of extreme accuracy, and they will turn up in the most unexpected way. The aberration of light would not have been discovered had not Bradley been able to measure to less than 1 part in 10,000; and what a brilliant and momentous discovery it was! He was aiming at the detection of stellar parallax, but the finite velocity of light was a bigger discovery than any parallax. This is the type of result which sometimes lurks in the fifth decimal, and which confers upon it an importance beside which the demands of men who wish to serve the taste and the pocket of the British public sink into insignificance.

In a National Observatory accuracy should be the one great end: the utmost accuracy in every determination that is decided on and made. Only one thing should be more thought of than the fifth significant figure, and that is the sixth. The consequences flowing from the results may safely be left; such as are not obvious at once will distil themselves out in time. And the great army of outside physicists, assured of the good work being done at headquarters, will (to speak again in astronomical parable) cease from peddling with taking transits or altitudes, and will be free to discover comets, to invent the spectroscope, to watch solar phenomena, to chemically analyse the stars, to devise celestial photography, and to elaborate still more celestial theories; all of which novelties in their maturity may be handed over to the National Observatory, to be henceforth incorporated with, and made part of, its routine life; leaving the advance guard and skirmishers free to explore fresh territory, secure in the knowledge that what they have acquired will be properly surveyed, mapped, and utilised, without further attention from them. As to the practical applications, they may in any case be left to take care of themselves. The instinct of humanity in this direction, and the so-called solid gains associated with practical achievements, will always secure a sufficient number of acute and energetic workers to turn the new territory into arable land and pasture adapted to the demands of the average man. The labour of the agriculturist in rendering soil fertile is, of course, beyond praise; but it is not the work of the pioneer. As Mr. Huxley eloquently put it, when contrasting the application of science with the advance of science itself, speaking of the things of commercial value which the physical philosopher sometimes discovers:—"Great is the rejoicing of those who are benefited thereby, and, for the moment, science is the Diana of all the craftsmen. But even while the cries of jubilation resound, and this flotsam and jetsam of the tide of investigation is being turned into the wages of workmen and the wealth of capitalists, the crest of the wave of scientific investigation is far away on its course over the illimitable ocean of the unknown."

I have spoken of the work of the National Laboratory as devoted to accuracy. It is hardly necessary to say that it will be also the natural custodian of our standards, in a state fit for use and for comparison with copies sent to be certified. Else perhaps some day our standard ohm may be buried in a brick wall at Westminster, and no one living may be able to recall precisely where it is.

But, in addition to these main functions, there is another, equally important with them, to which I must briefly refer. There are many experiments which cannot possibly be conducted by an individual, because forty or fifty years is not long enough for them. Secular experiments on the properties of materials—the elasticity of metals, for instance; the effect of time on molecular arrangement; the influence of long exposure to light, or to heat, or to mechanical vibration, or to other physical agents.

Does the permeability of soft iron decay with age, by reason of the gradual cessation of its Ampèrian currents? Do gases cool themselves when adiabatically preserved, by reason of imperfect elasticity or too many degrees of freedom of their molecules? Unlikely, but not impossible. Do thermo-electric properties alter with time? And a multitude of other experiments which appear specially applicable to substances in the solid state—a state which is more complicated, and has been less investigated, than either the liquid or the gaseous: a state in which time and past history play an important part.

Whichever of these long researches requires to be entered on, a national laboratory, with permanent traditions and a continuous life, is undoubtedly the only appropriate place. At such a place as Glasgow the exceptional magnitude of a present occupant may indeed inspire sufficient piety in a successor to secure the continuance of what has been there begun; but in most college laboratories, under conditions of migration, interregnum, and a new *régime*, continuity of investigation is hopeless.

I have at any rate said enough to indicate the kind of work for which the establishment of a well-furnished laboratory with fully equipped staff is desirable, and I do not think that we, as a nation, shall be taking our proper share of the highest scientific work of the world until such an institution is started on its career.

There is only one evil which, so far as I can see, is to be feared from it: if ever it were allowed to impose on outside workers as a central authority, from which infallible dicta were issued, it would be an evil so great that no amount of good work carried on by it could be pleaded as sufficient mitigation.

If ever by evil chance such an attitude were attempted, it must rest with the workers of the future to see that they permit no such shackles; for if they are not competent to be independent, and to contemn the voice of authority speaking as mere authority, if their only safeguard lies in the absence of necessity for struggle and effort, they cannot long hope to escape from the futility which surely awaits them in other directions.

I am thus led to take a wider range, and, leaving temporary and special considerations, to speak of a topic which is as yet beyond the pale of scientific orthodoxy, and which I might, more wisely, leave lying by the roadside. I will, however, take the risk of introducing a rather ill-favoured and disreputable looking stranger to your consideration, in the belief—I might say, in the assured conviction—that he is not all scamp, and that his present condition is as much due to our long-continued neglect as to any inherent incapacity for improvement in the subject.

I wish, however, strenuously to guard against its being supposed that this Association, in its corporate capacity, lends its countenance to, or looks with any favour on, the outcast. What I have to say—and after all, it will not be much—must rest on my own responsibility. I should be very sorry for any adventitious weight to attach to my observations on forbidden topics from the accident of their being delivered from this chair. The objection at which I have now hinted is the only one that seems to me to have any just weight, and on all other counts I am willing to incur such amount of opprobrium as naturally attaches to those who enter on a region where the fires of controversy are not extinct, and in which it is quite impossible, as well as undesirable, for everyone to think alike.

It is but a platitude to say that our clear and conscious aim should always be truth, and that no lower or meaner standard should ever be allowed to obtrude itself before us. Our ancestors fought hard and suffered much for the privilege of free and open inquiry, for the right of conducting investigation untrammelled by prejudice and foregone conclusions, and they were ready to examine into any phenomenon which presented itself. This attitude of mind is perhaps necessarily less prominent now, when so much knowledge has been gained, and when the labours of many individuals may be rightly directed entirely to its systematization and a study of its inner ramifications; but it would be a great pity if a too absorbed attention to what has already been acquired, and to the fringe of territory lying immediately adjacent thereto, were to end in our losing the power of raising our eyes and receiving evidence of a totally fresh kind, of perceiving the existence of regions into which the same processes of inquiry as had proved so fruitful might be extended, with results at present incalculable and perhaps wholly unexpected. I myself think that the ordinary processes of observation and experiment are establishing the existence of such a region; that, in fact, they have already established the truth of some phenomena not at present contemplated by science, and to which the orthodox man shuts his ears.

For instance, there is the question whether it has or has not been established by direct experiment that a method of communication exists between mind and mind irrespective of the ordinary channels of consciousness and the known organs of sense, and, if so, what is the process. It can hardly be through some unknown sense organ, but it may be by some direct physical influence on the ether, or it may be in some still more subtle manner. Of the process I as yet know nothing. For brevity it may be styled "thought-transference," though the name may turn out to be an unsuitable one after further investigation. Further investigation is just what is wanted. No one can expect others to accept his word for an entirely new fact, except as establishing a *prima facie* case for investigation.

But I am only now taking this as an instance of what I mean; whether it be a truth or a fiction, there is not, I suppose, one of the recognized scientific societies who would receive a paper on the subject.[1] There are individual scientific men who have investigated these matters for themselves; there are others who are willing to receive evidence, who hold their minds open and their judgment in suspense; but these are only individuals. The great majority, I think I am right in saying, feel active hostility to these researches and a determined opposition to the reception or discussion of evidence. And they feel this confirmed scepticism, as they call it, not after prolonged investigation, for then it might be justified, but sometimes after no investigation at all. A few tricks at a public performance, or the artifices of some impostor, and they decline to consider the matter further.

That individuals should take this line is, however, natural enough; they may be otherwise occupied and interested. Everybody is by no means bound to investigate everything; though, indeed, it is customary in most fields of knowledge for those who have kept aloof from a particular inquiry to defer in moderation to those who have conducted it, without feeling themselves called upon to express an opinion. Some there are, no doubt, who consider that they have given sufficient time and attention to the subject with only negative results. Their evidence is, of course, important; but plainly, negative evidence should be of immense bulk and weight before it can outweigh even a moderate amount of positive evidence. However, it is not of the action of individuals that I wish to speak, it is of the attitude to be adopted by scientific bodies in their corporate capacity; and for a corporate body of men of science, inheritors of the hard-won tradition of free and fearless inquiry into the facts of nature untrammelled by prejudice, for any such body to decline to receive evidence laboriously attained and discreetly and inoffensively presented by observers of accepted competency in other branches, would be, if ever actually done and persisted in, a terrible throwing away of their prerogative, and an imitation of the errors of a school of thought against which the struggle was at one time severe.

In the early days of the Copernican theory, Galileo for some years refrained from teaching it, though fully believing its truth, because he considered that he had better get more fully settled in his University chair before evoking the storm of controversy which the abandonment of the Ptolemaic system would arouse. The same thing in very minor degree is going on to-day. I know of men who hesitate to avow interest in these new investigations (I do not mean credence—the time is too early for avowing credence in any but the most rudimentary and definitely ascertained facts—but hesitate to avow interest) until they have settled down more securely and made a name for themselves in other lines. Caution and slow progress are extremely necessary; fear of avowing interest or of examining into unorthodox facts is, I venture to say, not in accordance with the highest traditions of the scientific attitude.

We are, I suppose, to some extent afraid of each other, but we are still more afraid of ourselves. We have great respect for the opinions of our elders and superiors; we find the matter distasteful to them, so we are silent. We have, moreover, a righteous mistrust of our own powers and knowledge; we perceive that it is a wide region extending into several already cultivated branches of science, that a many-sided and highly-trained mind is necessary adequately to cope with all its ramifications, that in the absence of strict inquiry imposture has been rampant in some portions of it for centuries, and that unless we are preternaturally careful we may get led into quagmires if we venture on it at all.

Now let me be more definite, and try to state what this field is, the exploration of which is regarded as so dangerous. I might call it the borderland of physics and psychology. I might call it the connection between life and energy; or the connection between mind and matter. It is an intermediate region, bounded on the north by psychology, on the south by physics, on the east by physiology, and on the west by pathology and medicine. An occasional psychologist has groped down into it and become a metaphysician. An occasional physicist has wandered up into it and lost his base, to the horror of his quondam brethren. Biologists mostly look at it askance, or deny its existence. A few medical practitioners, after long maintenance of a similar attitude, have begun to annex a portion of its western frontier. The whole region seems to be inhabited mainly by savages, many of them, so far as we can judge from a distance, given to gross superstition. It may, for all I know, have been hastily traversed, and rudely surveyed by a few clear-eyed travellers; but their legends concerning it are not very credible, certainly are not believed.

Why not leave it to the metaphysicians? I say it has been left to them long enough. They have explored it with insufficient equipment. The physical knowledge of the great philosophers has been necessarily scanty. Men of genius they were, and their writings may, when interpreted, mean much. But to us, as physicists, they are unsatisfactory; their methods are not our methods. They may be said to have floated a balloon over the region with a looking-glass attached, in which they have caught queer and fragmentary glimpses. They may have seen more than we give them credit for, but they appear to have guessed far more than they saw.

Our method is different. We prefer to creep slowly from our base of physical knowledge, to engineer carefully as we go, establishing forts, making roads, and thoroughly exploring the country; making a progress very slow, but very lasting. The psychologists from their side may meet us. I hope they will; but one or other of us ought to begin.

A vulnerable spot on our side seems to be the connection between life and energy. The conservation of energy has been so long established as to have become a commonplace. The relation of life to energy is not understood. Life is not energy, and the death of an animal affects the amount of energy no whit; yet a live animal exerts control over energy which a dead one cannot. Life is a guiding or directing principle, disturbing to the physical world but not yet given a place in the scheme of physics. The transfer of energy is accounted for by the performance of work; the guidance of energy needs no work, but demands force only. What is force? and how can living beings exert it in the way they do? An automaton worked by preceding conditions—that is, by the past—say the materialists. Are we so sure that they are not worked by the future too? In other words, that the totality of things, by which every one must admit that actions are guided, includes the future as well as the past, and that to attempt to deduce those actions from the past only will prove impossible.[1] In some way matter can be moved, guided, disturbed, by the agency of living beings; in some way there is a control, a directing-agency active, and events are caused at its choice and will that would not otherwise happen.

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[1] This, however, is mere conjecture. I am not aware that the experiment has been tried.

[1] The expression "controlled by the future" I first heard in a conversation with G. F. Fitzgerald, who seemed to consider it applicable to all events, without exception.

A luminous and helpful idea is that *time* is but a relative mode of regarding things; we progress through phenomena at a certain definite pace, and this subjective advance we interpret in an objective manner, as if events necessarily happened in this order and at this precise rate. But that may be only one mode of regarding them. The events may be in some sense existent always, both past and future, and it may be we who are arriving at them, not they which are happening. The analogy of a traveller in a railway train is useful. If he could never leave the train nor alter its pace, he would probably consider the landscapes as necessarily successive, and be unable to conceive their co-existence.

The analogy of a solid cut into sections is closer. We recognise the universe in sections, and each section we call the present. It is like the string of slices cut by a microtome; it is our way of studying the whole. But we may err in supposing that the body only exists in the slices which pass before our microscope in regular order and succession.

We perceive, therefore, a possible fourth-dimensional aspect about time, the inexorableness of whose flow may be a natural part of our present limitations. And if once we grasp the idea that past and future may be actually existing, we can recognise that they may have a controlling influence on all present action, and the two together may constitute "the higher plane," or the totality of things, after which, as it seems to me, we are impelled to seek, in connection with the directing of force or determinism, and the action of living beings consciously directed to a definite and preconceived end.

Inanimate matter is controlled by the *vis a tergo;* it is operated on solely by the past.[1] Given certain conditions, and the effect in due time follows. Attempts have been made to apply the same principle to living and conscious beings, but without much success. These seem to work for an object, even if it be the mere seeking for food; they are controlled by the idea of something not yet palpable. Given certain conditions, and their action cannot certainly be predicted; they have a sense of option and free will. Either their actions are really arbitrary and indeterminate—which is highly improbable—or they are controlled by the future as well as by the past. Imagine beings thus controlled: automata you may still call them, but they will be living automata, and will exhibit all the characteristics of live creatures. Moreover, if they have a merely experiential *knowledge*, necessarily limited by memory and bounded by the past, they will be unable to predict each other's actions with any certainty, because the whole of the data are not before them. May not a clearer apprehension of the meaning of life and will and determinism be gradually reached in some such direction as this?

By what means is force exerted, and what, definitely, is force? I can hardly put the question here and now so as to be intelligible, except to those who have approached and thought over the same difficulties; but I venture to say that there is here something not provided for in the orthodox scheme of physics; that modern physics is not complete, and that a line of possible advance lies in this direction.

I might go further. Given that force can be exerted by an act of will, do we understand the mechanism by which this is done? And if there is a gap in our knowledge between the conscious idea of a motion and the liberation of muscular energy needed to accomplish it, how do we know that a body may not be moved without ordinary material contact by an act of will? I have no evidence that such a thing is possible. I have tried once or twice to observe its asserted occurrence, and failed to get anything that satisfied me. Others may have been more fortunate. In any case, I hold that we require more knowledge before we can deny the possibility. If the conservation of energy were upset by the process, we should have grounds for denying it; but nothing that we know is upset by the discovery of a novel medium of communication, perhaps some more immediate action through the ether. It is no use theorising; it is unwise to decline to examine phenomena because we feel too sure of their impossibility. We ought to know the universe very thoroughly and completely before we take up that attitude.

Again, it is familiar that a thought may be excited in the brain of another person, transferred thither from our brain, by pulling a suitable trigger; by liberating energy in the form of sound, for instance, or by the mechanical act of writing, or in other ways. A prearranged code called language, and a material medium of communication, are the recognised methods. May there not also be an immaterial (perhaps an ethereal) medium of communication? Is it possible that an idea can be transferred from one person to another by a process such as we have not yet grown accustomed to, and know practically nothing about? In this case I have evidence. I assert that I have seen it done; and am perfectly convinced of the fact. Many others are satisfied of the truth of it too. Why must we speak of it with bated breath, as of a thing of which we are ashamed? What right have we to be ashamed of a truth?

And after all, when we have grown accustomed to it, it will not seem altogether strange. It is, perhaps, a natural consequence of the community of life or family relationship running through all living beings. The transmission of life may be likened in some ways to the transmission of magnetism, and all magnets are sympathetically connected, so that if suitably suspended a vibration from one disturbs others, even though they be distant ninety-two million miles.

It is sometimes objected that, granting thought-transference or telepathy to be a fact, it belongs more especially to lower forms of life, and that as the cerebral hemispheres develop we become independent of it; that what we notice is the relic of a decaying faculty, not the germ of a new and fruitful sense; and that progress is not to be made by studying or attending to it. It may be that it *is* an immature mode of communication, adapted to lower stages of consciousness than ours, but how much can we not learn by studying immature stages? As well might the objection be urged against a study of embryology. It may, on the other hand, be an indication of a higher mode of communication, which shall survive our temporary connection with ordinary matter.

I have spoken of the apparently direct action of mind on mind, and of a possible action of mind on matter. But the whole region is unexplored territory, and it is conceivable that matter may react on mind in a way we can at present only dimly imagine. In fact, the barrier between the two may gradually melt away, as so many other barriers have done, and we may end in a wider perception of the unity of nature, such as philosophers have already dreamt of.

I care not what the end may be. I do care that the inquiry shall be conducted by us, and that we shall be free from the disgrace of jogging along accustomed roads, leaving to outsiders the work, the ridicule, and the gratification, of unfolding a new region to unwilling eyes.

It may be held that such investigations are not physical and do not concern us. We cannot tell without trying. In that I trust my instinct: I believe there is something in this region which does concern us as physicists. It may concern other sciences too. It must, one would suppose, some day concern biology; but with that I have nothing to do. Biologists have their region, we have ours, and there is no need for us to hang back from an investigation because they do. Our own science, of Physics or Natural Philosophy in its widest sense, is the King of the Sciences, and it is for us to lead, not to follow.

And I say, have faith in the Intelligibility of the universe. Intelligibility has been the great creed in the strength of which all intellectual advance has been attempted, and all scientific progress made.

At first things always look mysterious. A comet, lightning, the aurora, the rainbow—all strange anomalous mysterious apparitions. But scrutinized in the dry light of science, their relationship with other better-known things becomes apparent. They cease to be anomalous; and though a certain mystery necessarily remains, it is no more a property peculiar to them, it is shared by the commonest objects of daily life.

The operations of a chemist, again, if conducted in a haphazard manner, would be an indescribable medley of effervescences, precipitations, changes in colour and in substance; but, guided by a thread of theory running through them the processes fall into a series, they all become fairly intelligible, and any explosion or catastrophe that may occur is capable of explanation too.

Now I say that the doctrine of ultimate intelligibility should be pressed into other departments also. At present we hang back from whole regions of inquiry, and say they are not for us. A few we are beginning to grapple with. The nature of disease is yielding to scrutiny with fruitful result; the mental aberrations and abnormalities of hypnotism, duplex personality, and allied phenomena, are now at last being taken under the wing of science after long ridicule and contempt. The phenomenon of crime, the scientific meaning and justification of altruism, and other matters relating to life and conduct, are beginning, or perhaps are barely yet beginning, to show a vulnerable front over which the forces of science may pour.

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[1] This is, of course, not assertion, but suggestion. It may be erroneous to draw any such distinction between animate and inanimate.

Facts so strange that they have been called miraculous are now no longer regarded as entirely incredible. All occurrences seem reasonable when contemplated from the right point of view, and some are believed in which in their essence are still quite marvellous. Apply warmth for a given period to a sparrow's egg, and what result could be more incredible or magical if now discovered for the first time. The possibilities of the universe are as infinite as is its physical extent. Why should we grope with our eyes always downward, and deny the possibility of everything out of our accustomed beat.

If there is a puzzle about free-will, let it be attacked; puzzles mean a state of half-knowledge; by the time we can grasp something more approximating to the totality of things the paradoxity of paradoxes drops away and becomes unrecognizable. I seem to myself to catch glimpses of clues to many of these old questions, and I urge that we should trust consciousness, which has led us thus far; should shrink from no problem when the time seems ripe for an attack upon it, and should not hesitate to press investigation, and ascertain the laws of even the most recondite problems of life and mind.

What we know is as nothing to that which remains to be known. This is sometimes said as a truism; sometimes it is half doubted. To me it seems the most literal truth, and that if we narrow our view to already half-conquered territory only, we shall be false to the men who won our freedom, and treasonable to the highest claims of science.

I must now return to the work of this Section, from which I have apparently wandered rather far afield, further than is customary—perhaps further than is desirable. But I hold that occasionally a wide outlook is wholesome, and that without such occasional survey, the rigid attention to detail and minute scrutiny of every little fact, which are so entirely admirable and are so rightly here fostered, are apt to become unhealthily dull and monotonous. Our life-work is concerned with the rigid framework of facts, the skeleton or outline map of the universe: and, though it is well for us occasionally to remember that the texture and colour and beauty which we habitually ignore are not therefore in the slightest degree non-existent, yet it is safest speedily to return to our base and continue the slow and laborious march with which we are familiar and which experience has justified. It is because I imagine that such systematic advance is now beginning to be possible in a fresh and unexpected direction that I have attempted to direct your attention to a subject which, if my prognostications are correct, may turn out to be one of special and peculiar interest to humanity.

THE LATE PROF. MARTIN DUNCAN, F.R.S.

WE have already announced the death of this well-known geologist; and now give a brief account of his services to science.

As a Fellow of the Royal, Linnean, Geological, and Microscopical Societies, and for some time President of the two last-named of these, it goes without saying that his attainments were of no mean order. Educated for the medical profession at King's College, London, he matriculated at the London University in 1841, taking honours in anatomy and physiology in 1844, and the degree of Bachelor of Medicine in 1846, in which year also he qualified as a Member of the Royal College of Surgeons. His early life was passed at Rochester with Dr. Martin, and at Colchester, where he was in practice for some years, and where he so won the esteem of all who knew him that he was elected Mayor of that city. Fascinated with the study of geology, and impressed with the idea that to make any mark in the scientific world a man should take up some *spécialité*, he not only obtained a broad grasp of his favourite subject, but devoted himself especially to a study of fossil corals and echinoderms, on which subjects at intervals he published numerous valuable memoirs. Indeed, for many years, and up to within a comparatively short period of his death, he continued to work at his special subject, and contributed many important papers to the *Annals and Magazine of Natural History*, the Journal of the Geological Society, the *Geological Magazine*, *Quarterly Journal of Microscopical Science*, the Philosophical Transactions and Proceedings of the Royal Society, the Proceedings and Transactions of the Zoological Society, and the Journal of the Linnean Society.

He soon found that residence out of London, away from scientific societies and important works of reference, was a great obstacle to work, and that if he was to make any real progress with his special studies it was absolutely necessary for him to seek some appointment in the metropolis. Fortunately for him, as it happened, the Chair of Geology at King's College became vacant, and he was appointed to fill it. This at once gave him the opportunity he had so long hoped for, and the preparation of his lectures proceeded side by side with much useful work, which, by degrees, he found time to publish. Such, for example, was his account of the Madreporaria collected during the expedition of H.M.S. *Porcupine*, which appeared in the Transactions of the Zoological Society (Part 1, vol. viii. p. 303, &c., and Part 2, vol. x. p. 235, &c.); his description of deep-sea and litoral corals from the Atlantic and Indian Oceans (Proc. Zool. Soc., 1876, p. 428, &c.); and his important revision of the Echinoidea, printed in the Journal of the Linnean Society, of which it occupied four numbers.

This was all strictly scientific work, but by no means represented all that he accomplished. As a popular exponent of the teaching of geology and zoology, especially in regard to the lower forms of life, he published many excellent articles which were designed to awaken an interest in subjects little investigated, though well worthy of attention.

Lucidly written and full of facts, these articles were at once instructive and suggestive, and from a teachers' point of view did more to educate youthful naturalists and encourage research than any of his more scientific papers, which, being of a more technical character, were less acceptable to the majority of readers because less intelligible to them.

Of this class were his articles on "Corals and their Polypes" (*Intellectual Observer*, 1869, pp. 81–91, 241–50, with two coloured plates); "Studies amongst Amœbæ" (*Popular Science Review*, 1877, with two plates), and "Notes on the Ophiurans, or the Sand and Brittle Stars" (*Popular Science Review*, 1878, with a plate).

His attention, however, was not confined to invertebrate zoology or geology. In 1878 he commenced the publication, in six volumes quarto, of a popular "Natural History," which had the merit of being written by a number of able specialists upon a comprehensive plan under his direction, and, while taking upon himself the laborious duties of editor-in-chief, he contributed many of the sections himself. Thus, while securing the co-operation of such well-known zoologists as the late Prof. W. K. Parker, the late Mr. Dallas, Prof. Seeley, Prof. Boyd Dawkins, Dr. H. Woodward, Dr. Murie, Mr. H. W. Bates, and Mr. R. B. Sharpe, he himself undertook the preparation of the articles on Apes and Monkeys, Lemurs (part), Edentata, Marsupialia, Reptilia, and Amphibia. He also wrote the introduction to the Invertebrata, and the articles Vermes, Zoophytes, and Infusoria which appeared in the last volume, published in 1883.

For an excellent summary of marine zoology, in which the appearance, structure, and habits of such animals and plants as may be found upon our coasts are well described, the reader may be referred to a little volume by Dr. Duncan, entitled "The Sea-shore." It forms one of a series of "Natural History Rambles," issued a few years since by the Society for Promoting Christian Knowledge, and, for the amount of information which it contains, as well as for its lucid expression, deserves to be better known.

Dr. Martin Duncan was undoubtedly one of the working bees in the great hive of science; and in his own quiet, unostentatious way has stored up a considerable amount of material the value of which will be more and more appreciated as those for whose benefit it was accumulated come to examine and understand it.

In his ardent devotion to science, and patient industry in spite of trials and troubles which would have deterred many less earnest workers, he set a bright example, which those of a younger generation of naturalists would do well to follow.

NOTES.

IT seems that those members of the Government, whichever they may be, who are responsible for buildings for science and art, have determined to erect new galleries for the Art Museum at South Kensington; practically to cover all the ground which is supposed to be applicable for art purposes there. These buildings are to cost some £400,000, and, when this money is spent, we suppose the South Kensington Art Museum will be finished. We suppose, also, that the building of a Science Museum will, by this action, be delayed for another twenty years. This will be a great victory for art, and will afford another interesting example of the results of the way in which matters scientific are managed in this country.

MR. EDGAR THURSTON, Curator of the Government Museum at Madras, has been appointed to officiate for two years for Dr. Watt, at Calcutta, in reporting on economic products and organizing collections of products and manufactures for the Calcutta and other Indian Museums; his duties at Madras being in the meantime discharged by Dr. Warth, of the Geological Department.

PROF. GOEBEL, of Marburg, has been appointed to the Chair of Botany at Munich in succession to the late Prof. Naegeli.

WE regret to announce the death of Dr. Weiss, the Professor of Botany and Director of the Plant-Physiological Institute of the University of Prague.

THE late Cardinal Haynald's important herbarium and botanical library has been placed in the National Museum at Budapest.

WE learn from Madras that the observations made under the direction of the late Mr. Pogson are in a forward state of reduction, and that the real activity of the Observatory is not to be measured by the fact that the last published volume of observations contains the record of those made in 1870. The funds at the disposal of the Madras Observatory have not permitted the regular and early publication of the masses of observations which the industry of Mr. Pogson and his assistants has accumulated, and the scheme which the Director proposed to himself did not permit him to give, from time to time, an abstract of his work through the ordinary and recognized channels open for the dissemination of astronomical results. Mr. Michie Smith writes that the "Variable Star Atlas" alone contains the observations of about 60,000 stars, made and reduced by Mr. Pogson. We may express an earnest wish that no long time may be suffered to elapse before astronomers have an opportunity of judging the value of this mass of material in an interesting branch of astronomical inquiry.

UNDER the McKinley *régime* it seems to be a very generous thing for an American *savant* to communicate a paper to a British society. One of them writes as follows to the *Nation*:—"A learned society of Scotland, in pursuance of its liberal policy, mailed to me fifty author's copies of a paper which had been honoured by admission to its Transactions. The bundle came to the local post-office this week opened, and accompanied by a slip giving the package a 'commercial value' of twelve dollars, and assessing a duty of 25 per cent. The local collector of customs thinks that I am resisting the just claims of a hard-working Government in delaying payment; but curiosity as to how they discover the commercial value of a paper whose real audience might, I think, be numbered on the fingers of the two hands, has led me to appeal the case."

Science states that the executors of the estate of the late William B. Ogden, the first Mayor of Chicago, have selected the University of Chicago as one of the beneficiaries, giving it a scientific school. The gift, which will amount to from three hundred thousand to half a million dollars, will endow a separate department of the University, to be called the Ogden Scientific School, its purpose being to furnish graduate students with the best facilities possible for scientific investigation by courses of lectures and laboratory practice. The income of the money appropriated is to be devoted to and used for the payment of salaries and fellowships, and the maintenance of laboratories in physics, chemistry, biology, geology, and astronomy, with the subdivisions of these departments. A large share of the time of the professors in the school is to be given to original investigation, and encouragement of various kinds is to be furnished them to publish the results of their investigations, a portion of the funds being set apart for the purpose of such publication.

IT seems as if in time the publishers of sea-side guides may realize that some people who require a holiday are intelligent, possess eyes, and perchance even some acquaintance with natural history. We have just received a copy of Johnson's illustrated "Visitors' Companion" to Eastbourne and its vicinity, which contains, besides the matter usually supplied, an account of the flora, consisting of 291 varieties of wild flowers, 9 orchids, 18 ferns, 12 mosses and their allies, 34 varieties of sea-weeds (with directions for collecting and preserving them); particulars are also given of 56 varieties of butterflies (with time of appearance), 45 varieties of moths (with time of appearance, and how to catch them by the electric light), 29 varieties of wild bees, pebbles, fossils, land and freshwater mollusca, a brief geological survey of the district, and an extensive list of wild birds which frequent the neighbourhood, together with a guide to fresh and salt water fishing. Have we to thank Prof. Huxley's local influence for this?

AN exhibition of the successes in acclimatization achieved in Russia will be opened at Moscow, in connection with the International Congresses of Zoology and Prehistoric Archæology and Anthropology which will be held in the Russian capital in August 1892. The results of the numerous experiments in acclimatization of a great variety of plants which have been made during the last twenty-five years, especially in the Asiatic dominions of the Empire, will be exhibited.

IN a Vice-Presidential Report to the U.S. National Geographic Society, on the "Geography of the Air," Lieut. A. W. Greely reviews the progress of meteorological science during the past year, chiefly with reference to the work of American meteorologists. Referring to the recent controversy on the causes of cyclones and anticyclones, he says:—"The status of the meteorological discussion which has been going on for some time seems to be this. A number of men, applying themselves to investigation in separate branches or stages of the same science, are attempting to reconcile their views, which, based as they are upon entirely different processes of investigation, are not entirely accordant. Some at least of these writers are still apparently groping in the preliminary, the 'natural history' stage of the science of meteorology, while one alone stands as the exponent of the 'natural philosophy' of meteorology." This view seems somewhat inappreciative, and the account given of Dr.

Hann's work inadequate and not quite correct. Dr. Hann's memoir demonstrated that the temperature conditions of anticyclones, and probably extra-tropical cyclones, are inconsistent with the convectional hypothesis as worked out by Prof. Ferrel, and he suggested as an alternative that their cause is to be sought in the general circulation of the atmosphere. But he did not originate this view, which had been put forward long before by Werner Siemens; nor did he attempt to develop it. It is incorrect, therefore, to represent this hypothesis as the main object of his memoir. In connection with the work of the Weather Bureau, of which Lieut. Greely is Director, he notices the experiments of Prof. Marvin on wind pressures and velocities, which confirm the results of some previous experimenters in proving that the indications of the Robinson anemometer are too high; also that pressures computed from velocities by the usual formula are much in excess of the truth; the result being that the pressure computed from the readings of the Robinson anemometer, when the actual velocity is sixty miles per hour, is 50 per cent. too high. Other subjects briefly noticed are Finley and Hazen's work in connection with tornadoes, and Prof. Russell's on cold waves.

IN a pamphlet entitled "Physical and Geological Traces of Permanent Cyclone Belts," Mr. Marsden Manson treats of a somewhat large subject in the small space of ten pages. Starting with the assumption that the main features of the barometric zones of the earth have been the same throughout past ages as they are at the present day, and that there has always been a belt in the north temperate zone, between 50° and 60° N. lat., which is the mean track of maximum cyclone frequency and low mean pressure, he infers that, owing to the diminished pressure, this has always been an axis of upheaval, and at the same time, owing to excessive precipitation, a zone of maximum denudation. His ideas are apparently suggested by the geological structure, the orographic and meteorological features of North America, and little or no attempt is made to verify his inferences by the geological and meteorological conditions of Europe and Asia, which hardly seem to bear out his hypothesis. Thus he instances the Archæan axis of Canada as the secular result of upheaval and denudation along an axis roughly coinciding with the average storm track; but he omits to show any similar relations between the Archæan rocks of Bohemia or the Alpine chain and the average course of storms in Europe. It is, however, altogether premature to criticize a theory put forward in so crude a stage of development, and it is hard to see what service can be rendered to science by such premature publication.

DR. W. DOBERCK has published the observations made at the Hong Kong Observatory in the year 1889. Returns were received from forty land stations, and extracts from logs of ninety-three ships which visited Chinese waters were collected during the year, and will be utilized in investigations of the meteorology and typhoons of the Eastern seas. The stations in connection with maritime meteorology extend to the Island of Luzon, and a most valuable station has been established on the Island of Formosa, by the Chinese Maritime Customs. The observations of the rain-band have been regularly continued, and have been found of use both in prediction of fine weather and of heavy thunderstorms. An advance Report issued for 1890 shows that considerable improvement in the storm-warning service has been effected by the connection of the Observatory with the telegraph offices. A committee of inquiry which sat in the early part of 1890, has recommended that more financial and other assistance be given to Dr. Doberck in carrying out his work.

THE Central Meteorological Office of Paris has recently published its *Annales* for the year 1888, consisting of three large quarto volumes. Vol. i. contains:—A discussion by M. Fron on the character of the thunderstorms of the years 1887 and 1888, with charts for each day on which such storms occurred; a review by M. Moureaux of the magnetic observations at Park of Saint Maur, together with *facsimile* curves of the most interesting disturbances. Owing to an agreement with Greenwich Observatory, the curves published in this country and in France will generally correspond to the same disturbances, and will therefore allow of interesting comparisons. *Résumés* of the magnetic observations made at 53 other stations in France are also published. A discussion by M. Angot of the phenological and other periodical phenomena during the years 1886 and 1887. These observations have now been continued for eight years. M. Angot has also studied the effect of the amount of cloud on the daily variation of temperature at Paris. A paper by M. Raulin on the seasonal rainfall of various countries in Europe, in which he shows that when a number of years are taken into consideration the condensation of vapour follows a regular seasonal range, with a minimum in winter and a maximum in summer, where the range is not interfered with by secondary causes, such as proximity to the sea, &c. M. Teisserenc de Bort presents a paper on the mode of formation of types of isobars, and on the theory of the general circulation of the atmosphere, illustrated by diagrams. Vol. ii. contains the observations made at various stations and mountain observatories, including also several stations in Algeria, Egypt, Panama, &c. Vol. iii. contains values of rainfall at a large number of stations, with monthly, seasonal, and annual charts. The actual number of stations reaches nearly 1800, and daily values are published for 925 stations.

A REMARKABLE weather change is reported to have occurred at Orenburg on November 19, 1890. After a temperature of 3° C., with heavy rain, there was a fall to – 30° C. in 20 minutes. Some thirty Kirghises, who were returning to Orenburg, were drenched with the rain, then frozen on their horses. Ten of them had been found, and the others were being sought for. Many horses and other animals succumbed to the cold.

SNOW-DRIFTS are found a serious disturbance of the Russian railway system. With a view to forecasting such occurrences, M. Sresnewskij has lately collected information about snow-drifts on the Russian lines during 1879–89 (*Rep. für Met.*). The drifts occur in the Northern and Eastern Governments, chiefly with south-west wind, but in Southern Russia with north-east. In the north, greater gradients are required than in the south. The maximum of the drifting is in mid-winter, but there is more in the second half of winter than in the first, that having more snow. In course of winter the snow grows in thickness, so that in March there is more to drift than in December. The marked diminution of drifting in February is due to the less wind in that month (a fact not yet explained, as the number of cyclones shows no decrease). Two kinds of drifting are distinguished; it may be only or chiefly snow lying on the ground that is whirled and carried along, or the wind may drive falling snow. There are most drifts in the months that have least snowfall and the smallest number of days of snow. The snow-drifts in South Russia with north-east wind are chiefly connected with anticyclones in the central region, or cyclones on the southern border; those in the east and north with cyclones in European Russia. In Central Russia they occur with cyclonic winds of various direction, seldom with anticyclones.

AN investigation (more comprehensive than the previous ones by Forel, Fritz, and others) of the variations of Alpine glaciers, has been recently made by Herr Richter, of the German and Austrian Alpine Club. To six advances of glaciers, previously known, he adds three, and his account of the six differs somewhat from previous ones. The dates of commencement of the nine advances are 1592, 1630, 1675, 1712, 1735, 1767, 1814, 1835, 1875 (?). The following are some of Richter's conclusions:—Glacier advances recur in periods varying between twenty and forty-five years; on the average of three centuries, thirty-five years. The advances are not all of equal intensity, nor alike in their progress. Nor is the intensity in a given advance-period the same in all glaciers. In the case of some glaciers, a period is occasionally skipped, the advance or retirement being very weak, so that the thirty-five years period gives place to one of seventy years. The glacier variations correspond, in general, with Brückner's climate variations. The glacier advance generally begins a few years after the moist and cool period has set in. There is no good reason to suppose that, in historic time, before the sixteenth century, the Alpine glaciers were smaller than now, or that variations occurred of different order and period from those of the last 300 years. About 1880, the earth was passing through a moist and cold period, which should have resulted in a general advance; but the advance has been but slight hitherto, and, in the Eastern Alps, mostly absent. The cause of this is not at present clear, but the mild nature of this last cold period may have something to do with it.

THE bacillus of tuberculosis, it is known, is often to be found in places lived in by consumptives. Herr Prausnitz has lately collected the dust in various compartments of trains which often convey patients from Berlin to Meran, and inoculated a number of guinea-pigs with it. Two, out of five compartments so examined, were found to contain the bacillus; the dust of one rendered three out of four guinea-pigs tuberculous; that of the other, two. The animals were killed after ten to twelve weeks, but in no case was the disease very advanced; the author supposes the number of bacilli to have been but small. The facts, however, seem to point to the necessity of disinfection of such railway carriages, especially the carpets or mats.

To the usual well-known ways of stimulating muscles to contraction, viz. electrical, thermal, mechanical, and chemical, M. D'Arsonval has recently added that by means of light. He could not, indeed, get any contraction in a fresh frog-muscle, when he suddenly threw bright light on it in a dark chamber; but having first in darkness stimulated a muscle with induction currents too weak to give a visible effect, and then suddenly illuminated the muscle with an arc light, the muscle showed slight tremulation. Not thinking this conclusive, however, M. D'Arsonval attached a muscle to the middle of a piece of skin stretched on a funnel, and connected the tube of the funnel by means of a piece of india-rubber tube with the ear. The muscle being now subjected to intense intermittent light, he heard a tone corresponding to the period of illumination, and this ceased when the muscle was killed with heat. Arc light was used, which was concentrated by a lens and passed through an alum-solution to stop the heat rays.

FOR nearly two years there has been at work in Denver, Colo., an automatic refrigerator system, which seems to be thoroughly successful. Ammoniacal liquor in the proportion of 29 parts pure ammonia to 71 parts water, is forced through a main to the point where refrigeration is desired; a sudden increase of space is afforded there for quick vaporization, and after absorption by water, the liquid returns by suction to the central station. There are two miles of mains having connection with twenty-nine boxes, each containing a grill near the top to which the liquor is admitted. The space formerly devoted to ice is a clear gain; and the temperature, instead of being a varying quantity, dependent on the arrival of the ice man, and never below 40° F., can be reduced to any degree above 25° F. in a few minutes, and kept within 2° of the same. The air is dry, sweet, and clean; the moisture collects on the grill as frost. In one experiment a piece of meat was kept six months and then cooked and eaten, and it seemed no way different from fresh meat.

THE French Société de l'Encouragement lately offered a prize of 1000 francs for conservation of potatoes and other vegetables. Four of the five applicants used some isolating substance (wood-ash, sawdust, rye-straw with sand). M. Schribaux, who gained the prize, puts potatoes for ten hours in a 1½ per cent. solution of commercial sulphuric acid to kill the buds (a 2 per cent. solution for thick skins). The potatoes are taken out and thoroughly dried, and they will keep without alteration more than a year. The same solution serves for repeated immersions, the concentration remaining constant. The process is not applicable to onions. Another prize by the same Society (3000 francs) is awarded to M. Candlot for a memoir treating of the action of sea-water on cements. He shows that the sulphate of lime resulting from decomposition of sulphate of magnesia by lime-salts of the cement combines with aluminate of lime to give a double crystalline salt containing half its weight of water. The crystallization of a salt so greatly hydrated involves considerable swelling, and this accounts for the disaggregation of cements in marine work. M. Candlot has observed the curious fact that over-baked lime, which takes several days to extinguish in water, is extinguished in a few minutes in a 3 per cent. solution of chloride of calcium. This is thought to have important practical bearings.

M. RASPAIL has lately called attention, in the Zoological Society of France, to the serious diminution of birds in that country through destruction of their nests. Some insectivorous species are becoming very rare, while the ravages of parasites on useful plants are extending. Boys, of course, do a great deal of the mischief; and of the various animals which attack nests (the squirrel, the hedgehog, the dormouse, the magpie, &c.) M. Raspail regards the cat as the worst offender. On a recently-wooded property of about 7 acres he observed last year as follows:—Out of 37 nests, carefully watched, only 8 succeeded; 29 were destroyed, 14 of these by the cat, though effort had been made to ward off this insatiable marauder. On a large property in the centre of a village the owner had about 80 cats annually caught in traps. The place having lately changed hands, the gardeners estimate that more than 100 nests were destroyed last year, three-fourths of these by cats. M. Raspail advocates a rigorous application of the law for protection of insectivorous species, the disqualification of the cat as a domestic animal, and the giving of prizes to foresters and others for destruction of all animals which prey on eggs and young in the nest.

TOBACCO fermentation, a very essential process, is brought about by firmly packing ripe tobacco in large quantities. It had been generally supposed that the fermentation is of purely chemical nature, but Herr Suchsland, of the German Botanical Society, finds that a fungus is concerned in it. In all the tobaccos he examined, he found large quantities of fungi, though of only two or three species. Bacteriaceæ were predominant, but Coccaceæ also occurred. When they were taken and increased by pure cultivation, and added to other kinds of tobacco, they produced changes of taste and smell which recalled those of their original nutritive base. In cultivation of tobacco in Germany it has been sought to get a good quality, chiefly by ground cultivation, and introduction of the best kinds of tobacco. But it is pointed out that failure of the best success may be due to the fact that the more active fermenting fungi of the original country are not brought with the seeds, and the ferments here cannot give such good results. Experiments made with a view to improvement on the lines suggested have apparently proved successful.

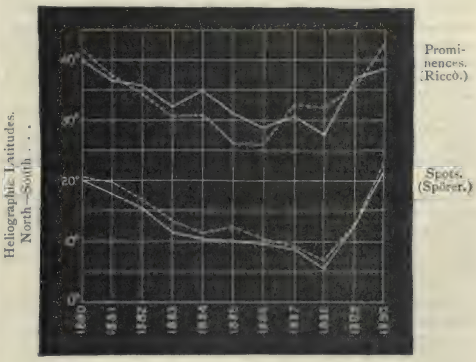
A profitable industry, little heard of, is carried on among the hills of Connecticut (*Sci. Am.*). It is the manufacture of birch oil, which is used largely for confectionery, and gives a perfect wintergreen flavour. There are eight mills in the State—the first built only ten years ago. Birch brush, without foliage, and not over 2½ inches in diameter, from the black, mountain or sugar birch (not the yellow or white), is chopped up and boiled with water in tanks. The steam, passing through an iron pipe near the top, is condensed in a coil immersed in running water, and drops into a glass jar. The oil is much heavier than water, and in the crude state is of copper hue. The mills work only from October to April. A good deal of adulterated birch oil is used in tanning leather to imitate Russia leather.

We have just received the Report for 1890 of the Botanical Exchange Club of the British Isles. There are about fifty members, and a list of the plants that are wanted is sent out every spring. The Secretary is Mr. Charles Bailey, Ashfield College Road, Whalley Range, Manchester. The distributor for last year was the Rev. E. T. Linton, one of our most painstaking British botanists, and the Report is edited by him. The number of specimens received was 4100, from twenty-six contributors. The most interesting novelty of the year is an Arenaria found at the head of Ribblesdale, in Yorkshire, which is nearly allied to, but not quite identical with, *A. norvegica*, known only within the British area in the Shetland Islands and Orkney, and *A. ciliata*, known only in County Sligo. Mr. Linton treats it as *A. gothica*, Fries., but that plant is an annual, whilst the Ribblesdale plant is a perennial. It is, in fact, a form about half-way between *norvegica* and *gothica*. Out of thirty-eight pages of the Report, eleven are occupied by Rubi. A new general working up of the British Rubi is much wanted, and it is evident the different referees to whom the specimens have been sent do not use some of the names with the same application or range of significance. What beginners want are good typical specimens of the most distinct forms. To give them the intermediate connecting links before they know thoroughly the typical sub-species only bewilders them. In roses the difficulty is that it is often impossible to determine a given plant positively without seeing it in three stages—flower, young fruit, and mature fruit—and nearly all the specimens sent to the Club arrive in a single stage. The above remark applies to *R. mollis* and *tomentosa*, concerning which there are eleven paragraphs in the Report, none of which tend to any real enlightenment. To Hieracia the same remark applies as to Rubi; but Mr. F. A. Hanbury's elaborate monograph, now fairly started off, will put this right. Three other sets of plants are at present receiving much attention from the members, *i.e.* hybrid willows, hybrid Epilobia, and Potamogetons. At the end of the Report there is a long list of new county records.

The additions to the Zoological Society's Gardens during the past week include a Brown Capuchin (*Cebus fatuellus* ♀) from Guiana, presented by Miss Phyllis Duncan; a Red-bellied Squirrel (*Sciurus variegatus*) from Trinidad, a Golden Agouti (*Dasyprocta aguti*) from Guiana, a West Indian Agouti (*Dasyprocta cristata*) from the West Indies, two Violet Tanagers (*Euphonia violacea*) from Brazil, presented by Mr. R. J. L. Guppy, C.M.Z.S.; a Common Otter (*Lutra vulgaris*), British, presented by Mr. D. E. Cardinall; a Marbled Polecat (*Putorius sarmaticus*) from Quettah, presented by Colonel C. Shepherd; a Vulpine Squirrel (*Sciurus vulpinus*) from North America, presented by Miss Pickford; seven Lemmings (*Myodes lemmus*) from Norway, presented by Mr. T. T. Somerville; two Sparrow-Hawks (*Accipiter nisus*), British, presented by Mr. Digby F. W. Nicholl, F.Z.S.; a Grey Parrot (*Psittacus erithacus*) from West Africa, presented by Mrs. Hale; a Golden Eagle (*Aquila chrysaëtus*), European, presented by Captain Taylor; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, a Dwarf Chameleon (*Chamæleon pumilus*) from South Africa, presented by Captain Wood; two Common Chameleons (*Chamæleon vulgaris*) from North Africa, presented by Mr. E. Palmer; an Egyptian Ichneumon (*Herpestes ichneumon*) from Spain, a Black-headed Caique (*Caica melanocephala*) from Demerara, deposited; a Yak (*Poëphagus grunniens*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Periodic Variations in the Latitude of Solar Prominences.—From a paper by Prof. Riccò, in *Comptes rendus* for August 3, it appears that the mean latitude of solar prominences varies periodically in the same way as that of spots. During the last eleven years observations of the form, position, and dimension of solar prominences have been made at Palermo on 2207 days, with the same refractor and spectroscope. In this period 7663 prominences have been observed, having a height equal to or greater than 30″. Neglecting a few irregularities, the observations show that about the time of maximum solar activity prominences occur nearest the sun's equator; the mean latitude for both hemispheres in the second year after the last maximum being 27°·5. There is then a rapid general increase in the latitude of most frequent occurrence up to the minimum epoch, the mean latitude for both hemispheres in the year following the last minimum—that is, in 1890—being 41°·3. In other words, up to the commencement of the minimum period prominences approach the equator. They then appear in high latitudes, to descend again to the equator in an eleven-year cycle. The intimate relation that exists between this variation and that observed in the distribution of spots is evident from an inspection of the accompanying figure, which represents the mean latitudes of spots according to Prof. Spörer's observations, and those found for prominences by Prof. Riccò. The pairs of like curves run almost parallel to each other, and are separated by an approximately equal number of degrees at all points. It is worthy of remark that the photographs of the solar corona recently investigated by Prof. Bigelow exhibit a movement in latitude which is most probably connected with the latitude variations of sun-spots and prominences.



Photography of Solar Prominences and their Spectra.—In the *American Journal of Science* for August, and *Astronomische Nachrichten*, No. 3053, Prof. G. H. Hale gives some results which he has obtained in solar prominence photography, utilizing the methods noted in Nature, vol. xliii. p. 133. With the fourth-order spectrum of a grating having 14,438 lines to the inch, and both radial and tangential slits, the broad H and K lines invariably have bright lines running through them, apparently to the top of every prominence. This is an important fact, for the position of H and K in the spectrum makes it unnecessary to stain the photographic plates, or prolong the exposure, as would be the case if the C line were employed; and their characteristic banded appearance renders them peculiarly useful as backgrounds for the bright prominence lines, and allows the use of a wide slit. Working with a tangential slit, Prof. Hale has obtained excellent photographs of reversals of H and K. The former line is found to be double, the companion being about 1·5 tenth-metres less refrangible, and possibly coincident with a line of hydrogen at λ 3970·25. The photographs also show three bright lines, which appear to be coincident with the lines α, β, and γ of the hydrogen series. The first of these is seen as a double line, the components of which are separated by a fraction of a tenth-metre.

It is highly probable that a large number of prominences cannot be made out by the ordinary method of observing the C line. These invisible or "white" prominences must therefore be detected photographically. But as it would be an extremely troublesome process to take a set of photographs with the slit tangential to various points on the limb, and as prominences having a considerable elevation could not be easily photographed by this method, another arrangement has been devised which nullifies these objections, and allows eye observations of C to be made while the exposure to the H and K region is going on. Certainly, if Prof. Hale should be able to do for invisible prominences what has been done at Palermo for those visually observable, our knowledge of the relation between the two classes of phenomena and their connection with sun-spots would be considerably extended.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE following is the list of candidates successful in the competition for the Whitworth Scholarships and Exhibitions, 1891:—(1) Scholarships, £125 a year each (tenable for three years):—Robert W. Weekes, electrical engineer; William G. Rennie, engineering student; Thomas G. Jones, engineer; William H. Pretty, mechanical engineer. (2) Exhibitions, £50 a year each (tenable for one year):—Julian J. King-Salter, student; Louis Martineau, engineer; Harold R. Cullen, engineer apprentice; Frederick Hossack, mechanical engineer; William A. Lelean, engineering draughtsman; William F. Nixon, engineer; John Chambers, draughtsman; Joseph W. Kershaw, student; Charles H. Gadsby, engineer's draughtsman; Frederick Charles Lea, apprentice millwright; George Thomas White, mechanic; Joseph H. Gibson, marine engineer; Henry Fowler, engineer apprentice; Arthur E. Malpas, engine fitter apprentice; James Hall, student; Walter E. Lilly, engineer; Charles Jefcoat, Jun., turner; Percy V. Vernon, fitter; George E. Armstrong, engineer student; Martin DeVille, draughtsman; Richard H. Cabena, marine engineer's draughtsman; Frederick Dodridge, engine fitter; Alfred J. Ward, mechanical engineer; William E. Tubbs, coachmaker; Alexander Norwell, mechanical engineer; Richard Baxendale, draughtsman; Walter Amor, fitter; Thomas Bouts, engineer; Alfred Meyer, draughtsman; John W. Anderson, draughtsman.

The list of successful candidates for Royal Exhibitions, National Scholarships, and Free Studentships, 1891, is as follows:—National Scholarship for Biological Subjects—George S. West, student. National Scholarship for Chemistry and Physics—James Bruce, student. National Scholarship for Mechanics—Sydney G. Starling, student. National Scholarships—Charles H. Sidebotham, student; Bernard E. Spencer, student; James H. Smith, pattern maker; John Ball, engineer; Charles Harold Robinson, tobacconist; George W. Fearnley, student; Charles J. Gray, student; Francis Carroll, student; Ralph M. Archer, teacher; Harry Verney, fitter; James Thompson, teacher. Royal Exhibitions—Hubert Cartwright, student; Walter H. Watson, laboratory assistant; Sidney G. Horsley, student; Charlie R. Cross, student; Watson Crossley, cotton weaver; Samuel D. Crothers, farmer; Peter Pinkerton, student. Free Studentships—David Baxandall, student; Herbert C. Robinson, student; William G. Freeman, student; Charles H. Gadsby, engineer's draughtsman; Stephen Pace, none; William H. Dolman, teacher.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 10.—M. Duchartre in the chair.—Artificial production of a micaceous trachyte, by MM. F. Fouqué and Michel Lévy. This trachyte was obtained by the action of water under pressure on a glass resulting from the fusion of Vire granite, and at a bright red heat. The rock was homogeneous, and its sections exhibited beautiful octahedral crystals of a variety of spinel in connection with orthoclase and black mica.—Note on an experiment on ostriculture that has been carried out in the fish-pond of the Roscoff Laboratory, by M. H. de Lacaze-Duthiers.—Physiological research on carbon monoxide in a medium containing it in the proportion of one ten-thousandth, by M. N. Gréhant. After passing a mixture containing a ten-thousandth part of carbon monoxide through blood for half an hour, it was found that the respiratory capacity of the blood was diminished from 23·7 to 23·0 per cent. The difference (0·7) represents the amount of oxygen replaced by carbon monoxide. When the gas was passed through under a pressure of five atmospheres, it was found that the respiratory capacity had diminished from 23·7 to 17·2. This result may be applied to the detection of small quantities of carbon monoxide in confined air, and it also indicates that it is not only the percentage proportion of the gas which must be considered in questions relating to the absorption of it by hæmoglobin, for this remained the same in both experiments, viz. $\frac{1}{10000}$th.—On the refraction and dispersion of crystallized chlorate of soda, by M. Frantz Dussaud. The author has measured with five different instruments the refractive index of chlorate of soda at temperatures between 0° and 30°, and for twelve lines in the spectrum. For the sodium line (D) and a temperature of 20° the value obtained is 1·51510. The result for *a* is 1·50197, and for Cd (18) 1·58500.—On the habits of *Gobius minutus*, by M. Frédéric Guitel.—On the pathological types of the curve of muscular action, by M. Maurice Mendelssohn.—On the preventive inoculations of yellow fever, by M. Domingos Freire. The author has inoculated 10,881 persons with cultures of *Micrococcus amaril.* The mortality of those so vaccinated was 0·4 per cent., although the patients lived in districts infected with yellow fever, whilst the death-rate of the uninoculated during the same period was from 30 to 40 per cent. These results have led the Government of the Brazilian States to found an institute for the culture of the virus of yellow fever and other infectious diseases, and to appoint M. Freire the director.—On a new incandescent light, by M. Bay.

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THURSDAY, AUGUST 27, 1891.

THE CONGRESS OF HYGIENE.

WE continue this week our account of the work done at this Congress. It will be clear that with the space at our disposal it is only possible to refer to few among the many subjects discussed. Among these we have selected those which have the closest connection with those researches now attracting special attention.

In regard to the subject of tuberculosis it was certainly a happy inspiration of the officials of the Bacteriological (II.) and Comparative Pathological (III.) Sections of the International Congress of Hygiene and Demography, to call a joint meeting in order that a full discussion of the scientific and practical bearings of the questions relating to "the transmission of tuberculosis from animals to man by means of flesh and milk derived from tuberculous animals" might be possible; and it was also fortunate, as far as its success was concerned, that the discussion was opened by Profs. Burdon Sanderson and Bang, each of whom in his own sphere is singularly well fitted to lay before the members of the Sections what is at present known in the medical and veterinary scientific worlds concerning this important subject. Prof. Sanderson's early researches on tuberculosis have opened up the way for much of our present knowledge on the subject, in addition to which he has watched the question most carefully through its various stages of evolution; whilst Prof. Bang, by his numerous practical observations and scientific experiments, has given a completeness to our knowledge which has not been attained as the outcome of the work of any other observer.

The discussion on this question afforded another instance of the intimate connection between the purest research and the most practical affairs of every-day life.

Thus from the tenor of the discussion it may be gathered that the danger arising from the ingestion of tuberculous milk and meat has probably been exaggerated.

Some of those who took part in the discussion, for example, seemed to doubt whether primary tuberculosis of the alimentary canal—*i.e.* tuberculosis confined to this region and evidently the result of infection through the mucous membrane—was ever met with in adults, and even whether it was of very frequent occurrence in the child; whilst other speakers were able to instance out of their own experience certain cases of the former and many of the latter, strongly accentuating the fact that such primary disease of the intestinal canal does exist. Then, again, one speaker was convinced that Koch's bacillus had little or nothing to do with the production of tubercular disease; but the contention had been met by so many accurate observations and experiments that he may be said to have been ruled out of court, though it was on all hands agreed that the bacillus might be helped in its work by various predisposing causes, many of which were brought into full prominence during the discussion. It was also accepted that the tuberculosis of cattle is similar, as regards its causal agent, to the tuberculosis of the human subject, and that the disease is merely apparently modified owing to the different conditions, and perhaps delicate tissue modifications, offered by the different hosts of the parasitic bacillus; and from the most careful and detailed experiments, of which a large number were described, there seems to be no question that tuberculosis is communicable from animals to man, and certainly there appears to be none that it is communicable in the opposite direction.

There was a general expression of opinion as the outcome of the discussion that legislation of some kind or other is necessary, but, as pointed out by Burdon Sanderson, if laws were made to-morrow there is absolutely no staff of inpectors capable of giving effect to any that might be drafted. It is probable that this will draw attention, first, to the necessity for conferring powers of inspection of dairy and store cattle on some central authority; and second, to the necessity there is that our veterinary surgeons should undergo a thorough scientific and practical training, such as would fit them to fill the posts from which unfortunately they are necessarily now in many instances excluded.

When all is said and done, it appears that the danger arising from the consumption of tuberculous meat is far less serious than that involved in the consumption of milk from tuberculous animals, as meat, if *thoroughly* cooked, appears to be perfectly innocuous, the tubercle bacilli being readily destroyed by heat, whilst the nutrient properties of the meat itself are little, if at all, interfered with by judicious cooking. In the case of milk, however, in which the presence of tubercle bacilli has been so often demonstrated, it has to be borne in mind that boiling so alters the constituents of the milk, especially the proteids, that it is rendered very much less digestible; and its nutritive value is greatly interfered with.

We now pass to the discussion.

TUBERCULOSIS IN ALL ITS RELATIONS.

Prof. Burdon Sanderson said the subject which he had undertaken to bring before the notice of the conjoint Sections for discussion was one of the gravest importance, for there was no disease, acute or chronic, which was so productive of human suffering or so destructive of human life. In a Congress of Hygiene the subject of tuberculosis could only be considered in relation to its causes, the aim of hygiene being to prevent disease, not to cure it. He wished specially to direct attention to those questions which relate to the dangers which are alleged to arise from the use of tuberculous food. (1) Does general tuberculosis in man originate from intestinal infection? (2) If it does, is it possible to guard against so fearful a danger? For the purpose of avoiding useless discussion on subjects on which there ought to be perfect agreement of opinion, he asked that certain fundamental propositions should be accepted as settled; such as, for example, the existence of a *materies morbi* in the form of the tubercle bacillus, its constant association with the tuberculous process, and the identity of human with bovine tubercle; and also that it be assumed that any part of the body of a tuberculous animal or any secretion of such an animal would, if it contained tubercle bacilli, be a source of danger, and that the use of such liquid or part ought to be prohibited or avoided. This being understood, we were in a position to enter on the questions which require answers, some of which are pathological or etiological, the others practical or administrative. The etiological questions might be said to relate to the three possible ways in which a human being may be infected by tubercle—namely, inheritance, pulmonary inhalation (atmospheric infection), and food (enteric infection). The practical issues were—

(1) Is the risk to the individual consumer of such a nature that it can be detected and estimated?

(2) Is it of such a nature that it can be counteracted?

(3) Is the collective risk to which the community is exposed sufficient to demand the interference of the State? and

(4) If it is, How can the State interfere with effect?

Of the two practical questions which relate respectively to infection by milk and to infection by meat, the latter was very largely discussed at a Congress on the subject of tuberculosis held in Paris in 1888, and has again been discussed very recently. In the first of these debates the medical profession did not take a very prominent part. The question whether the flesh of tuberculous animals is dangerous or not was regarded chiefly from the point of view of the veterinarian.

In 1888, M. Arloing, following out the principles enunciated by another gifted pathologist, the late M. Toussaint, that tubercle is a disease *totius substantiæ corporis*, maintained that the time had come to act "conformant à la logique." One out of every six carcasses had been shown, he said, to be infective, when tested by administering it to test animals as food. He calculated that over one thousand persons joined in the consumption of every such carcass, and consequently that one-sixth of this number—that is, about 170 persons—must be subjected to the risk of infection for every animal sent to the shambles. If this reasoning were true, if we could measure the danger to the human consumer by the presence of tuberculosis among animals used for food irrespectively of other considerations, then M. Arloing was right in his practical deduction from it that whatever interests conflict with public health they must give way. It was our duty to insist on the right of science to dictate; but in doing so it was necessary to be careful not to do so until the question had been looked at from all sides and the whole evidence had been heard.

In some of these discussions it had not been sufficiently considered that the question was not whether the consumption of tuberculous meat was in itself attended with risk, but whether the presence of tuberculous diseases among ourselves was in any way due to the fact that we occasionally eat meat which contained bacilli. It was not sufficient to show that on the one hand there was a fearful mortality from tuberculous diseases, and that on the other there existed a cause to which this calamity might be attributed. It must also be shown that the effect was actually produced by the cause, in such sense that if the cause were removed we might hope that the effect would disappear.

Twenty-three years ago Chauveau fed three heifers with tuberculous material from the body of a cow and obtained positive results. At that time the idea that tuberculosis was a virulent disease was new. M. Villemin had made his great discovery, but it had not yet been accepted, and consequently Chauveau's results were severely criticized, and were the subject of much discussion, which extended over several years (1868–74), during which he repeated his observations, effectually silenced his opponents, and determined with the greatest exactitude all the conditions which are required to insure success in the experimental production of tuberculosis by feeding. Gerlach about the same time made similar experiments in Germany which led him to advocate in the most energetic manner the restriction of the sale of tuberculous meat.

These two initial investigations were followed by many others. In 1884, Baumgarten showed that a couple of ounces of milk to which a pure culture of tubercle bacillus had been added were sufficient to produce characteristic tuberculosis in the intestines of a rabbit; and that the effect of such feeding was so constant that by examining the animals so fed at successive periods all the stages of the process could be thoroughly investigated, the most important result being that after a period of latency of a fortnight, during which no traces of infection were visible, the lymphatic follicles of the mucous membrane and the mesenteric glands began to enlarge simultaneously without any change whatever in the intestinal epithelium.

It was thus shown with a precision which was not before obtainable that the initial phenomenon of tuberculosis was primarily a proliferation of the adenoid tissue of the lymphatic system, and that the bacillus was capable of finding its way into the lymphatic system without leaving behind it any appreciable traces of its presence at the portals by which it had gained admission. Since 1884 our knowledge of the subject had been still further advanced by Cornil, under whose direction two very important researches, confirming and extending Baumgarten's results, have been recently published, from which it was evident that when the tubercle bacillus is absorbed from the intestine it follows the course of the lacteals, and that the lesions which it produces correspond closely with those which present themselves in those rare instances in which it is possible to observe the first beginnings of enteric tubercle in the human subject.

Much, however, has still to be learned by the experimental method—information which could only be gained by observations on animals. According to those who regard tuberculosis as necessarily a disease, *totius substantiæ corporis*, in which every part of the body is contaminated, all meat derived from the body of a tuberculous animal ought to be condemned, whether it appears healthy or not, for they argue that in every such animal, however localized the disease may be, bacilli circulate in the blood, and are so universally distributed.

Prof. Sanderson believed that this was not true, and that we are not entitled to assume that the flesh of every tuberculous animal is infectious unless it be proved to be so. As against the probability of its being so, it must be noted that the tuberculosis of cattle, although the product of the same bacillus as the tuberculosis of man, is a disease of comparatively slow progress. It localizes itself in structures which are not essential to life, and nutrition might be so little interfered with that the animal could be readily fattened for the market. There was no doubt that the flesh of such animals might be to all appearances in good condition, and might be offered for sale as meat of prime quality, and as yet we have no evidence that it is infective.

Turning from the source of infection to its effects, from the bacillus to its field of disease and death-producing action, Prof. Sanderson said that tuberculous diseases contribute something like 14 per cent. to the total of deaths from all causes, and that during childhood, as distinguished from adult life on the one hand and from infancy on the other, tuberculous mortality scarcely amounts to a quarter of this percentage, whereas in infancy it only falls a little short of it, and in early adult life, it very far exceeds it.

There was evidence that under certain conditions the virus of tubercle was absorbed by the lymphatic system from the small intestine in man, and that when this happens it may give rise to lesions of the same nature as those produced in animals by the injection of liquids in which bacilli are suspended—that is, to lesions which originate in the lymphatic system. Tuberculous disease of the intestinal mucous membrane, although very common, never occurred in the adult and very rarely in infancy as a primary disease. In the adult it might occur as an ulterior consequence of pulmonary consumption, the way in which it occurred being very evident. In the advanced stages of that disease muco-purulent liquid was discharged in quantity from the softened parts. This material charged with virulent bacilli might infect the mucous membrane along which it passed so that it is easy to distinguish bronchi which lead from vomicæ by the tuberculous nodules with which they are more or less beset. In advanced phthisis the sputum is so abundant that a certain proportion of it is from time to time swallowed. No effect is produced in the œsophagus or stomach, for along the former it passes too rapidly, while in the latter the mucous membrane is effectually protected by the gastric juice, which, although incapable of devitalizing the bacillus of tubercle, arrests its development. In the alkaline contents of the small intestine a condition more favourable to its development was found, and from there it was absorbed, just as any other particle of similar size might be, by the lymphatic follicles. Tuberculous disease of the small intestine in the adult thus occurred. It was always a secondary result of pulmonary phthsis.

In childhood the case is different. Tuberculosis does not begin to assert itself as a cause of death until the third month of extra-uterine life, but after this there was good reason for supposing that the bacillus plays an important part as a cause of mortality.

To the pathologist the question of how latent tuberculosis of the lymphatic system or of bone originates, *i.e.* how the bacilli which produce them are introduced into the blood stream was one of great interest. Prof. Sanderson confessed it to be his belief that in a certain proportion of cases the cryptogenetic tuberculoses were due to causes which operate before birth. From Dr. Muller's Munich statistics it might be gathered that in less than half of the cases in which the lymphatic glands are found to be tuberculous the affection has its seat in the mesentery, and that the mucous membrane of the intestine is tuberculous in a still smaller proportion—less than a quarter. In many of these cases the mucous membrane was no doubt affected subsequently on tuberculous disease of the lungs, but in the remainder the disease seemed to be primary. If it could be proved that such cases were primary, the fact would afford clearer evidence than any we now possess of the enteric origin of tuberculosis.

In the absence of such proof, human pathology had very little indeed to say in favour of the belief that human tuberculosis could owe its origin to the consumption of tuberculous food, and even if it were proved that the absorbents afforded a channel of entry for the tuberculous virus in children it would have little significance as regards the consumption of meat.

The author held, therefore, that we are not as yet in a position to demand the interference of the State on the ground that the community actually suffers from the consumption of tuberculous meat, the evidence that it is so being too weak to be insisted on; but he maintained that the consumption of tuberculous meat was attended with some danger, and that on that ground its consumption ought to be prevented by the State and avoided by the individual.

As regards the administrative question, he held that if we had, to-morrow, a law forbidding the sale of any meat containing the bacillus of tubercle, it could not be carried out unless those charged with its administration were able to distinguish such contaminated meat from healthy meat, so that the efficiency of the law would depend on the question whether the art of discriminating between infecting and non-infecting meat had attained to such perfection as to enable an adequately trained inspector to exercise his function with effect. The practical result to which we have come was this. Everything must turn on diagnosis. The Legislature might direct that all meat intended for consumption should be subjected to inspection, might appoint inspectors, impose penalties, and provide just and adequate compensation, but all this would be of no use unless the principles on which the discrimination of infecting from non-infecting meat is to be founded could be laid down, and the services of skilled persons of sufficient intelligence to apply them could be secured. We might consider it quite certain that in this country at least it would at present be extremely difficult to find such persons. Not that the veterinarian was less capable than the doctor of making a scientific investigation, but that he does not possess, and has, as yet, had no opportunity of acquiring, the sort of skill which is necessary for making what the French call the *diagnose précoce* of tuberculosis. Two things in short are required, neither of which we have at our disposal—special scientific knowledge and technical skill, and the former of these must be acquired first. Science must determine, much more definitely than has been done as yet, what are the earliest changes which have their seat in the parts of animals used for food, and which of these might indicate danger to the consumer. This knowledge could only be acquired by experiments specially made for the purpose, and having been attained it could only be applied by technically trained persons. He illustrated the sort of skill required by comparing it to that possessed by the professional tea taster as regards the commercial value of tea. Why was the judgment of the expert reliable? Because he was responsible for it and was paid for it. It would be the same as regards the early recognition of tubercle in cattle, if skill and discrimination were paid for; and the same moment that this skill was required it would come into existence. What would be wanted in the inspector was not that he should be a pathologist or even a bacteriologist, but a trained expert; for although the rules unconsciously used by him might be based on scientific principles, it is not by these principles he is guided in each case, but by practical skill.

Dr. Sanderson then submitted the following propositions to the meeting of the combined Sections:—

(1) That tuberculosis must be added to the list of diseases regarded by the law as contagious. There is no sufficient reason for supposing that in the human adult the introduction of the bacilli of tubercle by enteric absorption is the efficient cause of tuberculosis. In infancy a large proportion of the apparently idiopathic tuberculous diseases of the lymphatic system are probably due to the penetration of bacilli into the organism from the intestine; but the evidence which we at present possess on this subject is not sufficiently precise or extended to serve as a basis for prophylactic action. For this reason the origin of tuberculosis in infancy is a subject which urgently requires investigation.

(2) It has been proved that the ingestion of any material which contains the bacilli of tubercle is a source of risk to the consumer, but the conditions which limit this risk are insufficiently known. It would, therefore, be unjust to enforce the destruction of any specimen of meat apparently healthy, even though it were known to be derived from a tuberculous animal, excepting on evidence given as regards the particular case that it would be infecting if administered to test animals.

(3) As regards the duty of the State in relation to the prevention of tuberculosis, what is immediately required is that an efficient system of skilled inspection should be created. This is desirable, not merely as a first step towards a prevention of the sale and consumption of tuberculous meat, but as an indispensable means of acquiring better information than now exists. To be of use it must be carried out on the principles I have already set forth. It must be conducted by men of technical skill acting under scientific guidance.

"In conclusion," said Dr. Sanderson, "I would beg you to notice that I have limited myself to the question of the consumption of meat. Although I have purposely left the milk question out of consideration, I have referred to facts which bear upon it. We have seen it to be exceedingly probable that about 40 per cent. of the children that die in hospital, die tuberculous. I have already expressed my belief that in some of these cases the disease is congenital—that is, dependent on causes which have operated before birth. Some are probably infected by inhalation of the tubercle bacillus from the atmosphere, notwithstanding that pathology affords so little evidence of it; but for the rest, notwithstanding the lack of satisfactory evidence, I cannot resist the conviction that the consumption of unboiled milk during the years which follow weaning must have its share in bringing about the fatal prevalence of tuberculous disease at that period of life. This being the case, I feel that, whatever course may be taken as regards meat, I can join heartily with those who think that the sale of contaminated milk ought to be put a stop to by all possible means, and I trust that on this subject there will be no difference of opinion, and that this Congress will take such action as may promote the progress of legislation."

Dr. Bang, Lecturer in the Royal Veterinary College, Copenhagen, in a paper on "The Alleged Danger of consuming the apparently Healthy Meat and Milk of Tuberculous Animals," stated that the great majority of investigators are agreed that the essential source of tuberculosis in man is found in man himself; but almost all admit that he may contract the disease through the ingestion of milk derived from animals affected with tuberculosis.

It is always agreed that such a danger exists, but as to the extent of the danger there is little unanimity.

Of course, it might be said that there would be no danger if the use of meat and milk from the tuberculous animals were entirely interdicted; but it must not be ignored that the application of such a stringent measure would entail enormous loss from an economical point of view, especially in those countries where the disease has a very wide distribution amongst bovine animals. He looked upon the general application of the French regulations as out of the question, at least for the present, whilst such a course appeared on the whole to be unnecessary. As regards milk, the question of prophylaxis was comparatively easily settled if it was resolved that it should never be employed without first being boiled. But then the question comes to be, How can we protect ourselves against the products of milk?

The experiments made by Galtier, the author, Hum, and others have proved that the various products derived from milk, butter, cream cheese, cheese, and butter-milk may all contain tubercle bacilli, and that these retain their vitality in such products for a period of from fourteen to thirty days. It was true the majority of these bacilli may be separated from milk if the cream be removed by means of a centrifugal machine, as is generally done in Denmark, but if the milk is very rich in bacilli a few usually remain in the milk, and even in the cream. In order to do away with this danger it is necessary to expose the milk or the cream before churning to a temperature high enough to kill the tubercle bacilli (85° C. for about five minutes); a temperature of from 60° to 75° C., however, being quite sufficient to attenuate the organic virus, so far as to render it incapable of setting up infection of the alimentary canal. This method is coming more and more into use in Denmark, as by it several other sources of infection in the butter are also neutralized. As, however, many people object to the taste of boiled milk, it became an important question to determine whether the milk of phthisical cows is really a source of danger in the majority of cases. He had determined that when the udder is affected with tuberculosis there are usually numerous bacilli in the milk, which is consequently extremely dangerous. But he also finds that mammary tuberculosis is not so common as was at one time supposed. At the *abattoir* of Copenhagen, for example, it has been found that only in 1 per cent. of tuberculous cattle was there disease of the udder. From twenty-eight tuberculous cows, in which, however, there was no disease of the udder, the milk was injected into forty-eight rabbits, and in only two was there any positive result. He then inoculated forty guinea-pigs with milk from twenty-one tuberculous cows, in this case with four positive results. Recently he had carried on a new series of experiments with the milk from fourteen extremely phthisical cows. In this series the milk was virulent in three cases, so that from sixty-three tuberculous cows the milk contained virulent tubercle bacilli in nine cases only. All these cows were affected in a very high degree, and it is probable that in some at any rate the udder was affected; though this could not be demonstrated in the living animal, as it was in three out of the four cases of the second series. Others were affected with miliary tuberculosis in the different organs, a condition which one rarely finds in an animal that is still giving milk, and in one case the supramammary lymphatic glands were affected with tuberculosis, although no lesions in the udder itself could be demonstrated.

In several of the positive cases the number of bacilli in the milk must have been very small, as one only of the two guinea-pigs experimented upon succumbed to the disease, this happening in three instances.

It should be added that the quantity of milk injected in the later series was larger than in the earlier series. In the two first series 1 to 3 c.c. was injected, in the third 5 to 10 c.c. He maintained that, although in many cases the milk from phthisical cows is not virulent when the mammary gland is unaffected, it is in a certain proportion of cases, and should always be looked upon with suspicion, and that it is absolutely necessary to take prophylactic measures against the use of such milk, although the danger should no doubt not be exaggerated.

Meat.—Flesh itself very seldom contains any tubercle. Nevertheless it had been proved by a number of experiments that the muscle juice may contain tubercle bacilli, but such cases, according to the observations of Chauveau, of Arloing, Peuch, Galtier, Nocard, Kastner, and others, are absolutely in a minority. Amongst seventy-three phthisical cows these observers have found only ten in which the muscle juice gave evidence of virulence on injection into rabbits or guinea-pigs, and sometimes the juice inoculated only produced the disease in one of several animals inoculated.

M. Nocard's experiments in this connection are very interesting. He found that when a culture very rich in bacilli was injected into the vein of the ear of a rabbit, the muscle juice of the animal was virulent only when it was killed within five days after the inoculation, from which he argued that the bacilli carried by the vessels to the muscles only preserve their vitality for five days. If to this experimental result be added the observation that tubercle is very seldom developed in the muscles, even during the development of a condition of general tuberculosis, it must be concluded that muscular tissue is a soil so unfavourable for the growth of tubercle bacilli that they are not able to multiply. The number of bacilli, then, that can be found in the flesh of tuberculous animals is always extremely limited. It is of course true, as M. Arloing has objected to M. Nocard's conclusions, that the circulatory system of a tuberculous animal can continually receive into it fresh bacilli, and therefore until within only a few minutes before the animal is slaughtered. But, on the other hand, it must not be forgotten that it is only in the case of the development of an acute miliary tuberculosis that one can suppose that the number of bacilli introduced into the vessels can be considerable. In ordinary cases in which the tubercular process is developed slowly the bacilli would without doubt escape into the blood in very small quantities, and the number of bacilli that could be found at any given moment in the meat would be very small. Moreover, the experiments carried out by Galtier, Gebhardt, and others, render it very probable that the number of bacilli introduced into the alimentary canal, by which infection does not readily occur, plays a not unimportant part in the result obtained.

Prof. Bang stated that he had recently completed a series of experiments on the virulence of the blood of cows in an advanced stage of tuberculosis. From twenty tuberculous cows he inoculated thirty-eight rabbits and two guinea-pigs with defibrinated blood, injecting from 10 to 18 c.c. (in four cases only 5 to 9 c.c.). In eighteen cases the results were negative, in two positive, and one of these in which the lesion was small was one of two rabbits injected with blood from the same cow. The cow that supplied the blood with which the other positive result was obtained had developed acute miliary tuberculosis after an injection of tuberculin. Three weeks previously blood from the same cow had given negative results. Even amongst those cases in which the results were negative there were several cases of acute miliary tuberculosis.

He concluded from the foregoing that the seizure of all tuberculous animals is too stringent a measure. So long as the tuberculosis is strictly localized, the meat is not a source of danger; where the malady is generalized, the consumption of the meat may be dangerous, although it is not always so. The eating of uncooked meat should be discouraged, but the best means of avoiding danger to the health of man is to take all possible measures for preventing the propagation of tuberculosis amongst our domestic animals.

Prof. Arloing, of Lyons, contended that the question of transmissibility of tuberculosis from animals to man was one of very great importance, but he admitted that the *diagnose précoce* was very difficult. The danger to children of drinking milk from tuberculous cows was great, and he thought could scarcely be exaggerated. Moreover, he held very strongly that, except under certain special circumstances, the total condemnation of tuberculous meat was necessary, and on grounds of public health he dissented entirely from Dr. Bang's position.

The flesh of all tuberculous animals should be suspected as dangerous to health, the more so as meat was very often insufficiently cooked, the bacilli present under these conditions remaining pathogenic. From statistics he had gathered, he felt no doubt on this subject, and although it might be possible, by first carefully cooking under public supervision, to allow the flesh from animals in which the tuberculosis was localized to be sold, he still maintained his position that total confiscation of tuberculous meat was the safest method to be adopted. It was necessary, however, that in the first instance we should have a system of strict inspection, not only in our large towns, but also in all the smaller centres of population.

A paper was then given by Prof. M'Fadyean (Edinburgh) and Dr. Woodhead (London), on the transmission of tuberculosis from animals to man, by means of flesh and milk derived from tuberculous animals. They maintained that the evidence as to the transmission through the flesh or milk of tuberculous animals was very conflicting, apparently in great part because the methods used were different, and the conditions were not uniform. They had attempted to follow the line of infection of tuberculosis in a number of children, and had found that in 127 cases analyzed tubercle of the intestine was present in 43; 24 of these cases occurring between one and five and a half years; tubercle of the mesenteric glands was found in 100 cases, or in nearly 79 per cent. of the whole; here, again, 62 of these occurring between one and five and a half years; and of 14 cases in which the mesenteric glands were primarily affected—*i.e.* no trace of tubercle could be found in any other part of the body—9 were referred to the same period. It was noticeable that of these 100 cases only 20 were diagnosed during life as suffering from abdominal tubercle. From all that could be learned from these cases (and reference could be made to a large number of other sets of statistics practically proving the same point), it was evident that intestinal and mesenteric tubercle are most frequently met with in children during the period after they are weaned, at which time cow's milk has been substituted for mother's milk. The point of entrance appeared in these cases to be by the intestine. They had come to the conclusion that in some cases at least the tubercle bacilli had passed from the intestine into the mesenteric glands without leaving any trace of lesion to indicate their point of entrance. There could now be no doubt that tubercle bacilli were sometimes present in the milk from tuberculous cattle, especially where the udder was affected, and they had been able to obtain such bacilli embedded in the epithelium of the milk ducts, or lying free in the ducts after the death of the animal. They concluded that wherever the presence of a tuberculous condition of the udder could be demonstrated clinically it would be little less than criminal to give the milk to delicate children, or even to children suffering from any catarrhal derangement of the intestine, a condition that is specially frequent amongst the poor classes, where the standard of health is exceedingly low and the liability to catarrhal conditions very great. From a series of inoculations with tuberculous udder, and with milk from tuberculous udders, 14 out of 19, or over 70 per cent., had given positive results; with non-tubercular udders, and with milk from otherwise tuberculous cows, only 2 cases out of 13, or a little under 16 per cent., gave positive results. Where the failure to produce tuberculosis occurred in the first series, the number of bacilli was invariably small, and inoculations were usually into the subcutaneous tissue, though negative results were also obtained when other methods of infection were employed. They thought that in relation to the danger of taking tuberculous milk by the human subject, the site of the infection, and the relation of the number of bacilli introduced, played an important part in determining the severity and rapidity of the course of the disease, and they stated that their experience accorded with that of other observers, that inoculation into the peritoneal cavity is much more certain than inoculation into the subcutaneous tissue, especially where the number of bacilli introduced is comparatively small. They are also led to believe, from a number of feeding experiments, that the production of tuberculosis through the introduction of bacilli into the alimentary canal is of still less frequent occurrence than when inoculation is made into the connective tissue. As regards the possibility of the flesh of tuberculous animals setting up tuberculosis, (*a*) when introduced *en masse*, (*b*) when expressed juice only was exhibited, their experiments went to prove that the juice only did not in most cases contain a sufficient number of bacilli to set up tubercle, even when inoculated into small rodents, but from the fact that they have observed tubercular masses in the muscles of the buttock of tuberculous cattle, it must be accepted that tubercle bacilli may sometimes, though perhaps rarely, be present in considerable numbers in this position. Of three cows slaughtered in one day at one slaughter-house, well-defined tubercle was found in the muscles of the buttock of two animals; in one of these there was tuberculosis in every organ and part of the body; in the other there were only a few nodules and in some of the glands; there was certainly no pleural or peritoneal tubercle, and all the other organs were unaffected. They concluded that there was great necessity for a thorough inspection of both dairy cattle and of animals that were slaughtered for food purposes, but it might be accepted that the danger of contracting tubercle from milk was greater than that of contracting it from meat, and that only in a certain proportion of cattle affected with tuberculosis did there seem to be any danger to be anticipated from the ingestion of the flesh. In the main they agreed with Prof. Burdon Sanderson and Dr. Bang that there was not yet sufficient evidence on which to decide that the total seizure of meat from tuberculous animals should be resorted to.

Prof. Hamilton, of Aberdeen, said that there were two principal channels of infection, (1) the gastro-intestinal tract, (2) the lungs; but in addition to these we had what might be spoken of as localized tubercle, which seemed to be shut off entirely from all communication with the external world. (1) In the body the affection might take place by the air channels, as in the case of tubercular pneumonia, where the virus was probably inhaled and the air vesicles were the primary seat of infection. (2) By the blood vessels, as in cases of eruption of miliary tuberculosis. (3) By the lymphatic vessels, as in the more chronic forms of tuberculosis. In the gastro-intestinal canal a tubercular lesion might accompany an ordinary phthisis; it was often seen in children as a primary condition, and he should not be inclined to agree with Dr. Burdon Sanderson that it was not also primary in adults, as he himself had seen several cases, one quite recently. Previous catarrh was not always met with in the lung, but it was certainly a predisposing cause of tubercle, as it interfered with the protective epithelial covering. When tubercle followed whooping-cough, measles, and so on, it was probably the result of the spread of infection from pre-existing caseous spots, or it might be that the glands, weakened by the disease, fell an easy prey to the tubercle bacillus. He could not understand the comparative immunity from tubercle enjoyed by the pericardium and the stomach.

Prof. Nocard, of Paris, did not think that sufficient proof had as yet been accumulated that ingestion of tuberculous meat could give rise to tuberculosis in any large proportion of cases; the greater number of experimental cases had given negative results, and he should, to convince himself, require to see more positive results obtained in which all possible sources of failure could be eliminated. Whilst saying this, he must admit that in the case of children tuberculous material, whether in meat or milk, would always prove a very important source of danger. He would draw attention to the disease as it occurred in cats, on which animals he had made many experiments.

Dr. Hime, of Bradford, was glad to find that our foreign friends, who are not hampered as we are in making experiments, agree with us in the main. He thought that we were likely to run wild on the subject of the total seizure of tubercular meat, and he would point out that in no country does a total seizure law exist such as it is proposed to adopt here in England. In England he would point out that the inspection is worse than in any other country. He referred to Prof. Lingard's experiments given in an official report, which, he pointed out, spoke only of tubercle being transmitted by caseous material, and not by meat from a tuberculous cow, as was usually assumed. We had the authority of Koch himself, said Dr. Hime, that there is danger only when tubercular material itself is ingested. Infection by milk he looked upon as proved, but he would also insist very strongly that the majority of infection in cases of phthisis was directly between man and man, and it was far more important that we should eliminate possible sources of contagion between human subjects than that we should pay so much attention to the minor possibilities of infection from animals to man.

Dr. Barlow (London), speaking from a clinical point of view, was scarcely able to indorse the results of experimental researches, and he maintained that as regards tuberculosis in children we must for the present keep our minds open. There was no doubt that the *post-mortems* in children's hospitals gave evidence of the enormous frequency of tuberculosis, but the evidence that such disease was the result of the ingestion of milk and meat was comparatively slight. Other sanitary precautions, which he looked upon as of primary importance, must not be lost sight of in our discussion of the subject. He would, however, enter a protest against the use of the raw meat juice in the case of delicate children, as from what we had heard it was evident that such aliment might prove a source of considerable danger.

Prof. Perroncito, of Turin, referred to a number of experiments that he had carried out with meat, milk, and the products of the latter, and then pointed out that spontaneous tubercle very rarely occurred in the pig, though it might frequently be met with as the result of infection. The same might be said of sheep. Here, also, it might occur, though rarely, as the result of direct infection.

Prof. Burdon Sanderson, in reply, said he was pleased to find that the difference of opinion amongst so many authorities was so slight. It was evident that all were agreed that inspection was necessary, and there was also a general consensus of opinion as regards the difficulty of diagnosis. He was glad to find that although M. Arloing still retained his opinion as to the necessity for total seizure, except under very well-defined conditions, he had so far given way as to acknowledge that such meat might after careful cooking be retailed under special restrictions. In order that something definite might come out of this discussion, he proposed that it be minuted that "the etiology of tubercular disease of early infancy (between three months and five years)" be referred for discussion at the next Congress.

This was seconded by Dr. Septimus Gibbon, and was carried unanimously.

The President said that he had been greatly interested in the discussion, and he hoped that much good should arise therefrom. He was glad to find that there were some animals, such as the sheep and pig, in which spontaneous tubercle was never met with, and he hoped that we might eat these in safety. Sheep especially appeared to have a great immunity as regards tubercle, but pigs were not so safe, as they were apparently frequently the subject of tuberculosis.

Dr. Metschnikoff and Dr. Roux gave a joint paper on the changes that took place in the tissues around tubercle bacilli. It was read by the former, who illustrated his remarks by means of drawings on the black-board, and by microscopic specimens. They indicated the difference in the reaction of our tissues to the tubercle bacilli when the disease is going to run a favourable course, and when the animal is about to succumb rapidly to the disease. The process of recovery was indicated by the presence of concentric rings of hard and inflammatory tissue around the bacilli, which eventually lead to their absorption, the inflammatory tissue itself finally undergoing a process of calcification.

Prof. Ehrlich proceeded to give Koch's present views regarding tuberculin. He said that the results that had been obtained were exceedingly favourable, and most of those who had failed to obtain equally good results had failed because they had used too large doses of the remedy. The principle of cure rested in the local effects which tuberculin exercises on the specifically affected tissues; the inflammatory reaction passing to necrosis was neither desirable nor necessary, but, on the other hand, slight and even repeated stimuli would so act as to give rise to cicatrization of the tuberculous centres, so that the essence of this method of treatment was to retain as long as possible the specific excitation of the tissues, and not to do away with this, as was the case where large doses were used. Wherever successful results had been obtained they had all been by the use of repeated minute doses of tuberculin, which were only very gradually increased in strength, and it should be specially noted that the pathological signs found as the result of the action of tuberculin were always produced by large doses.

Prof. Cornil, Dr. Bardach, Dr. Ponfinck, and Prof. Hueppe were agreed that tuberculin was an heroic and dangerous remedy about which we as yet knew little, and which was therefore to be looked upon as still being experimented with. It also seemed to be the general opinion that where it was in use there existed a danger of setting up generalization of a tuberculosis that had hitherto been localized.

Dr. Hunter gave the results of his own experiments (described in the *British Medical Journal*), from which he had been able to show the nature of the active principle of tuberculin. He had succeeded in isolating principles quite different from those mentioned by Koch, or even reported by Dr. Ehrlich that morning as having been obtained by Koch. They were three—(1) those which produced fever, but set up no local reaction; (2) those which gave a local reaction, but no fever; and (3) those which set up neither fever nor local reaction, which had a distinctly remedial effect.

The President, summing up, hoped that in time we should all be able to obtain the wonderfully satisfactory results that had been so fully described by Prof. Ehrlich on Dr. Koch's behalf.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Rain-gauges.

I DO not think that valuable space in your columns should be occupied by rediscussing old questions. I do not wish to say a word in any respect discourteous to Mr. Fletcher, whose ability in other subjects has, I understand, been already recognized, but it really would have been better had he read up the subject before writing the remarkable letter which appears in NATURE of the 20th inst. (p. 371).

For experimental work, spherical, conical, inclined, horizontal, vertical, and tipping funnels have been used; but until the soil of the British Isles can be made to tilt in altitude and rotate in azimuth, so as to meet the path of falling rain, I think that we must adhere to gauges with horizontal mouths as the best representatives of the surface of the earth.

G. J. SYMONS.

British Association Reception Room, Cardiff, August 21.

Cloud Heights—Kinematic Method.

IN NATURE of April 16 (p. 563), and possibly elsewhere, I am made to speak of the method of determining the heights of clouds at sea used by Finemann and myself as the "aberration method." This was a misnomer that I supposed had been corrected. The more proper term is the "kinematic method," since in it we discuss the apparent motions of the clouds considered as the resultant of the true motions of the cloud and the observer. This is the term that I have used since May 1890, and would commend to others. CLEVELAND ABBE.

Weather Bureau, Department of Agriculture,
Washington, August 8.

THE BRITISH ASSOCIATION.

THE Cardiff meeting, if it was not made remarkable by any incident of very special importance, was, upon the whole, successful. Several of the addresses delivered by the Presidents of Sections were of exceptional interest, but some were very long, and we shall not be able to print all of them.

At the first meeting of the General Committee, held on Wednesday, August 19, the report of the Council for 1890–91 was read by Sir Douglas Galton. Dr. Gladstone moved a vote of thanks to Prof. Williamson for his long and valuable services as general treasurer, paying a tribute to the manner in which that gentleman had fulfilled his duties. Sir Douglas Galton seconded, and the resolution was cordially agreed to. Mr. Vernon Harcourt moved, and Sir J. Douglass seconded, the appointment of Prof. Arthur Rücker as general treasurer. This motion was also agreed to. At the meeting of the General Committee on Monday, a deputation from Nottingham was introduced. The Association was invited by the Mayor and town authorities to visit Nottingham in 1893. It was stated that it was twenty-five years since the Association had visited Nottingham. The invitation was accepted on the motion of Mr. Preece. It was also unanimously agreed, on the motion of Canon Tristram, to elect Sir A. Geikie as President of the Association, which meets at Edinburgh next year. The Lord Provost of Edinburgh, the Marquis of Lothian, the Earl of Rosebery, Lord Kingsburgh, Principal Sir William Muir, Prof. Sir Douglas Maclagan, Sir William Turner, Prof. Tait, and Prof. Crum Brown were elected Vice-Presidents for the Edinburgh meeting. Prof. G. F. Armstrong, Principal F. Grant Ogilvie, and Mr. John Harrison were elected Local Secretaries for the meeting at Edinburgh, and Mr. A. Gillies Smith Local Treasurer. A deputation from Edinburgh also attended with reference to the fixing of a date for the Edinburgh meeting. It was stated on behalf of the Town Council that September 28 was favoured as the opening date of the meeting, though August 3 and September 21 were also mentioned as alternative dates. A motion was made to fix August 3, while an amendment was moved for September 12; but as only thirteen voted for the amendment, the original motion was agreed to—that is, the Association will meet at Edinburgh next year on August 3. The general officers were re-elected, and the following gentlemen were elected Members of Council for the ensuing year:—Dr. W. Anderson, Prof. Ayrton, Sir B. Baker, Mr. H. W. Bates, Prof. Darwin, Sir J. N. Douglass, Prof. Edgeworth, Dr. J. Evans, Prof. Fitzgerald, Sir Archibald Geikie, Mr. R. T. Glazebrook, Profs. J. W. Judd, Liveing, Lodge, Mr. W. H. Preece, Profs. W. Ramsay, Reinold, Roberts-Austen, Schäfer, Schuster, Sidgwick, Mr. G. J. Symons, Profs. T. E. Thorpe, Marshall Ward, Mr. W. Whitaker, Dr. H. Woodward. The following impressions have been recorded by a correspondent:—

CARDIFF, *Tuesday Evening*.

One of the most prominent features of the Cardiff meeting has undoubtedly been the prevailing bad weather. Rain and cold have had their usual depressing results, and may to some extent account for the disappointment which exists among many of those in attendance. The Local Committee have done their best to render the meeting a social success, but the entertainments by the Municipality and the citizens of Cardiff have been of a somewhat restricted character. Notwithstanding the unpromising state of the weather, the excursions on Saturday and Sunday were largely taken advantage of, and the reception given by Lord Windsor on the latter day was specially appreciated. The total attendance has been about 1500, within 200 of the Leeds meeting, while the amount of money available for grants is within a few pounds of last year. Naturally there has been considerable talk with reference to the address of the President of Section A, and opinion is divided as to the propriety of introducing the metaphysical into a Section which has emphatically to do with the "solid ground of Nature." On the other hand, Prof. Lodge's experiment to test whether the ether is disturbed in the presence of a rapidly-moving body has excited the greatest interest and admiration.

The *soirées* at the present meeting can hardly be compared in attractiveness and brilliancy with those held last year in Leeds. Wealthy and populous as Cardiff is, she has not command, apparently, of the scientific and artistic collections which are so creditable to the intelligence and taste of the dingy Yorkshire city. However, the dance into which to-night's *conversazione* developed evidently atoned for a multitude of shortcomings. The lectures have been fairly well attended, Prof. Rücker's beautiful experiments evidently fascinating his audience, in spite of a serious hitch caused by the failure of a steam-engine to do its duty when called upon. The discussion, in Section D, as to the relations between animal and plant life was well sustained, and it is a pity that arrangements had not been made to have it fully reported. This can be done at very small cost, and the publication of detailed reports of such discussions could not but greatly increase the good they are calculated to do. There is a general belief that inter-Sectional discussions would be of immense advantage in showing the intimate relations which exist between the different branches of science, and in stimulating research in profitable directions. It is probable that several such discussions may be arranged for the next meeting.

As usual, Section E had its sensation. A very large audience attended to hear Mrs. French Sheldon describe her journey to Lake Chala, at the base of Kilimanjaro. Mrs. Sheldon was evidently suffering greatly from her serious accident; and although her address was somewhat disjointed, it contained a good deal of fresh information, especially on the natives, which male travellers have hitherto overlooked. Mrs. Bishop (Miss Isabella L. Bird) proved equally attractive in describing her visit to the Bakhtiari country and the Karun River, and, as might have been expected, was somewhat more solid than her less-experienced fellow traveller.

The Ordnance Survey formed the subject of an important discussion in Section E, and the Association as a body has resolved to do its utmost to induce Government to introduce reforms. It is fortunate that by the combined action of Sections A, E, and G, a grant of £75 has been obtained for supplying instruments for climatological observations in Central Africa.

There was considerable discussion at the general committee meeting yesterday as to the date of the Edinburgh meeting next year. In certain quarters the end of September was advocated, but there can be no doubt that the great majority of the working members of the Association preferred the beginning of August, a date which will suit those connected with the Universities and will catch the citizens of Edinburgh before they leave for their holidays. It is, therefore, not surprising that August 3 has been fixed upon for the Edinburgh meeting, the President of which will be Sir Archibald Geikie. Nottingham has been selected as the place of meeting for 1893.

It is evident that the people of Cardiff are somewhat at a loss what to make of the Association and of the hundreds who are crowding the streets of the town and rushing from one Section room to another. The Sectional secretaries especially, seem to be a puzzle. In the hotel in which they are housed a commercial stock-room has been set apart for their use, with a long baize-covered table down the centre; while to discourage all tendencies to loafing, they have been provided with nothing else but hard kitchen chairs to sit upon.

Altogether, from a scientific point of view, the Cardiff meeting may be said to have come up to a fair average.

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. W. C. ROBERTS-AUSTEN, C.B., F.R.S., PRESIDENT OF THE SECTION.

THE selection of Cardiff as a place of meeting of the British Association led to the presidency of Section B being intrusted to a metallurgist. It will be well, therefore, to deal in this address mainly with considerations connected with the subject to which my life has been devoted, and I hope that it may be possible for me to show that this practical art has both promoted the advancement of science and has received splendid gifts in return.

It is an art for which in this country we have traditional love; nevertheless the modes of teaching it, and its influence on science, are but imperfectly understood and appreciated. Practical metallurgists are far too apt to think that improvements in their processes are mainly the result of their own experience and observation, unaided by pure science. On the other hand, those who teach metallurgy often forget that for the present they have not only to give instruction in the method of conducting technical operations, but have truly to educate, by teaching the chemistry of high temperatures, at which ordinary reactions are modified or even reversed, while they have further to deal with many phenomena of much importance, which cannot, as yet, be traced to the action of elements in fixed atomic proportions, or in which the direct influence of the atom is only beginning to be recognized.

The development of a particular art, like that of an organism, proceeds from its internal activity; it is work which promotes its growth and not the external influence of the environment. In the early stage of the development of an industry the craftsmen gather a store of facts which afford a basis for the labours of the investigator, who penetrates the circle of the "mystery" and renders knowledge scientific. Browning, inspired by the labours of a chemist, finely tells us in his "Paracelsus":—

> To know
> Rather consists in opening out a way
> Whence the imprisoned splendour may escape,
> Than in effecting entry for a light
> Supposed to be without.

If it be asked who did most in gaining the industrial treasure and in revealing the light of chemical knowledge, the answer is certainly the metallurgists, whose labours in this respect differ materially from others which have ministered to the welfare of mankind. First it may be urged that in no other art have the relations between theory and practice been so close and enduring. Bacon, who never undervalued research, tells us that in the division of the labour of investigation in the New Atlantis there are some "that raise the former discoveries by experiment into greater observations, axioms, and aphorisms: these we call the *interpreters of nature.*" There are also others "that bend themselves, looking into the experiments of their fellows and casting about how to draw out of them things of use and practice for man's life and knowledge: . . . these we call the *dowry men or benefactors.*" In reviewing the history of metallurgy, especially in our islands, it would seem that the two classes of workers, the interpreters of nature and the practical men, have for centuries sat in joint committee, and, by bringing theoretical speculation into close connection with hard industrial facts, have "carried us nearer the essence of truth."

The main theme of this address will therefore be the relation between theory and practice in metallurgy with special reference to the indebtedness of the practical man to the scientific investigator.

We will then consider—

(1) Certain facts connected with "oxidation" and "reduction," upon which depend operations of special importance to the metallurgist.

(2) The influence in metallurgical practice of reactions which are either limited or reversible.

(3) The means by which progress in the metallurgic art may be effected, and the special need for studying the molecular constitution of metals and alloys.

(1) The present year is a memorable one for chemists, being the centenary of the birth of Faraday and the bi-centenary of the death of Robert Boyle. The work of the former has recently been lovingly and fittingly dealt with in the Royal Institution, where he laboured so long. I would, in turn, briefly

recall the services of Boyle, not, however, on account of the coincidence of date, but because with him a new era in chemistry began. He knew too much about the marvellous action of "traces" of elements on masses of metal to feel justified in pronouncing absolutely against the possibilities of transmutation, but he did splendid service by sweeping away the firm belief that metals consist of sulphur, salt, and mercury, and by giving us the definition of an element. He recognized the preponderating influence of metallurgy in the early history of science, and quaintly tells us that "those addicted to chemistry have scarce any views but to the preparation of medicines or to the improvement of metals," a statement which was perfectly correct, for chemistry was built up on a therapeutic as well as a metallurgic basis. The fact is, however, that neither the preparation of materials to be employed in healing, nor the study of their action, had anything like the influence on the growth of theoretical chemistry which was exerted by a few simple metallurgical processes. Again, strange as it may seem, theoretical chemistry was more directly advanced by observations made in connection with methods of purifying the precious metals, and by the recognition of the quantitative significance of the results, than by the acquisition of facts incidentally gathered in the search for a transmuting agent. The belief that chemistry "grew out of alchemy" nevertheless prevails, and has found expression in this Section of the British Association. As a fact, however, the great metallurgists treated the search for a transmuting agent with contempt, and taught the necessity of investigation for its own sake. George Agricola, the most distinguished of the sixteenth century metallurgists, in his work "De Ortu et Causis Subterraneorum" (lib. v.), written about the year 1539, disdainfully rejects both the view of the alchemists that metals consist of sulphur and mercury, and their pretended ability to change silver into gold by the addition of foreign matter.

Biringuccio (1540) says, "I am one of those who ignore the art of the alchemists entirely. They mock nature when they say that with their medicines they correct its defects, and render imperfect metals perfect." "The art," he adds, "was not worthy of the consideration of the wise ancients who strove to obtain possible things." In his time, reaction between elements meant their destruction and reconstitution; nevertheless, his sentence "transmutation is impossible, because in order to transmute a body you must begin by destroying it altogether," suggests that he realized the great principle of the conservation of mass upon which the science of chemistry is based. We have also the testimony of the German metallurgist, Becher, who improved our tin-smelting in Cornwall. He is said to have caused a medal to be struck in 1675, which bore the legend, "Hanc unciam argenti finissimi ex plumbo arte alchymica transmutavi," though he should have been aware that he had only extracted the precious metal from the lead, and had not transmuted the base one. This is a lapse which must be forgiven him, for his *terra pinguis* was the basis of the theory of phlogiston, which exerted so profound an influence for a century after his death, and he wrote, "I wist that I have got hold of my pitcher by the right handle, for the pseudo-chemists seek gold, but I have the true philosophy, science, which is more precious."

At this critical period what was Boyle doing when the theory of phlogiston dawned in the mind of the metallurgist Becher? In 1672 Boyle wrote his paper on "Fire and Flame weighed in the Balance," and came to the conclusion that the "ponderous parts of flame" could pass through glass to get at melted lead contained in a closed vessel. It has been considered strange that he did not interpret the experiment correctly, but he, like the phlogistic chemists, tried to show that the *subtilis ignis*, the material of fire or phlogiston, would penetrate all things, and could be gained or lost by them. Moreover, his later experiments showed him that glass was powerless to screen iron from the "effluvium of a loadstone." His experiment with lead heated in a closed glass vessel was a fundamental one, to which his mind would naturally revert if he could come back now and review the present state of our knowledge in the light of the investigations which have been made in the two centuries that have passed since his own work ceased. If he turned to the end of the first century after his death he would see that the failure to appreciate the work of predecessors was as prevalent in the eighteenth century as in the sixteenth. The spirit of intolerance which lead Paracelsus to publicly burn, in his inaugural lecture at Basle, the works of Galen, Hippocrates, and Avicenna, survived in the eighteenth century, when Madame Lavoisier burnt the works of Stahl, but it was reserved for the nineteenth century to reverently gather the ashes, recognizing that when the writers of the school of Becher spoke of phlogiston they meant what we understand by potential energy.

If Boyle, finding that the Fellows of the Royal Society had not carried out their intention to build a "Repository and Laboratory," sought the School of Mines and came to the Royal College of Science, he would surely thank my colleague, Prof. Thorpe, for his vigorous defence last year, as President of this Section, of the originality of the work of Priestley and Cavendish, to which Boyle's own researches had directly led. We on our part, remembering Berzelius's view that "oxygen is the centre point round which chemistry revolves," would hope to interest him most by selecting the experiments which arose out of the old metallurgical operation of separating the precious metals from lead by "cupellation." When, in conducting this operation, lead is heated in the presence of air, it becomes converted into a very fluid dross. Boyle had, in 1661, taken this operation as the very first illustration in his "Sceptical Chemist" in proof of his argument as to the elemental nature of metals. He would remember the quantitative work of Geber in the eighth century, who stated that the lead so heated in air acquired a "new weight," and he would appreciate the constant reference to the operation of cupellation from the close of the sixth century B.C., when the prophet Jeremiah wrote, to the work of Jean Rey in 1629, whose conclusions he would wish he had examined more closely. Lord Brouncker, as first President of the Royal Society, had called attention to the increase in weight of the lead in the "coppels" in the Assay Office in the Mint in the Tower, and Mayo had shown that the increase in weight comes from a distinct "*spiritus*" in the air. Boyle would incidentally see that Newton had accepted office in the Mint, where he doubtless continued his experiments on calcination, begun some time before, and, as if to mark his interest in the operation of assaying, figures are represented on a bas-relief on his tomb in Westminster Abbey as conducting cupellation in a muffle. The old work merges wonderfully into the new. Chevreul, in the nineteenth century, confirms Otto Tachens's view in the seventeenth, as to the saponifying action of litharge. Deville employs molten litharge to absorb oxygen dissociated from its compounds, and Graham, by extracting occluded gases from iron and other metals, proves the accuracy of the old belief that elastic fluids can freely permeate even solid metals.

We may imagine with what vivid interest Boyle would turn, not merely to the results of Priestley's work, but to his methods. Priestley had decomposed litharge with the electric spark, and had satisfied himself in 1774 by heating red lead that the gas he obtained in his earlier experiments was really the one now called oxygen.

Boyle would see, that in the period 1774–77 Lavoisier, being attracted by the "sceptical chemist's" own experiment on the heating of lead in closed vessels, overthrew the phlogistic theory, and placed chemistry on a firm basis by showing that the increase in weight of lead and tin, when heated in air, represents exactly the weight of the gaseous body added; and, finally, Dalton having developed the atomic theory and applied it to chemistry, Berzelius made lead memorable by selecting it for the first determination of an atomic weight.

Without diverting his attention from the phenomena of oxidation, Boyle would find questions the interest of which is only equalled by their present obscurity. He would contemplate the most interesting phase of the history of chemical science, described by Van 't Hoff as that of its evolution from the descriptive to the rational period, in the early days of which the impossibility of separating physics and chemistry became evident, and Boyle would find that chemistry is now regarded from the point of view of the mechanics of the atoms.

Deville's experiments on dissociation have rendered it possible to extend to the groups of atoms in chemical systems the laws which govern the fusion and vaporization of masses of matter, and this has produced a revolution comparable in its importance to that which followed the discovery of the law of definite proportions, for dissociation has shown us that true causes of chemical change are variations of pressure and of temperature. For instance, oxygen may be prepared on an industrial scale from air by the intervention of oxide of barium heated to a constant temperature of 700°, provided air be admitted to the heated oxide of barium, under a pressure of $1\frac{1}{2}$ atmospheres, while the oxygen, thus absorbed, is evolved if the containing vessel be rendered partially vacuous. It will be evident, therefore, that at a certain critical temperature and pressure the slightest variation of either will destroy the equilibrium of the system and induce chemical change.

The aim of Boyle's chemical writings was to show that no barrier exists between physics and chemistry, and to "serve the commonwealth of learning by begetting a good understanding betwixt the chemists and the mechanical philosophers," who had, as he said, "been too great strangers to each other's discoveries." In view of the dominant lines of research which occupy chemists at the present time, such, for instance, as the investigations of "osmotic pressure" and of the application of Boyle's own law to salts in solution, he would feel that his hope had been realized, and that, though he lived a century too soon to take part in Berthollet's discussion with Proust, he nevertheless shares Berthollet's triumph in the long-delayed but now rapid development of chemistry as a branch of applied mechanics.

We need, however, no longer look at these questions from the point of view of Boyle, for our own interest in the application of chemical mechanics to metallurgy is sufficiently vivid, as instances to be given subsequently will show.

Hitherto I have mainly dwelt on questions relating to oxidation, but not less interesting is the history of the steps by which an accurate knowledge was acquired of the other great process practised by the metallurgist, the one to which Paracelsus was the first to apply the name of "reduction." Its explanation followed naturally from the elucidation of the phenomena of combustion by Lavoisier, who in continuation of Macquer's experiments of 1771 proved, in conjunction with other workers, that carbonic anhydride is produced when the diamond is burnt in air or oxygen. Carbon has been known for ages as the most important of the reducing agents, but when, in 1772, Lavoisier heated oxide of lead and carbon together, he did not at first recognize that carbonic anhydride had been produced, simply because the volume of the gas set free was the same as if oxygen merely had been liberated. He soon, however, saw that neither the carbon alone, nor the oxide of lead alone, gave rise to the evolution of carbonic anhydride, which resulted from the *mutual action* of carbon and a constituent of the litharge. "This last observation leads us insensibly," he adds, "to very important reflections on the use of carbon in the reduction of metals." It most certainly did, and by 1815 an accurate, if incomplete, view of reduction had passed into the encyclopædias. It was seen that the removal of oxygen from burnt metals, by carbon, "gives the metals," as Fourcroy and Vauquelin put it, "a new existence." Some ten years later Le Play attempted to show that reduction is always effected by the intervention of carbonic oxide, which elicited the classical rejoinder from Gay-Lussac, who pointed out that "carbon alone, and at very moderate temperatures, will reduce certain metallic oxides without the intervention of carbonic oxide or of any other elastic fluid." I mention these facts because metallurgists are slow to recognize their indebtedness to investigators, and too often ignore the extreme pains with which an accurate knowledge has been acquired of the principles upon which their processes have been based.

The importance of a coherent explanation of reduction in smelting pig-iron is enormous. The largest blast-furnaces in 1815 hardly exceeded those in use in the previous century, and were at most only 40 feet high, with a capacity of 5000 cubic feet. At the present day their gigantic successors are sometimes 90 feet high, with a capacity of 25,000 cubic feet. This development of the blast-furnace is due to the researches of a number of investigators, among whom von Tunner, Lowthian Bell, and Grüner deserve special mention. We are, however, forcibly reminded of the present incompleteness of our knowledge of the mechanism of reduction, when we remember that the experiments of H. P. Baker have led us to believe that pure carbon cannot be burnt in perfectly dry and pure oxygen, and therefore that the reducing agent, carbonic oxide, cannot be produced at all unless moisture be present.

Ludwig Mond, Langer, and Quincke, teach us not only that nickel can separate carbon from carbonic oxide, but the wholly unexpected fact that dry carbonic oxide can at a temperature of 100° take up nickel, which it again deposits if heated to 150°. Mond and Quincke, and, independently, Berthelot, have since proved the existence of the corresponding compound of iron and carbonic oxide, and it may safely be concluded that in the blast-furnace smelting iron this peculiar action of carbonic oxide plays an important part, and it doubtless aids the carburization of iron by cementation. It is truly remarkable that the past year should have brought us so great an increase in our knowledge of what takes place in the reduction of an oxide of iron, and in the carburization of the liberated metal. My own experiments have, I trust, made it clear that iron can, at an elevated temperature, be carburized by the diamond *in vacuo*; that is, in the absence of anything more than "a trace" of an elastic fluid or of any third element. Osmond has further shown within the last few months that the action between iron and carbon is a mutual one, for though carbon in the pure diamond form carburizes iron, the metal in its turn, at a temperature of 1050°, attacks the diamond, invests it with a black layer, and truly unites with it.

The question of the direct carburization of iron (Darby's process) by filtering the molten metal through carbon, promises to be of much importance, for at present, as is well known, two millions of tons of steel which are made in the Bessemer converter in this country alone, are re-carburized after "the blow" by the addition of spiegeleisen.

Carbonic oxide, moreover, would appear to be more chemically active than had been supposed; for during the present year Berthelot has shown that the perfectly pure gas heated to 500° or 550° produces carbonic anhydride with the deposition of carbon at red heat, not by ordinary dissociation, but by decomposition preceded by polymerization. He further shows that carbonic oxide will decompose ammoniacal nitrate of silver, and thus brings it into close connection with the aldehydes.

(2) In turning to the modern aspects of metallurgical practice, we shall see that the whole range of the metallurgist's field of study is changing. It is no longer possible for him to devise a series of operations on the evidence afforded by a set of equations which indicate the completion of an operation; he has, as I have already suggested, to consider the complicated problems which have been introduced into chemistry from the sciences of physics and mechanics. He has, in fact, no longer to deal merely with atoms and molecules, but with the influence of mass. As Ostwald points out, we are reminded that many chemical processes are reciprocating so that the original products may be obtained from the product of the reaction. The result of such opposed processes is a state of CHEMICAL EQUILIBRIUM, in which both the original and the newly-formed substances are present in definite quantities that remain the same so long as the conditions, more especially temperature and pressure, do not undergo further change. Again, in very many metallurgical processes, reactions are rendered incomplete by the limitations imposed by the presence of bodies which cannot be speedily eliminated from the system, and the result may be to greatly retard the completion of an operation. The time has come when the principles of dynamic chemistry must be applied to the study of metallurgical problems if they are to be correctly understood, and it is, moreover, necessary to remember the part played by the surface separating the different aggregates in contact with one another. When, for instance, a reaction has to take place accompanied by the evolution of gas, there must be space into which the gas can pass. The rate, therefore, at which change takes place will obviously depend on the state of division of the mass.

One of the most remarkable points in the whole range of chemistry is the action engendered between two elements capable of reacting by the presence of a third body. It may be, and this is the most wonderful fact of all, that merely a trace of a third body is necessary to induce reaction, or to profoundly modify the structure of a metal. H. Le Chatelier and Mouret have pointed out that in certain cases it is inaccurate to say that the third body causes the reaction to take place, because, after it has destroyed the inter-molecular resistances which prevented the reaction taking place, the third body ceases to intervene. This is apparently the case when platinum sponge effects the union of oxygen and hydrogen, or, conversely, when very hot platinum splits up water vapour into its constituent gases. Future investigation will, it is to be hoped, show whether the platinum does not exert some direct action in both cases. We can no longer neglect the study of such questions from the point of view of their practical application. The manufacture of red lead presents a case in point. In "drossing" molten lead, the oxidation of the lead is greatly promoted by the presence of a trace of antimony; and conversely, in the separation of silver from molten lead, by the aid of zinc, H. Roessler and Endelmann have recently shown that aluminium has a remarkable effect in protecting the zinc from loss by oxidation, and further, the presence of one-thousandth part of aluminium in the zinc is sufficient to exert this protecting action on that metal. I am satisfied that if our metallurgists are to advance their industrial practice, they must, if I may use such an expression, persistently think in calories, and not merely employ the ordinary atomic "tools of thought." They will then be able to state what reactions can, under given conditions, take place; to indicate those which will be completed; and to avoid those that are impracticable.

In France, the country of so many great metallurgists, men like Le Chatelier and Ditte are doing admirable service, by bringing the results of the labours and teaching of St. Claire Deville within the range of practical men. And if I do not refer more specifically to their work it is for want of space and not of appreciation, but a few simple cases of reversible actions will perhaps make the subject clear. In the blast-furnace the main reducing agent, carbonic oxide, is produced from the solid fuel by the reaction $CO_2 + C = 2CO$, a reaction which is theoretically impossible because it is endothermic, and would be attended by absorption of heat. But heat external to the system intervenes, and acts either by depolymerizing the carbon into a simpler form which can combine with oxygen of the CO_2 with evolution of heat, or by dissociating carbonic anhydride sets oxygen free which combines with the carbon. Reduction of oxide of iron in the blast-furnace is mainly effected by carbonic oxide according to the well-known reaction

$$Fe_2O_3 + 3CO = 2Fe + 3CO_2.$$

But the gas issuing from a blast-furnace contains carbonic oxide, an important source of heat. The view that this loss of carbonic oxide was due to the fact that the contact of the ore and the reducing gas was not sufficiently prolonged, led to a great increase in the height of blast-furnaces, but without, as Grüner showed, diminishing the proportion of carbonic oxide escaping from the throat. The reduction of an iron ore by carbonic oxide only takes place within certain well-defined limits, and a knowledge of the laws of chemical equilibrium would have saved thousands and thousands of pounds which have been wasted in building unduly high furnaces. I would add that large sums have also been sacrificed in the vain attempt to smelt oxide of zinc in the blast-furnace, for which operation patents have frequently been sought, in ignorance or defiance of the readiness with which the inverse action occurs, so that the reducing action of carbon on oxide of zinc may be balanced by the re-oxidation of the reduced zinc by carbonic anhydride, which is the product of the reduction. A further instance may be borrowed from an electro-chemical process which has been adopted for obtaining alloys of aluminium. As is well known, all attempts to effect the direct reduction of alumina by carbon have failed, because the reaction

$$2(Al_2O_3)+3C=4Al+3CO_2$$

requires 783·2 calories, while only 291 calories would result from the conversion of carbon into carbonic anhydride, therefore the reaction cannot be effected; but in Cowles's process aluminium is nevertheless liberated when alumina is mixed with charcoal and strongly heated by the passage of an electric current. This result is due, not to a simple reduction of alumina, but to its dissociation at the high temperature produced by the passage of a current of 1600 amperes between carbon poles, the liberated aluminium being at once removed from the system by metallic copper, which is simultaneously present and may not be without action itself.

An instance of the importance of these considerations is presented in the manufacture of steel by the basic process. Much care is devoted to obtaining conditions which will insure, not only the elimination, but the order of the disappearance of the impurities from the molten pig-iron. In the basic process as conducted in the closed converter, the phosphorus does not disappear until the carbon has left the fluid bath, whilst, when the open-hearth furnace is used, the elimination of the phosphorus may be effected before that of the carbon, and it is asserted that, if the carbon goes before the phosphorus is got rid of, a further addition of carbon is necessary. A curious and subtle case of chemical equilibrium is here presented. In the open-hearth furnace and Bessemer converter respectively, the temperatures and pressures are different, and the conditions as to the presentation of oxygen to the fluid bath are not the same. The result is that the relative rates of oxidation of the phosphorus and carbon are different in the two cases, although in either case, with a given method of working, there must be a ratio between the phosphorus and carbon in which they disappear simultaneously. The industrial bearing of the question is very remarkable. In the basic Bessemer process the tendency of the phosphorus to linger in the bath renders an "after-blow" necessary; it may be only of a few seconds' duration, but much iron is nevertheless burnt and wasted, and Mr. Gilchrist tells me that, if this after-blow could be avoided, a saving of some 6 per cent. of the yield of steel would be effected annually, the value of which, at the present rate of output and price of steel, is no less than a quarter of a million sterling.

The volatilization of sulphur in the converter while it is retained by the steel in the open-hearth furnace, and the increase in the percentage of manganese which leaves the slag and returns to the bath of metal in the converter at the end of the "blow," will probably be traced to the disturbance of equilibrium which attends very slight variations in the conditions, especially as regards temperature and pressure, under which the operations are conducted.

In the blast-furnace the reducing action must be greatly dependent on the rate at which alkaline cyanides are formed, and Hempel has recently shown, by the aid of well-devised experiments, that the quantity of cyanides which may be obtained at a high temperature from carbon, nitrogen, and alkaline oxides, increases as the pressure becomes greater.

Metallurgical chemistry is, in fact, a special branch of chemical science which does not come within the ordinary sphere of the academic teaching of chemistry. It is often urged that metallurgical practice depends upon the application of chemical principles which are well taught in every large centre of instruction in this country, but a long series of chemical reactions exist which are of vital importance to the metallurgist, though they are not set forth in any British manual of chemistry, nor are dealt with in courses of purely chemical lectures. I feel bound to insist upon this point, because, as Examiner in Metallurgy for the Science and Art Department, I find that purely analytical and laboratory methods are so often given in the belief that they are applicable to processes conducted on a large scale, and at high temperatures.

We are told that technical instruction should be kept apart from scientific education, which consists in preparing the student to apply the results of past experience in dealing with entirely new sets of conditions, but it can be shown that there is a whole side of metallurgical teaching which is truly educational, and leads students to acquire the habit of scientific thought as surely as the investigation of any other branch of knowledge.

It is, in fact, hardly possible in a course of theoretical chemistry to devote much attention to specific cases of industrial practice in which reactions are incomplete, because they are limited by the presence of bodies that cannot be directly eliminated from the chemical system. Take, for instance, the long series of reactions studied by Plattner, who published the results of his investigations in his celebrated treatise, "Die Metallurgische Röstprozesse," Freiberg, 1856, whose work I have chosen as a starting-point on account of our presence in South Wales near the great copper-smelting district of Swansea. A complex sulphide, of which copper is the main metallic constituent, contains some fifty ounces of silver to the ton. The problem may be supposed for the present to be limited to the extraction of the precious metal from the mass in which it is hidden, and the student deriving his knowledge from an excellent modern chemical treatise would find the case thus stated:—

"Ziervogel's process depends upon the fact that when argentiferous copper pyrites is roasted, the copper and iron sulphides are converted into insoluble oxides, whilst the silver is converted into a soluble sulphate, which is dissolved out by lixiviating the roasted ore with hot water, the silver being readily precipitated from this solution in the metallic state."

It is certain that if an observant, chemically-trained student visited a silver extraction works, and possessed sufficient analytical skill to enable him to secure evidence as to the changes that occur, he would find a set of facts which his training had not enabled him to predict, and he would establish the existence of a set of reactions to the nature of which his chemical reading had hardly given him a clue. The process to be considered is a simple one, but it is typical, and applies to a large proportion of the 7,000,000 ounces of silver annually obtained in the world from cupriferous compounds. He would be confronted with a ton or more of finely-divided material spread in a thin layer over the bed of a reverberatory furnace. Suppose the material is what is known as a complex regulus, as imported into Swansea or produced at Freiberg, to which are added rich native sulphides. The mixture then consists of sulphides mainly of iron and copper, with some sulphide of lead, and contains fifty or sixty ounces of silver to the ton, and a few grains of gold. It may also contain small quantities of arsenic and antimony as arsenides, antimonides, and sulpho-salts, usually with copper as a base.

The temperature of the furnace in which the operation is to be performed is gradually raised, the atmosphere being an oxidizing one. The first effect of the elevation of the temperature is to distil off sulphur, reducing the sulphides to a lower stage of sulphurization. This sulphur burns in the furnace atmosphere to sulphurous anhydride (SO_2), and, coming in contact with the material undergoing oxidation, is converted into sulphuric anhydride (SO_3). It should be noted that the material of the brickwork does not intervene in the reactions, except by its presence as a hot porous mass, but its influence is, nevertheless, considerable. The roasting of these sulphides presents a good case for the study of chemical equilibrium. As soon as the sulphurous anhydride reaches a certain tension, the oxidation of the sulphide is arrested, even though an excess of oxygen be present, and the oxidation is not resumed until the action of the draught changes the conditions of the atmosphere of the furnace, when the lower sulphides remaining are slowly oxidized, the copper sulphide being converted into copper sulphate mainly by the intervention of the sulphuric anhydride formed as indicated. Probably by far the greater part of the iron sulphide only becomes sulphate for a very brief period, being decomposed into the oxides of iron, mainly ferric oxide, the sulphur passing off. Any silver sulphide that is present would have been converted into metallic silver at the outset were it not for the simultaneous presence of other sulphides, notably those of copper and of iron, which enables the silver sulphide to become converted into sulphate. The lead sulphide is also converted into sulphate at this low temperature. The heat is now raised still further with a view to split up the sulphate of copper, the decomposition of which leaves oxide of copper. If, as in this case, the bases are weak, the sulphuric anhydride escapes mainly as such; but when the sulphates of stronger bases are decomposed, the sulphuric anhydride is to a great extent decomposed into a mixture of sulphurous anhydride and oxygen. The sulphuric anhydride, resulting from the decomposition of this copper sulphate, converts the silver into sulphate, and maintains it as such, just as, in turn, at a lower temperature, the copper itself had been maintained in the form of sulphate by the sulphuric anhydride eliminated from the iron sulphide. When only a little of the copper sulphate remains undecomposed, the silver sulphate begins to split up, and the furnace charge must therefore be immediately withdrawn, or the whole of the silver sulphate would be converted into metallic silver, partly by the direct action of heat alone and partly by reactions such as those shown in the following equations:—

$$Ag_2SO_4 + 4Fe_3O_4 = 2Ag + 6Fe_2O_3 + SO_2,$$
$$Ag_2SO_4 + Cu_2O = 2Ag + CuSO_4 + CuO.$$

If the charge were not withdrawn, the silver would thus be effectually removed from the solvent action of water, and the smelter's efforts would have failed entirely. The charge still contains lead sulphate, which cannot be completely decomposed at any temperature attainable in the roasting furnace, except in the presence of silica, and it is well to leave it where it is if the residue has subsequently to be smelted with a view to the extraction of the gold. The elimination of arsenic and antimony gives rise to problems of much interest, and again confronts the smelter with a case of chemical equilibrium. For the sake of brevity it will be well for the present to limit the consideration to the removal of antimony, which may be supposed to be present as sulphide. Some sulphide of antimony is distilled off, but this is not its only mode of escape. An attempt to remove antimony by rapid oxidation would be attended with the danger of converting it into insoluble antimoniates of the metals present in the charge. In the early stages of the roasting it is therefore necessary to employ a very low temperature, and the presence of steam is found to be useful as a source of hydrogen, which removes sulphur as hydrogen sulphide, the gas being freely evolved. The reaction

$$Sb_2S_3 + 3H_2 = 3H_2S + 2Sb$$

between hydrogen and sulphide of antimony is, however, endothermic, and could not, therefore, take place without the aid which is afforded by external heat. The facts appear to be as follows: sulphide of antimony, when heated, dissociates, and the tension of the sulphur vapour would produce a state of equilibrium if the sulphur thus liberated were not seized by the hydrogen and removed from the system. The equilibrium is thus destroyed, and fresh sulphide is dissociated; the general result being that the equilibrium of the system is continually restored and destroyed until the sulphide is decomposed. The antimony combines with oxygen, and escapes as volatile oxide, as does also the arsenic, a portion of which is volatilized as sulphide.

The main object of the process which has been considered is the formation of soluble sulphate of silver. If arsenic and antimony have not been eliminated, their presence at the end of the operation would be specially inconvenient, as they give rise to the formation of arseniate and antimoniate of silver, insoluble in water, which may necessitate the treatment of the residues by an entirely different process from that which has hitherto been considered.

It will have been evident that effecting this series of changes demands the exercise of the utmost skill, care, and patience. The operations beginning at a dull red heat, or a temperature of some 500°, are completed at 700°, within a range, that is, of 200°. Judicious stirring has been necessary to prevent the formation of crusts of sulphates, which would impede the reactions, and, as has been shown, an undue elevation of temperature within a very limited range would, at any stage, have been fatal to the success of the operation. It is difficult to appreciate too highly the delicacy of sight and touch which enables an operator to judge by the aid of rough tests, but mainly from the tint of the streak revealed when the mass is rabbled, whether any particular stage has or has not been reached, and it will be obvious that the requisite skill is acquired solely by observation and experiment. The technical instructor may impart information as to the routine to be followed, and the appearances to be observed, but scientific knowledge of a high order can alone enable the operator to contend with the disturbing influences introduced by the presence of unexpected elements or by untoward variations in temperature. In the training of a metallurgist it is impossible to separate education from instruction, and the above description of a very ordinary operation will show the intimate relations between science and practice which are characteristic of metallurgical operations. Practice is dependent on science for its advancemement, but scientific workers too often hesitate to attack metallurgical problems, and to devote the resources of modern investigation to their solution, because they are not aware of the great interest of the physical and chemical problems which are connected with many very simple metallurgical processes, especially with those that are conducted at high temperatures.

Proceeding yet one step further, suppose that the copper-smelter takes possession of the residual mass, consisting mainly of oxide of copper, he would smelt it with fresh sulphide ores, and obtain, as a slag from the earthy matters of the ore, a ferrous silicate containing some small proportion of copper. The displacement of the copper from this silicate may be effected by fusing it with sulphide of iron, a fusible sulphide of iron and copper being formed, which readily separates from the slag. By this reaction some twenty thousand tons of copper are added to the world's annual production. Proceeding yet a step further, suppose the smelter to have reduced his copper to the metallic state. If arsenic had been originally present in the ore, and had not been eliminated entirely in the roasting, extraordinary difficulties will be met with in the later stages of the process, in extracting small quantities of arsenic which resist the smelter's efforts. Copper, moreover, containing arsenic cannot be "overpoled," as the presence of arsenic hinders the reducing action of gases on the copper. The amount of arsenic which the copper-smelter has to remove may vary from mere traces up to 1 per cent., and if the copper is destined for the use of the electrical engineer, he will insist on its being as pure as possible, for the presence of a trace of arsenic would materially increase the electrical resistance of the copper, and would be fatal to its use in submarine telegraphy. If, on the other hand, the copper is intended for the maker of locomotive fire-boxes, he will encourage the retention of small quantities of arsenic, as it is found to actually increase the endurance of the copper, and the smelter will in such a case have no inducement to employ the basic furnace lining which Mr. Gilchrist has offered him, nor will he care to use the special methods for the removal of arsenic with which he is familiar. It may all seem simple enough, but the modern process of copper-smelting has been laboriously built up, and has a long and interesting pedigree which may be traced to at least the eighth century, when Geber described the regulus, "coarse metal," as being "black mixed with livid," and our familiar "blue metal" as being "of a most clean and pleasant violet colour," and indicated the reason for the difference.[1]

(3) The foregoing instances have been given to indicate the general nature of metallurgical chemistry. It will be well now to show how the great advances in metallurgical practice have been made in the past, with a view to ascertain what principles should guide us in the future.

It is a grave mistake to suppose that in industry, any more than in art, national advance takes place always under the guidance of a master possessed of some new gift of invention; yet we have been reminded that we are apt to be reverent to these alone, as if the nation had been unprogressive and suddenly awakened by the genius of one man. The way for any great technical advance is prepared by the patient acquisition of facts by investigators of pure science. Whether the investigators are few or many, and consequently whether progress is slow or rapid, will depend in no small measure on the spirit of the nation as a whole. A genius whose practical order of mind enables him to make some great invention suddenly arises, apparently by chance, but his coming will, in most cases, be found to have "followed hard upon" the discovery by some scientific worker of an important fact, or even the accurate determination of a set of physical constants. No elaborate monograph need have reached the practical man—a newspaper paragraph, or a lecture at a Mechanics' Institute may have been sufficient to give him the necessary impulse; but the possessors of minds which are essentially practical often forget how valuable to them have been the fragments of knowledge they have so insensibly acquired that they are almost unconscious of having received any external aid.

The investigating and the industrial faculty are sometimes, though rarely, united in one individual. Rapid advance is often made by those who are untrammelled by a burden of precedent, but it should be remembered that though the few successes, which have been attained in the course of ignorant practice, may come into prominence, none of the countless failures are seen.

I would briefly direct attention to certain processes which have been adopted since the year 1849, when Dr. Percy presided over this Section at Birmingham, a great metallurgical centre. In that year the President of the Association made a reference to metallurgy, a very brief one, for Dr. Robinson only said "the manufacture of iron has been augmented six-fold by the use of the puddling-furnace and the hot-blast, both gifts of theory"; and so, it may be added, are most of the important processes which have since been devised. Take the greatest metallurgical advance of all, the Bessemer process, which has probably done more than any other to promote the material advance of all countries. It was first communicated to the world at the Cheltenham Meeting of the British Association, 1856. Its nature is well known, and I need only say that it depends on the fact that when air is blown through a bath of impure molten iron, sufficient heat is evolved by the rapid combustion of silicon, manganese, and carbon, to maintain the bath fluid after these elements have been eliminated, there being no external source of heat, as there is in the puddling furnace or the refinery hearth. We have recently been told that, at an early and perilous stage of the Bessemer process, confidence in the experiments was restored by the observation that the temperature of the "blown" metal contained in a crucible was higher than that of the furnace in which it was placed. The historian of the future will not fail to record that the way for the Bessemer process had been prepared by the theoretical work of Andrews, 1848, and of Favre and Silbermann, 1852, whose work on the calorific power of various elements showed that silicon and phosphorus might be utilized as fuel, because great heat is engendered by their combustion.

The basic process for removing phosphorus, a process of great national importance, the development of which we owe to Thomas and Gilchrist, is entirely the outcome of purely theoretical teaching, in connection with which the names of Grüner and Percy deserve special mention. In the other great group of processes for the production of steel, those in which Siemens's regenerative furnace is employed, we have the direct influence of a highly trained theorist, who concluded his address as President of this Association in 1882 by reminding us that "in the great workshop of Nature there is no line of demarcation to be drawn between the most exalted speculation and commonplace practice." The recent introduction of the method of heating by radiation is, of course, the result of purely theoretical considerations.

The progress in the methods of extracting the precious metals has been very great, both on the chemical and engineering sides, but it is curious that in the metallurgy of gold and silver, many ancient processes survived which were arrived at empirically—a noteworthy exception being presented by the chlorine process for refining gold, by the aid of which many millions sterling of gold have been purified. The late Mr. H. B. Miller based this process for separating silver from gold on the knowledge of the fact that chloride of gold cannot exist at a bright red heat. The tension of dissociation of chloride of gold is high, but the precious metal is not carried forward by the gaseous stream, at least not while chloride of silver is being formed.

The influence of scientific investigation is, however, more evident in that portion of the metallurgic art which deals with the adaptation of metals for use, rather than with their actual extraction from the ores.

Only sixteen years ago Sir Nathaniel Barnaby, then Director of Naval Construction, wrote, "Our distrust of steel is so great that the material may be said to be altogether unused by private ship-builders, . and marine engineers appear to be equally afraid of it." He adds, "The question we have to put to the steel makers is, What are our prospects of obtaining a material which we can use without such delicate manipulation and so much fear and trembling?" All this is changed, for, as Mr. Elgar informs me, in the year ending on June 30 last, no less than 401 ships, of three-quarters of a million gross tonnage, were being built of steel in the United Kingdom.

Why is it, then, that steel has become the material on which we rely for our ships and for our national defence, and of which such a splendid structure as the Forth Bridge is constructed? It is because, side by side with great improvement in the quality of certain varieties of steel, which is the result of using the open-hearth process, elaborate researches have shown what is the most suitable mechanical and thermal treatment for the metal; but the adaptation of steel for industrial use is only typical, as the interest in this branch of metallurgy generally appears for the moment to be centred in the question whether metals can, like many metalloids, pass under the application of heat or mechanical stress from a normal state to an allotropic one, or whether metals may even exist in numerous isomeric states.

It is impossible to deal historically with the subject now, further than by stating that the belief of more than one "modification" is old and widespread, and was expressed by Paracelsus, who thought that copper "contains in itself its female," which could be isolated so as to give "two metals" . . . "different in their fusion and malleability" as steel and iron differ. Within the last few years Schützenberger has shown that two modifications of copper can exist, the normal one having a density of 8·95, while that of the allotropic modification is only 8·0, and is moreover rapidly attacked by dilute nitric acid, which is without action on ordinary copper. It may be added that Lord Rayleigh's plea for the investigation of the simpler chemical reactions has been partly met, in the case of copper, by the experiments conducted by V. H. Veley on the conditions of chemical change between nitric acid and certain metals.

Bergmann, 1781, actually calls iron polymorphous, and says that it plays the part of many metals, "Adeo ut jure dici queat polymorphum ferrum plurium simul metallorum vices sustinere." Osmond has recently demonstrated the fact that at least two modifications of iron must exist.

Prof. Spring, of Liége, has given evidence that in cooling lead-tin alloys polymerization may take place after the alloys have become solid, and it seems to be admitted that the same cause underlies both polymerization and allotropy. The phenomenon of allotropy is dependent upon the number of the atoms in each molecule, but we are at present far from being able to say what degree of importance is to be attached to the relative distance between the atoms of a metal or to the "position of one and the same atom" in a metallic molecule, whether the metal be alloyed or free, and it must be admitted that in this respect organic chemistry is far in advance of metallurgic chemistry. I cannot, as yet, state what is the atomic grouping in the brilliantly-coloured gold-aluminium alloy, $AuAl_2$, which I have had the good fortune to discover, but, in it, the gold is probably present in the same state as that in which it occurs in the purple of cassius.

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[1] It must not be supposed that when commercially pure copper lies on the furnace bed, ready to be transferred to moulds, that its turbulent career of reactions is over. It might be thought that the few tenths per cent. of impurity, dissolved oxide, and occluded gas, are so far attenuated by distribution that their interactions must be insignificant. This is far from being the case. I believe the bath of metal is seething from its reactions until the copper is solid, and then polymerization proceeds. There may not be a sharply-defined, critical range of temperature within which the metal can alone be successfully worked, and which varies, as regards its starting-point, with the kind of impurity present, as is the case with steel; but evidence of molecular change in the solid metal is afforded by the pyrometric curves of cooling referred to on p. 405, and by the singular behaviour as regards electrical resistance, of various samples of copper, in which chemical analysis hardly reveals a difference.

Much valuable information on the important question of allotropy in metals has already been gathered by Pionchon, Ditte, Moissan, Le Chatelier, and Osmond, but reference can only be made to the work of the two latter. Le Chatelier concludes that in metals which do not undergo molecular transformation the electrical resistance increases proportionally to the temperature. The same law holds good for other metals at temperatures above that at which their last change takes place; for example, in the case of nickel above 340°, and in that of iron above 850°.

It is probable that minute quantities of foreign matter, which profoundly modify the structure of masses of metal, also induce allotropic changes. In the case of the remarkable action of impurities upon pure gold I have suggested that the modifications which are produced may have direct connection with the periodic law of Mendeleeff, and that the causes of the specific variations in the properties of iron and steel may thus be explained. The question is of great industrial importance, especially in the case of iron; and Osmond, whose excellent work I have already brought before the members of this Association in a lecture delivered at Newcastle in 1889, has especially studied the influence upon iron exerted by certain elements. He shows that elements whose atomic volumes are smaller than that of iron delay, during the cooling of a mass of iron from a red heat, the change of the β, or hard variety of iron, to the α, or soft variety. On the other hand, elements whose atomic volumes are greater than that of iron tend to hasten the change of β to α iron. It is, however, unnecessary to dwell upon this subject, as it was dealt with last year in the address of the President of the Association.

It may be added that the recent use of nickel-steel for armour-plate, and the advocacy of the use of copper-steel for certain purposes, is the industrial justification of my own views as to the influence of the atomic volume of an added element on the mechanical properties of iron, and it is remarkable that the two bodies, silicon and aluminium, the properties of which when in a free state are so totally different, should, nevertheless, when they are alloyed with iron, affect it in the same way. Silicon and aluminium have almost the same atomic volumes.

The consequences of allotropic changes which result in alteration of structure are very great. The case of the tin regimental buttons which fell into a shapeless heap when exposed to the rigorous winter at St. Petersburg, is well known. The recent remarkable discovery by Hopkinson of the changes in the density of nickel-steel (containing 22 per cent. of nickel) which are produced by cooling to − 30°, affords another instance. This variety of steel, after being frozen, is readily magnetizable, although it was not so before; its density, moreover, is permanently reduced by no less than 2 per cent. by the exposure to cold; and it is startling to contemplate the effect which would be produced by a visit to the Arctic regions of a ship of war built in a temperate climate of ordinary steel, and clad with some three thousand tons of such nickel-steel armour; the shearing which would result from the expansion of the armour by exposure to cold would destroy the ship. Experimental compound armour-plates have been made, faced with 25 per cent. nickel-steel, but it remains to be seen whether a similar though lessened effect would be produced on the steel containing 5 to 7 per cent. of nickel, specially studied by J. Riley, the use of which is warmly advocated for defensive purposes. Further information as to the molecular condition of nickel-steel has within the last few weeks been given by Mercadier, who has shown that alloying iron with 25 per cent. of nickel renders the metal isotropic.

The molecular behaviour of alloys is indeed most interesting. W. Spring has shown, in a long series of investigations, that alloys may be formed at the ordinary temperature, provided that minute particles of the constituent metals are submitted to great pressure. W. Hallock has recently given strong evidence in favour of the view that an alloy can be produced from its constituent metals with but slight pressure if the temperature to which the mass is submitted be above the melting-point of the alloy, even though it be far below the melting-point of the most easily fusible constituent. A further instance is thus afforded of the fact that a variation of either temperature or pressure will effect the union of solids. It may be added that B. C. Damien is attempting to determine what variation in the melting-point of alloys is produced by fusing them under a pressure of two hundred atmospheres. Italian physicists are also working on the compressibility of metals, and F. Boggio-Lera has recently established the existence of an interesting relation between the coefficient of cubic compressibility, the specific gravity, and the atomic weight of metals.

Few questions are more important than the measurement of very high temperatures. Within the last few years H. Le Chatelier has given us a thermo-couple of platinum with platinum containing 10 per cent. of rhodium, by the aid of which the problem of the measurement of high temperatures has been greatly simplified. A trustworthy pyrometer is now at hand for daily use in works, and the liberality of the Institution of Mechanical Engineers has enabled me to conduct an investigation which has resulted in the adoption of a simple appliance for obtaining, in the form of curves, photographic records of the cooling of masses of metal. A report on the subject has already been submitted to a Committee, of which the Director-General of Ordnance Factories is the Chairman; and Dr. Anderson, to whom I am indebted for valuable assistance and advice, intends to add this new method for obtaining autographic curves of pyrometric measurements to the numerous self-recording appliances used in the Government factories which he controls. It has proved to be easy to ascertain, by the aid of this pyrometer, what thermal changes take place during the cooling of molten masses of alloys, and it is possible to compare the rate of cooling of a white-hot steel ingot at definite positions situated respectively near its surface and at its centre, and thus to solve a problem which has hitherto been considered to be beyond the range of ordinary experimental methods. Some of the curves already obtained are of much interest, and will be submitted to the Section. It is probable that the form of the curve which represents the solidification and cooling of a mass of molten metal affords an exceedingly delicate indication as to its purity.

Prof. H. E. Armstrong holds that the molecules of a metal can unite to form complexes with powers of coherence which vary with the presence of impurity. Crookes, by a recent beautiful investigation, has taught us how electrical evaporation of solid metals may be set up *in vacuo*, and has shown that even an alloy may be decomposed by such means. We may hope that such work will enable us to understand the principles on which the strength of materials depends.

Before leaving the consideration of questions connected with the molecular constitution of metals, I would specially refer to the excellent work of Heycock and Neville, who have extended to certain metals with low melting-point Raoult's investigations on the effect of impurity on the lowering of the freezing-point of solids. With the aid of one of my own students, H. C. Jenkins, I have further extended the experiments by studying the effect of impurity on the freezing-point of gold. Ramsay, by adopting Raoult's vapour-pressure method, has been led to the conclusion that when in solution in mercury the atom of a metal is, as a rule, identical with its molecule. The important research on the liquation of alloys has been extended by E. Matthey to the platinum-gold and palladium-gold series, in which the manipulation presented many difficulties; and E. J. Ball has studied the cases presented by the antimony copper-lead series. Dr. Alder Wright has continued his own important investigation upon ternary alloys; and A. P. Laurie has worked on the electromotive force of the copper-zinc and copper-tin and gold-tin series, a field of research which promises fruitful results.

In no direction is advance more marked than in the mechanical testing of metals, in which branch of investigation this country, guided by Kirkaldy, undoubtedly took the leading part, and in connection with which Kennedy and Unwin have established world-wide reputations. I would also specially mention the work which has been carried on at the Government testing works at Berlin under Dr. Wedding, and the elaborate investigations conducted at the Watertown Arsenal, Massachusetts, not to mention the numerous Continental testing laboratories directed by such men as Bauschinger, Jenny, and Tetmajer. Perhaps the most important recent work is that described by Prof. Martens, of Berlin, on the influence of heat on the strength of iron.

I might have dwelt at length on all these matters without doing half the service to metallurgy that I hope to render by earnestly pleading for the more extended teaching of the subject throughout the country, and for better laboratories, arranged on the model of engineering laboratories, in which the teaching is conducted with the aid of complete, though small, "plant." The Science and Art Department has done great and lasting service by directing that metallurgy shall be taught practically, but much remains to be done. With regard to laboratories in works, which are too often mere sheds, placed, say, behind the boiler-house, when may we hope to rival the German chemical firm which has recently spent £19,000 upon its laboratories, in which research will be vigorously conducted? There is hardly any branch of inorganic chemistry which the metallurgist can afford to neglect, while many branches both of physics and mechanics are of the utmost importance to him.

The wide range of study upon which a metallurgical student is rightly expected to enter is leading, it is to be feared, to diminution in the time devoted to analytical chemistry, and this most serious question should be pressed upon the attention of all who are responsible for the training of our future chemists. There can be no question that sufficient importance is not attached to the estimation of "traces," an analysis being considered to be satisfactory if the constituents found add up to 99·9, although a knowledge as to what elements represent the missing 0·1 may be more useful in affording an explanation of the defects in a material than all the rest of the analysis. This matter is of growing interest to practical men, and may explain their marked preference for chemists who have been trained in works, to those who have been educated in a college laboratory.

The necessity for affording public instruction in mining and metallurgy, with a view to the full development of the mineral wealth of a nation, is well known. The issues at stake are so vast, that in this country it was considered desirable to provide a centre of instruction in which the teaching of mining and metallurgy should not be left to private enterprise or even intrusted to a corporation, but should be under the direct control of the Government. With this end in view, the Royal School of Mines was founded in 1851, and has supplied a body of well-trained men who have done excellent service for the country and her colonies. The Government has recently taken a step in advance, and has further recognized the national importance of the teaching of mining and metallurgy by directing that the School of Mines shall be incorporated with the Royal College of Science, which is, I believe, destined to lead the scientific education of the nation.

It is to be feared that as regards metalliferous mining our country has seen its best days, but the extraordinary mineral wealth of our colonies has recently been admirably described by my colleague, Prof. Le Neve Foster, in the inaugural lecture he delivered early in the present year on his appointment to the chair so long held by Sir Warington Smyth (*Engineering*, vol. li., p. 200 *et seq.*). We shall, however, be able to rightly estimate the value of our birthright when the Imperial Institute is opened next year, and the nation will have reason to be grateful to Sir Frederick Abel for the care he is devoting to the development of this great institution, which will become the visible exponent of the splendours of our Indian and colonial resources, as well as a centre of information.

The rapid growth of technical literature renders it unnecessary for a President of a Section to devote his address to recording the progress of the subject he represents. As regards the most important part of our national metallurgy, this has, moreover, been admirably done by successive Presidents of the Iron and Steel Institute, but it may have been expected that references would have been made to the main processes which have been adopted since Percy occupied this chair in 1849. I have not done so, because an enumeration of the processes would have been wholly inadequate, and a description of them impossible in the time at my disposal. Nevertheless, it may be well to remind the Section of a few of the more prominent additions the art has received in the last half-century, and to offer a few statements to show the magnitude on which operations are conducted. As regards iron, in the last twenty-five years the price of steel has been reduced from £55 per ton to £5 per ton; but, after giving the world the inestimable boon of cheap steel by the labours of Bessemer and of Siemens, we were somewhat slow to accept the teaching of experiment as to the best method of treating the new material; on the other hand, Hadfield has brought manganese steel and aluminium steel within the reach of the manufacturer, and J. Riley has done much to develop the use of nickel-steel.

In the case of copper, we have mainly contributed to the extraordinary development of wet processes for its extraction from poor sulphides, and have met the great demands for pure metal by the wide adoption of electrolytic processes.

As regards the precious metals, this country is well to the front, for Great Britain and her colonies produce about 38 per cent. of the gold supply of the world; and it may be well to add, as an indication of the scale on which operations are conducted, that in London alone one ton of gold and five tons of silver bullion can easily be refined in a day. No pains have been spared in perfecting the method of assay by which the value of gold and silver is ascertained, and during my twenty years' connection with the Royal Mint I have been responsible for the accuracy of the standard fineness of no less than five hundred and fifty-five tons of gold coin, of an aggregate value of seventy millions five hundred thousand pounds sterling. In the case of the platinum industry we owe its extraordinary development to the skill and enterprise of successive members of the firm of Johnson, Matthey, and Co., who in later years have based their operations upon the results of the investigations of Deville and Debray. Some indication of the value of the material dealt with may be gathered from the statement that two and a half hundred-weight of platinum may easily be melted in a single charge, and that the firm, in one operation, extracted a mass of palladium valued at £30,000 from gold-platinum ore actually worth more than a million sterling.

I wish it were possible to record the services of those who have advanced metallurgy in connection with this Association, but the limitations of time render it difficult to do more than to refer to some honoured names of past presidents of this Section. Michael Faraday, President of this Section in 1837 and 1846, prepared the first specimen of nickel-steel, an alloy which seems to have so promising a future, but we may hardly claim him as a metallurgist; nor should I be justified in referring, in connection with metallurgical research, to my own master, Graham, President of this Section in 1839, and again in 1844, were it not that his experiments on the occlusion of gases by metals have proved to be of such extraordinary practical importance in connection with the metallurgy of iron. Sir Lyon Playfair presided over this Section in 1855, and again in 1859. His work in connection with Bunsen on the composition of blast-furnace gases was published in the Report of this Association in 1847, and formed the earliest of a group of researches, amongst which those of Sir Lowthian Bell proved to be of so much importance. The latter was President of this Section in 1889. Sir F. Abel, President of this Section in 1877, rendered enduring service to the Government by his elaborate metallurgical investigations in connection with materials used for guns and projectiles, as well as for defensive purposes. I will conclude this section of the address by a tribute to the memory of Percy. He may be said to have created the English literature of metallurgy, to have enriched it with the records of his own observations, and to have revived the love of our countrymen for metallurgical investigation. His valuable collection of specimens, made while Professor at the Royal School of Mines, is now appropriately lodged at South Kensington, and will form a lasting memorial of his labours as a teacher. He exerted very noteworthy influence in guiding the public to a just appreciation of the labours of scientific men, and he lived to see an entire change in the tone of the public press in this respect. In the year of Percy's presidency over this Section the *Times* gave only one-tenth of a column to a summary of the results of the last day but one of the meeting, although the usual discourse delivered on the previous evening had been devoted to a question of great importance—"The Application of Iron to Railway Purposes." Space was, however, found for the interesting statement that the "number of Quakeresses who attended the meetings of the Sections was not a little remarkable." Compare the slender record of the *Times* of 1849 with its careful chronicle of the proceedings at any recent meeting of the Association.

In drawing this address to a close, I would point to the great importance of extending the use of the less known metals. Attention is at present concentrated on the production of aluminium, and reference has already been made to the Cowles process, in which, as in that of Héroult, the reduction of alumina is effected by carbon, at the very high temperature of the electric arc; while, on the other hand, in the Kleiner and similar processes, the electric current acts less as a source of heat than by decomposing a fluid bath, the aluminium being isolated by electrolytic action; and doubtless in the immediate future, there will be a rapid increase in the number of metallurgical processes that depend on reactions which are set up by submitting chemical systems to electric stress. Incidental reference should be made to the growing importance of sodium, not only in cheapening the production of aluminium, but as a powerful weapon of research. In 1849, when Percy was President of this Section, magnesium was a curiosity; now its production constitutes a considerable industry. We may confidently expect to see barium and calcium produced on a large scale as soon as their utility has been demonstrated by research. Minerals containing molybdenum are not rare; and the metal could probably be produced as cheaply as tin if a use were to be found for it. The quantities of vanadium and thallium which are available are also far from inconsiderable; but we as yet know little of the action of any of these metals when alloyed with others which are in daily use. The field for investigation is vast indeed, for it must be remembered that valuable qualities may be conferred on a mass of metal by a very small quantity of another element. The useful qualities imparted to platinum by iridium are well known. A small quantity of tellurium obliterates the crystalline structure of bismuth; but we have lost an ancient art, which enabled brittle antimony to be cast into useful vessels. Two-tenths per cent. of zirconium increases the strength of gold enormously, while the same amount of bismuth reduces the tenacity to a very low point. Chromium, cobalt, tungsten, titanium, cadmium, zirconium, and lithium are already well known in the arts, and the valuable properties which metallic chromium and tungsten confer upon steel are beginning to be generally recognized, as the last Exhibition at Paris abundantly showed; but as isolated metals we know but little of them. Is the development of the rarer metals to be left to other countries? Means for the prosecution of research are forthcoming, and a rich reward awaits the labours of chemists who could bring themselves to divert their attention, for even a brief period, from the investigation of organic compounds, in order to raise alloys from the obscurity in which they are at present left.

It must not be forgotten that metallurgical enterprise rests on (1) scientific knowledge, (2) capital, and (3) labour; and that, if the results of industrial operations are to prove remunerative, much must depend on the relation of these three elements, though it is difficult to determine accurately their relative importance. A modern ironworks may have an army of ten thousand workmen, and commercial success or failure will depend in no small measure on the method adopted in organizing the labour. The relations between capital and labour are of so much interest at the present time that I do not hesitate to offer a few words on the subject.

Many examples might be borrowed from metallurgical enterprises in this and other countries to show that their nature is often precarious, and that failure is easily induced by what appear to be comparatively slight causes. Capitalists might consequently tend to select Government securities for investment in preference to metallurgical works, and the labouring population would then severely suffer. It is only reasonable, therefore, that if capitalists are exposed to great risks, they should, in the event of success, receive the greater part of the profits. There is a widespread feeling that the interests of capital and labour must be antagonistic, and as it is impossible to ignore the fact that the conflict between them is giving rise to grave apprehension, it becomes the duty of all who possess influence to strive not merely for peace, but to range themselves on the side of justice and humanity. The great labour question cannot be solved except by assuming as a principle that private ownership must be held inviolable; but it must be admitted that there was a time when capital had become arbitrary, and some kind of united action on the part of workmen was needed in self-defence. If, however, we turn to the action of the leaders of trades unions in the recent lamentable strikes, we are presented with a picture which many of us can only view as that of tyranny of the most close and oppressive kind, in which individual freedom cannot even be recognized. There are hundreds of owners of works who long to devote themselves to the true welfare of those they employ, but who can do little against the influence of the professional agitator, and are merely saddened by contact with prejudice and ignorance. I believe the view to be correct that some system by which the workman participates in the profits of enterprise will afford the most hope of putting an end to labour disputes, and we are told that profit-sharing tends to destroy the workmen's sense of social exclusion from the capitalistic board, and contents him by elevating him from the precarious position of a hired labourer. No pains should, therefore, be spared in perfecting a system of profit-sharing.

Pensions for long service are great aids to patience and fidelity, and very much may be hoped from the fact that strenuous efforts are being made by men really competent to lead. The Report of the Labour Commission which is now sitting will be looked for with keen interest. Watchful care over the health, interests, and instruction of the employed is exercised by many owners of works; and in this respect the Dowlais Works, which are being transplanted into your midst at Cardiff, have long presented a noteworthy example. Workmen must not forget that the choice of their own leaders is in their own hands, and on this the future mainly depends. "We may lay it down as a perpetual law that workmen's associations should be so organized and governed as to furnish the best and most suitable means for attaining what is aimed at—that is to say, for helping each individual member to better his condition to the utmost in body, mind, and property." These words will be found in the Encyclical Letter which Pope Leo XIII. has recently issued on the "Condition of Labour." To me it is specially interesting that the Bishop of Rome in his forcible appeal again and again cites the opinion of St. Thomas Aquinas, who was a learned chemist as well as a theologian.

Those of us who realize that "the higher mysteries of being, if penetrable at all by human intellect, require other weapons than those of calculation and experiment," should be fully sensible of our individual responsibility. Seeing that the study of the relations between capital and labour involves the consideration of the complex problems of existence, the solution of which is at present hidden from us, we shall feel with Andrew Lang that "where, as matter of science, we know nothing, we can only utter the message of our temperament." My own leads me to hope that the patriotism of the workmen will prevent them from driving our national industries from these shores; and I would ask those to whom the direction of the metallurgical works of this country is confided to remember that we have to deal both with metals and with men, and have reason to be grateful to all who extend the boundaries, not only of our knowledge, but also of our sympathy.

SECTION D.

BIOLOGY.

OPENING ADDRESS BY FRANCIS DARWIN, M.A., M.B., F.R.S., FELLOW OF CHRIST'S COLLEGE, CAMBRIDGE, PRESIDENT OF THE SECTION.

On Growth-curvatures in Plants.

A SEEDLING plant, such as a young sunflower, when growing in a state of nature, grows straight up towards the open sky, while its main root grows straight down towards the centre of the earth. When it is artificially displaced, for instance by laying the flower-pot on its side, both root and stem execute certain curvatures by which they reach the vertical once more. Curvatures such as these, whether executed in relation to light, gravitation, or other influences, may be grouped together as growth-curvatures, and it is with the history of our knowledge on this subject that I shall be occupied to-day. I shall principally deal with geotropic curvatures, or those executed in relation to gravitation, but the phenomena in question form a natural group, and it will be necessary to refer to heliotropism, and, indeed, to other growth-curvatures. The history of the subject divides into two branches, which it will be convenient to study separately.

When a displaced apogeotropic organ curves so as to become once more vertical, two distinct questions arise, which may be briefly expressed thus:—

(1) How does the plant recognize the vertical line; how does it know where the centre of the earth is?

(2) In what way are the curvatures which bring it into the vertical line executed?

The first is a question of irritability, the second of the mechanism of movement. Sachs has well pointed out that these two very different questions have been confused together (*Arbeiten*, ii. p. 282, 1879). They should be kept as distinct as the kindred questions, How, by what nervous apparatus, does an animal perceive changes in the external world; and how, by what muscular machinery, does it move in relation to such changes?

The history of our modern knowledge of geotropism may conveniently begin with Hofmeister's researches, because in an account of his work some of the points which re-occur in recent controversy are touched, and also because in studying his work the necessity of dividing the subject into the two above-named headings, Irritability and Mechanism, will be more clearly perceived.

In 1859 (*Berichte d. k. Sächs. Ges. d. Wiss.*), Hofmeister published his researches on the effect of disturbance, such as shaking or striking a turgescent shoot. This appears at first sight sufficiently remote from the study of geotropism, but the facts published in this work were the basis of the theory of geotropism formed by Hofmeister and accepted with some modification by Sachs. When an upright, vigorously-growing, turgescent shoot is struck at its base the upper end is made to curve violently towards the side from which the blow came. When the shoot comes to rest it is found to be no longer straight, but to have acquired a permanent bend towards the side on which it was struck. In explaining this phenomenon Hofmeister described those conditions of growth which give rise to what is known as the tension of tissues; these facts are still an important part of botanical study, though they hold quite a different position from that assigned to them by Hofmeister. The classification into active or erectile tissue and passively extended tissue was then first made. The pith, which is compressed, and strives to become longer, is the active or erectile part, the cortical and vascular constituents being passively extended by the active tissue. Hofmeister showed that when the shoot is violently bent the elasticity of the passive tissues on the convex side is injured by overstretching. The system must assume a new position of equilibrium; the passive tissues are now no longer equally resisting on the two sides, and the shoot must necessarily assume a curvature towards that side on which passive tissues are most resisting.

In a second paper, in 1860, Hofmeister (*Berichte d. k. Sächs. Ges. d. Wiss.*) applied these principles to the explanation of geotropism. It is true that in his second paper he does not refer to the former one, but I think that it can hardly be doubted that the knowledge which supplied the material for his paper of 1859 suggested the theory set forth in 1860. He had shown that in the system of tensions existing in a turgescent shoot lay the power of producing artificial curvatures, and he applied the same principle to the natural curvatures. When an apogeotropic organ is placed in a horizontal position, Hofmeister[1] supposed that the resisting tissues on the lower side became less resisting, so that they yielded more readily than those on the upper side to the longitudinal pressure of the turgescent pith. The system in such a case comes to rest in a new position, the shoot curving upwards; the passive tissues on the upper and lower sides once more resist the expansion of the pith in equal degrees. In this way Hofmeister hit on an explanation which, as far as mechanism is concerned, is in rough outline practically the same as certain modern theories, which will be discussed in the sequel.

His views resembled more modern theories in this, too: he clearly recognized that they were, *mutatis mutandis*, applicable to acellular[2] organs. The manner in which Hofmeister compared the mechanics of multicellular and acellular parts was curious; nowadays we compare the turgescent pith of a growing shoot with the hydrostatic pressure inside the acellular organ. Just as the pressure inside a single cell stretches the cell-walls, so in a growing shoot the turgescent pith stretches the cortex. As pith is to cortex, so is cell-pressure to cell-membrane. But Hofmeister would not have accepted any such comparison. In the case of acellular organs he localized both the erectile and passive tissues in the membrane. The cuticle was said to be passively extended by the active growth of the inner layers of the cell-wall.

It is remarkable that the obvious source of power which the pressure of the cell-sap against the cell-walls supplies should have been so much neglected. This may perhaps be accounted for as a revulsion against the excessive prominence given to osmosis in the works of Dutrochet.

The great fault of Hofmeister's views was the purely mechanical manner in which he believed changes in extensibility in the passive tissues to be brought about. When an apogeotropic shoot is placed horizontal there would be a tendency, according to Hofmeister, for the resisting passive tissues along the lower side of the shoot to become waterlogged owing to the fluid in the shoot gravitating towards that side. They would thus be rendered more extensible, and the shoot would bend up, since its lower parts would yield to the erectile tissues in the centre. Such a conception excludes the idea of gravitation acting as a stimulus, and tends to keep geotropism out of the category in which it now takes its place along with such obvious cases of response to stimulation as the movements of Mimosa. In this respect it was a retrogression from the views of some earlier writers. Dutrochet's clear statement (1824) as to growth-curvatures being an affair of stimulus and response will be quoted lower down. Treviranus, in his "Physiologie" (1838), speaks of geotropism as a *Trieb*, or impulse, and adds that though there is no question of desire or sensation, as in the impulses of animals, yet geotropism must be thought of as something higher than a merely mechanical or chemical action.

In taking such a view Hofmeister naturally neglected the biological side of the study of geotropism. Now, we think of gravitation as a stimulus, which the plant translates according to its needs. The plant, so to speak, knows where the centre of the earth is, and either grows away from it, or towards it, according as either direction suits its mode of existence.

We have seen how Hofmeister's view enabled him to apply a common explanation to acellular and multicellular organisms. But it led him into an error which more than counterbalances the credit due to such a generalization—namely, into separating what are now universally considered parts of a single phenomenon—viz. negative and positive geotropism. He gave totally different explanations of the bending down of a root and the bending up a stem. It is well known that he supposed a root to be plastic, and to bend over by its own weight, like a tallow candle on a hot day or a piece of heated sealing-wax.

The development of a unified view of heliotropism, geotropism, and other similar curvatures is a part of my subject, and for that reason the curious want of unity in Hofmeister's views is interesting.

In 1865, Sachs published his "Experimental-Physiologie." He here accepts Hofmeister's views with certain modifications.

Irritability.

When by a touch on a trigger the explosion of a pistol is caused, we do not say that the pistol is irritable, but when in an organism a similar release of stored-up energy occurs, we apple the term *irritability* to the phenomenon, and we call the influency which produced the change a stimulus. At this time (1865) there was, as far as I can discover, no idea that growth-curvatures were produced by external influences acting as stimuli. Gravitation and light were supposed to act directly, and not as releasing forces. This is all the more remarkable, because Dutrochet[1] had expressed with great clearness the conception which we now hold. He wrote:—"La cause inconnue de l'attraction n'est que la cause occasionelle du mouvement descendant des racines et de l'ascension des tiges; elle n'en est point la cause immédiate; elle agit dans cette circonstance comme agent nervimoteur. Nous verrons plus bas de nouvelles preuves de la généralité de ce fait important en physiologie, savoir que les mouvements visibles des végétaux sont tous des mouvements *spontanés*, exécutés à l'occasion de l'influence d'un agent extérieur et non des mouvements imprimés par cet agent." Nothing could be more to the purpose than this, and it is one of the most curious points in the history of the subject that the botanical mind should have taken more than fifty years to assimilate Dutrochet's view.

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[1] Knight had previously suggested an explanation (Philosophical Transactions, 1806), which is so far similar, that the sinking downwards by gravitation of the juices of the plant is supposed to be the primary cause of apogeotropism. Knight's explanation of positive geotropism is practically the same as Hofmeister's.

[2] Sachs's term *acellular* is, in the present connection, equivalent to *unicellular*.

[1] "Recherches anat. sur la Structure intime, &c." (1824), p. 107. Dutrochet, however, was not consistent in this matter, and later on gave explanations as mechanical as Hofmeister's.

In 1868 Albert Bernhard Frank published his valuable "Beiträge zur Pflanzenphysiologie," which was of importance in more than one way. In this work the term "geotropism" was first suggested in imitation of the existing expression "heliotropism." This uniformity of nomenclature had an advantage beyond mere convenience, for it served to emphasize the view that the curvatures were allied in character. His criticisms of Hofmeister and Sachs were directed against the following views:—

(i.) That roots and other positively geotropic organs bend owing to plasticity. By repeating and varying certain older experiments, Frank helped materially to establish the now universally accepted view that positive geotropism is an active, not a passive, curvature, and that it depends, like apogeotropism, on unequal distribution of longitudinal growth. Here, again, he introduced unity, bringing what had been considered different phenomena under a common heading. By studying the distribution of growth and of tension in a variety of curvatures he helped still more to unite them under a common point of view.

(ii.) He showed that Hofmeister's classification of organs into those (1) which have and (2) which have not tension, was valueless in connection with growth-curvatures; that is to say, that apogeotropism is not necessarily connected with the form of longitudinal tension found in growing shoots, and that the distinct kind of tension existing in roots has no connection with their positive geotropism. His work thus served to bring the subject into a more purely physiological condition, not only by his downright opposition to a mechanical theory backed by the great name of Hofmeister, but also by giving importance to physiological individuality.

In 1870, Frank published a more important work, "Die natürliche wagerechte Richtung der Pflanzentheilen." This paper not only tended to unite geotropism and heliotropism by proving the phenomena described to be common to both categories, but it more especially widened the field of view by showing that horizontal growth must be considered as kindred to vertical growth, and thus introduced a new conception of the reaction of plants to light and gravitation which has been most fruitful.

Frank showed that certain parts of plants, for instance the runners of the strawberries, even when kept in the dark, grow horizontally, and when displaced from the horizontal returned to it. Here, said Frank, is a new type of geotropism, neither positive nor negative, but *transverse*. Ten years later Elfving (Sachs's *Arbeiten*, 1880), working in Sachs's laboratory, got similar results with rhizomes of Scirpus, &c. These experiments are more conclusive than Frank's in one way, because the strawberry runners when darkened were in abnormal conditions, whereas the rhizomes used by Elfving were normally freed from light-effects. When a rhizome which has been placed so as to point obliquely upwards, moves down towards the horizontal position it is, according to the old nomenclature, positively geotropic, and, *vice versâ*, when it reaches the horizontal from below it is negatively geotropic. But it cannot be both positively and negatively geotropic. We are bound to assume that it is so organized that it can only assume a position of rest, and continue to grow in a straight line when it is horizontal, just as an ordinary geotropic organ cannot devote itself to rectilinear growth unless it is vertical. In this way Frank's conception of transverse geotropism paved the way for the theory that there are a variety of different organizations (or, as we may now say, irritabilities) in growing plants; and that, whether a plant grows vertically upwards or downwards or horizontally, depends on the individual and highly sensitive constitution of the plant in question. It is, of course, true that those who seek for mechanical explanations of growth curvatures might be able to find such a one for transverse geotropism. But when Frank's conception has once been seized such views are less and less acceptable; and, judging from my own experience, I cannot doubt that Frank's work deserved to have a powerful effect in preparing the minds of physiologists for a just view of irritability.

The belief in transverse geotropism received interesting support from Vöchting's work ("Die Bewegung der Bluthen und Früchte," 1882) on the movement of certain flowers which retain a horizontal position under the influence of gravitation.

Frank's views, it may be added, were accepted by my father and myself in our "Power of Movement," in which the term diageotropism was proposed, and has been generally accepted, for transverse geotropism. Nevertheless, though Frank was undoubtedly right, his views were strongly opposed at the time. He held similar views on the effect of light, believing that the power possessed by leaves of placing themselves at right angles to the direction of incident light must be considered as a new type of heliotropic movement, transverse or diaheliotropism. Frank's views were criticized and opposed by De Vries (Sachs's *Arbeiten*, 1872), who, by means of experiments carried out in the Würzburg Laboratory, tried to show that Frank's results can be explained without having resort to new types of geo- or heliotropism. De Vries believed, for instance, that a leaf may be apheliotropic and apogeotropic, and that its horizontal position under vertical illumination is due to a balance struck between the opposing tendencies, one of which calls forth an upward, the other a downward curvature.

The same point of view occurs again in Sachs's paper on "Orthotrope and Plagiotrope Plant-members" (Sachs's *Arbeiten*, 1879). Sachs holds to the opinion that Frank's theory is untenable, that it is upset by De Vries, and that the oblique or horizontal position is to be explained as the result of a balance between opposing tendencies.

In a paper published the following year, 1880 (Journal Linn. Soc.), I attempted to decide between the opposing views. My experiments proved that at least certain leaves can place themselves at right angles to the direction of incident light when there is no possibility of a balance being struck. The outcome of my experiments was to convince me that Frank's views are correct—namely, that the quality of growth called transverse heliotropism does exist.

This view was accepted by my father in the "Power of Movement." The conclusions of Vöchting, in the *Bot. Zeitung*, 1888, and Krabbe in Pringsheim's *Jahrbücher*, 1889, vol. xx., are on the same side of the question.

The general result of these confirmations of Frank's conception has been to bring to the front a belief in the individuality of the plant in deciding what shall be the effect of external conditions. Such a view does not necessarily imply irritability in a strict sense, for Frank himself explained the facts, as we shall see, in a different way. But it could not fail to open our eyes to the fact that in growth-curvatures, as in other relations to environment, external changes are effective as guides or sign-posts, not as direct causes.

Frank saw clearly that plants may gain such various aptitudes for reacting to light and gravitation as best suit their modes of life.

In stating this view, he refers to the influence of the "Origin of Species," which had shown how any qualities useful to living things might be developed by natural selection. Frank described the qualities thus gained under the term *polarity*. He supposed that the cell-membranes of a transversely heliotropic leaf (for instance) were so endowed that a ray of light striking it obliquely from base to apex produced an increase of growth on the side away from the light; while a ray oblique from apex to base caused a reverse movement. The polarity-assumption of Frank is a purely gratuitous one, and, if strictly interpreted, hardly tends to bring growth-curvatures into harmony with what we know of the relation of life to environment.

It will no doubt appear to be a forcing of evidence if, after such a statement as the last, I still claim for Frank that he led the way to our modern view of irritability. I can, of course, only judge of the effect of his writings on myself, and I feel sure that they prepared me to accept the modern views. It must also be insisted that Frank, in spite of his assumption of polarity, seems to have looked at the phenomena in a manner not very different from ours of the present day. Thus, he compares the action of gravitation on plants to the influence of the perception of food on a chicken. He speaks, too, of custom (Journal Linn. Soc., 1880, p. 91), or use, building up the specialized "instinct" for certain curvatures. These are expressions consistent with our present views, and I think that Vines ("Physiology") is perfectly just in speaking of Frank's belief in different kinds of irritability, although in so judging he may perhaps have followed equity rather than law.

One of the chief bars to the development of our present views on irritability was the fact that simple growth in length is influenced, and markedly influenced, by differences in illumination. Plants grow more quickly, *cæteris paribus*, in darkness than in light. With this fact to go on, it was perfectly natural that simple mechanical explanations of heliotropism should be made. De Candolle, as is well known, explained such curvatures by the more rapid growth of the shaded side. Thus it came about that heliotropism was discussed, for instance, in Sachs's "Text-book," edit. 4, 1874, under the same heading as the influence of light on rectilinear growth.

Shortly afterwards, in 1876, a pupil of Sachs—Müller-Thurgau—published ("Flora") a research carried out in the Würzburg Laboratory, which is of some importance. In the introductory remarks he wrote:—"It has been hitherto supposed that heliotropic curvatures depend on a difference in intensity of illumination on the two sides. Sachs came to a different opinion in his work on geotropism: he found himself compelled to believe that in heliotropic, just as in geotropic curvatures, it is not a question of different intensities on opposite sides, but rather that heliotropic effect depends on the direction of the light."[1]

Müller's research gave weight to this union of geo- and heliotropic effects by showing a number of resemblances in the manner and form of the two curvatures. Again, when it was found[2] that apheliotropic organs are influenced by light and darkness in precisely the same manner as positively heliotropic ones, it became clear that the mechanical explanation of De Candolle was untenable for negatively heliotropic organs, it might still no doubt be upheld for positively heliotropic organs, but, as a matter of fact, it was not so upheld. There was a tendency to unify our view of growth-curvatures, and the union of the two forms of heliotropism gave strength to the movement. Nor was this all; when it became clear that light did not produce heliotropic curvatures by direct mechanical effect, it was natural to remember that gravitation has none either; we cannot point to any reason (except the crudest ones) why the lower side of a horizontal stem, or the upper side of a horizontal root, should grow the faster for the direct effects of gravitation. That being so, light and gravitation could be classed together as external agencies acting, not directly, but in some unknown indirect manner. I do not imply that such a result followed immediately, but that the line of research above alluded to helped in some degree to lead the way to a belief in growth-curvatures as phenomena of irritability.

When my father was writing our book, "The Power of Movement in Plants" (1880), in which he adopted to the fullest extent a belief that growth-curvatures are phenomena of irritability, the only modern statement of such a view which he could find was in a passage by Sachs (*Arbeiten*, ii., 1879, p. 282), where he writes that "The living material of plants is internally differentiated in such a way that different parts are supplied with specific energies resembling those of the sensory-nerves (*Sinnesnerven*) of animals. Anisotropy in plants fulfils the same purpose as do sense-perceptions in animals."

The idea of irritability as applied to growth-curvatures is expressed with sufficient clearness in "The Power of Movement." Thus, for the case of geotropism we wrote (p. 521):—"Different parts or organs on the same plant, and the same part in different species, are thus excited to act in a widely different manner. We can see no reason why the attraction of gravity should directly modify the state of turgescence and subsequent growth of one part on the upper side, and of another part on the lower side. We are therefore led to infer that both geotropic, apogeotropic, and diageotropic movements, the purpose of which we can generally understand, have been acquired for the advantage of the plant by the modification of the ever-present movement of circumnutation. This, however, implies that gravitation produces some effect on the young tissues sufficient to serve as a guide to the plant." A similar view is given for heliotropism. It should be noted that the essence of the view—namely, that light and gravitation act as guides or landmarks by which the plant can direct itself—can be held without a belief in circumnutation.

In Pfeffer's admirable "Pflanzenphysiologie," 1881, the conception of stimulus and reaction is fully given, and is applied, among other cases, to that of heliotropism and geotropism. Pfeffer states clearly, and without reserve or obscurity, the view that light and gravitation act as stimuli or releasing forces, in manners decided by the organization of the plant. Pfeffer seems to me to be the first writer who has treated the subject fully and consistently.

In Sachs's "Vorlesungen" (1882), a view similar to that briefly sketched in his paper of 1879 is upheld. Geotropism and heliotropism are described as *Reizerscheinungen*, *i.e.* phenomena of stimulation. The phenomena in question are described under the heading Anisotropy, a word which expresses, according to Sachs (p. 855), "the fact that different organs of a plant under the influence of the same external forces assume the most varied directions of growth." In another passage (p. 859) he states that the anisotropy of the different organs "is nothing else than the expression of their different irritability to the influence of gravity [and] light, &c."

Vines ("Physiology of Plants"), who has recently (1886) summarized the evidence on growth-curvatures, and whose researches on kindred subjects entitle his opinion to respect, accepts fully the view that gravitation, light, &c., act as stimuli.

It is not necessary to trace the subject further, the views under discussion being now well-recognized canons of vegetable physiology.

I cannot, however, omit to mention Pfeffer's (*Tübing.n. Untersuchungen*, vol. i.) brilliant researches on the chemotaxis (irritability to certain reagents) of low organisms, such as antherozoids and bacteria. To take a single instance, Pfeffer showed that the antherozoids, in responding to the effect of malic acid, follow precisely the same law that in animals correlates the strength of stimulus and amount of effect. This result, although it has no direct connection with growth-curvatures, is nevertheless of the highest importance in connection with the general question of vegetable irritability.

Nor can I omit to mention the ingenious reasoning by which Noll (Sachs's *Arbeiten*, vol. ii. p. 466) localized the seat of irritability in a vegetable cell. He points out how in acellular plants, such as Caulerpa or Derbesia, the flowing protoplasm may travel from positively geotropic root to apogeotropic stem, and he argues from this that the motile endoplasm cannot be the seat of specific irritability. The flowing plasma, which is always changing its position with regard to external forces, must be as fully incapacitated from responding to them as though the plant were turning on a klinostat. It follows from this that it must be the stationary ectoplasm which perceives external change. From a different point of view, this is what we should expect—we should naturally suppose that the part which regulates the growth of the membrane, and therefore the curvature of the cell, should be the irritable constituent of the cell contents.

In attempting to trace the history of the establishment of growth-curvatures as phenomena of irritability, I have been forced to confine myself to a slight sketch. I have found it impossible to give a full account of the course of research on the subject. I have given an account of some of the halting-places in the journey of thought, but not to the manner in which belief has travelled from stage to stage. Far greater knowledge than mine would be required to compile such an itinerary.

Mechanism.

The first step in advance of Hofmeister's views was the establishment that the curvatures under consideration are due to unequal growth—that is to say, to an excess of longitudinal growth on the convex than the concave side. It is not, however, easy to say how far Hofmeister had this idea, for it, in fact, depends on how we define "growth." Hofmeister knew, of course, that the convex side of a curved shoot was longer than it had been before the curvature occurred; this is a mathematical necessity. But he also made out the important point that the concave side increases in length during the curvature. These permanent elongations he must have known to be growth, but his attention was directed to what is, after all, the more important point—namely, *why* it was that unequal elongation took place.

Sachs, in his "Experimental-Physiologie," held that growth-curvatures are due to unequal growth. In his "Text-book" (1874), English translation, 1882, p. 853, the author, referring to Hofmeister's work, says:—"I pointed out that the growth of the under surface of an organ capable of curving upwards was accelerated, and that of the upper surface retarded; I did not at the time express an opinion as to whether these modifications of growth were due to an altered distribution of plastic material or to a change in the extensibility of the passive layers of tissue." Frank's already-quoted paper made valuable contributions to the subject. He showed that the epidermic cells on the convex side of the root are longer than those on the concave side—that is, they have grown more; he explained apogeotropic curvatures in precisely the same way. He showed, moreover, that the sharp curve close to the tip of a geotropic root, and the long gradual curve of an apogeotropic shoot, are necessary consequences from the manner in which growth is distributed in these parts. He demonstrated that rectilinear growth and geotropic curvature require the same external conditions; that, for instance, a temperature low enough to check growth also puts a stop to geotropism.

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[1] In his "Vorlesungen," p. 854, Sachs states that he wrote Müller-Thurgau's introduction.

[2] Schmitz, *Linnæa*, 1843; Müller-Thurgau ("Flora," 1876); F. Darwin, Sachs's *Arbeiten*, 1880. The two latter researches were carried out under the direction of Sachs in his laboratory.

The distribution of longitudinal growth, which produces geotropism, was afterwards studied by Sachs (*Arbeiten*, i. p. 193, June 1871), who thoroughly established the fact that the convex side grows faster, while the concave side grows slower, than if the organ had remained vertical and uncurved.

These facts are of interest in themselves, but they do not, any more than Frank's results, touch the root of the matter. Until we know something of the mechanics of rectilinear growth, we cannot expect to understand curves produced by growth. The next advance in our knowledge did, in fact, accompany advancing knowledge of rectilinear growth. It began to be established, through Sachs's work, that turgescence is a necessary condition of growth. A turgescent cell is one which is, as it were, over-filled with cell sap; its cell-walls are stretched by the hydrostatic pressure existing within. In osmosis, which gives the force by which the cells are stretched, a force was at hand by which growth could be conceived to be caused. The first clear definition of turgor, and a statement of its importance for growth, occurs in Sachs's classical paper on growth (*Arbeiten*, p. 104, August 1871).

As soon as the importance of turgor in relation to growth was clearly put forward, it was natural that its equal importance with regard to growth-curvatures should come to the fore, and that increased growth on the convex side (leading to curvature) should be put down to increased internal cell-pressure in those tissues. In the fourth edition of Sachs's "Lehrbuch" (1874), Eng. trans., 1882, p. 834, such a view is tentatively given, but the author saw very clearly that much more evidence was needed before anything like a conclusion as to the mechanism of movement could be arrived at. The difficulty which faced him was not a new one—in a slightly different form it had occurred to Hofmeister—the question, namely, whether the curvatures of acellular and multicellular organs depend on the same or on different causes. If one explanation is applicable to both, then we must give up as a primary cause any changes in the osmotic force of the cells. For no change in the pressure inside a cell will produce a curvature in that cell, whereas, in a multicellular organ, if in the cells in one longitudinal half an increase of osmotic substances takes place, so that the cell-walls are subject to greater stretching force, curvature will take place.

On the other hand, if the cause of bending of acellular and multicellular organs is the same, we must believe that the curvature takes its origin in changes in the cell-walls. In an acellular organ, if the cell-membranes yield symmetrically to internal pressure, growth will be in a straight line; if it yields asymmetrically it will curve. Thus, if the membrane along one side of a cell becomes more or less resisting than the rest of the membrane, a curvature will result.

If we are to apply strictly the same principle to acellular and multicellular organs, we must suppose that the whole organ curves, because each individual cell behaves like one of the above-described free cells, the curvature of the whole resulting from the sum of the curves of the separate cells. This was Frank's view, and it also occurs in Sachs's "Text-book" (1874), Eng. trans., 1882, p. 842.

Are we bound to believe that the mechanism of acellular and multicellular curvatures is so strictly identical as Frank supposed? In the first place, it is not clear why there should be identity of mechanism in the movements of organs or plants of completely different types of structure. The upholders of the identity chiefly confine themselves to asseveration that a common explanation must apply to both cases. I believe that light may be thrown on the matter by considering turgescence, not in relation to growth, but in regard to stability of structure.

An acellular organ, such as the stalk of the sporangium of Mucor, owes its strength and stiffness to the tension between the cell contents and the elastic cell-wall, but it does not follow from this that in multicellular organs strength and stiffness are due to the sum of the strength of its individual cells. Indeed, we know that it is not so: the strength of a multicellular organ depends on the tension between pith and cortex. It is, in fact, a model of the single cell; the pith represents the cell-sap, the cortex the cell-wall. Here, then, it is clear that the function performed by the cell-wall in one case is carried out by cortical tissues in the other. If this is the case for one function, there is no reason why it should not hold good in another, viz. the machinery of movement.

If we hold this view that the cortex in one case is analogous with a simple membrane in the other, we shall not translate the unity of acellular and multicellular organs so strictly as did Frank. Indeed, we may fairly consider it harmonious with our knowledge in other departments to find similar functions performed by morphologically different parts. The cortex of a geotropic shoot would thus be analogous with the membrane of a geotropic cell in regard to movement, just as we know that these parts are analogous in regard to stability.

In spite of the difficulties sketched above, one writer of the first rank, namely, H. de Vries, has upheld the view that growth-curvatures in multicellular organs (*Bot. Zeitung*, 1879, p. 835), are due to increased cell-pressure on the convex side; the rise in hydrostatic pressure being put down to increase of osmotic substances in the cell-sap of the tissues in question. Such a theory flowed naturally from De Vries's interesting plasmolytic work (*ibid.* 1877, p. 1). He had shown that those sections of a turgescent shoot which were in most rapid growth show the greatest amount of shortening when turgescence is removed by plasmolysis. This was supposed to show that growth is proportional to the stretching or elongation of the cell-walls by turgor. Growth, according to this view, consists of two processes: (1) of a temporary elongation due to turgescence, and (2) of a fixing process by which the elongation is rendered permanent. De Vries assumed that where the elongation occurred, its amount must be proportional to the osmotic activity of the cell contents; thus neglecting the other factor in the problem—namely, the variability in the resistance of the membranes. He applied the plasmolytic method to growth-curvatures, and made the same deductions. He found that a curved organ shows a flatter curve[1] after being plasmolyzed. This, according to his previous argument, shows that the cell-sap on the convex is more powerfully osmotic than that on the concave side. This again leads to increased cell-stretching, and finally to increased growth.

The most serious objection to De Vries's views is that the convex half of a curving organ does *not* contain a greater amount of osmotically active substance.[2] It must, however, be noted in the heliotropic and geotropic curvature of pulvini, there is an osmotic difference between the two halves[3]—so that, if the argument from uniformity is used against De Vries (in the matter of acellular and multicellular organs), it may fairly be used in his favour as regards the comparison of curvatures produced with and without pulvini.

It is not easy to determine the extent to which De Vries's views on the mechanics of growth-curvature were accepted. The point, however, is of no great importance, for the current of conviction soon began to run in an opposite direction.[4]

Sachs ("Lehrbuch," ed. 4, Eng. trans. p. 835) had already pointed out that attention should be directed to changes in extensibility of cell-walls as an important factor in the problem.

Wiesner, in his "Heliotropische Erscheinungen" (*Wiener Sitzungsb.*, vol. lxxxi., 1880, p. 7; also in the *Denkschriften*, 1882), held that the curvature of multicellular organs is due both to an increase of osmotic force on the convex side, and to increased ductility[5] of the membranes of the same part. He repeated De Vries's plasmolytic experiments, and made out the curious fact that in many cases the curvature is increased instead of being diminished. He attributed the result to the concave tissues being more perfectly elastic than ductile convex tissues, so that when turgescence is removed, the more elastic tissues shorten most, and, by diminishing the length of the concave side, increase the curvature.

Strasburger, in his "Zellhäute" (1882), suggested that growth-curvatures are due to increased ductility of the convex membranes, and gave a number of instances to prove that a change to a ductile condition does occur in other physiological processes, such as the stretching of the cellulose ring in Œdogonium to a uniform thin membrane, the branching of Cladophora, and the escape of sexual products in certain Algæ.

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[1] Frank made similar experiments, but failed to find any diminution of curvature.

[2] Kraus, *Abhand. Nat. Gesell zu Halle*, xv., 1882. See also a different proof by Wortmann, *Deutsch Bot. Gesell.*, 1887, p. 459.

[3] Hilburg in Pfeffer's *Tübingen. Untersuch.*, vol. i., 1881, p. 31.

[4] An opportunity will occur later on for referring to some details of De Vries's work not yet noticed.

[5] Weinzierl, *Sitzungsb. Wien.*, 1877, showed that strips of epidermis taken off the convex side of heliotropically curved flower-stalks of tulip and hyacinth were about twice as extensible when stretched by a small weight, 7·5 grammes, as approximately corresponding strips for the concave side.

We now pass on to the work of two observers, Wortmann and Noll, who have devoted special attention to mechanism of curvatures. Wortmann (*Bot. Zeit.*, 1887, p. 785) started on the assumption, already several times mentioned, that the growth-curvature of acellular and multicellular organs must have a common cause. He began by testing Kohl's statement (*Bot. Hefte*, Marburg, Heft v. [I have not seen Kohl's paper]) that when the sporangiferous hypha of a Phycomyces curves apogeotropically or heliotropically, &c., there is a collection of protoplasm on the concave wall. Wortmann principally investigated the curvature discovered in Phycomyces by Errera (*Bot. Zeitung*, 1884) which can be produced by contact. When the hypha is touched with a glass filament or with a platinum wire, or by allowing a speck of indian ink to dry on it, it curves over towards the touched side. The hypha is so highly sensitive to contact that it curves in from three to six minutes; it is clearly a growth-curvature, for it only occurs in the part of the hypha which is growing. In curvatures thus produced, as well as in apogeotropic and heliotropic curvatures, the accumulation of protoplasm on the concave side is, according to Wortmann, clearly visible, and, what is more important, the membrane becomes thicker on the concave side, sometimes twice as thick as on the opposite side of the cell. In consequence of the unequal thickening of the membranes, the cell is supposed to yield asymmetrically cell-pressure, and the necessary consequence is that the cell grows into a curved form.

In applying the same method of investigation to multicellular parts, Wortmann followed Ciesielski (Cohn's "Beiträge," 1872, p. 1), who noticed that in geotropically curved roots the cells of the concave (lower) side of the organ are much more densely filled with protoplasm than are the convex cells. Sachs ("Vorlesungen," p. 842) describes a similar state of things in the halms of grasses, and Kohl, again, in tendrils and the stems of climbing plants.

Wortmann first of all made sure that no redistribution of protoplasm could be observed in the *individual* cells of curving multicellular organs. If each cell behaved independently like a free cell, we might expect to find a collection of protoplasm on the concave wall of all the constituent cells of a curving shoot. But this is not the case. Nor at first could any microscopic differences be made out between the concave and convex tissues of a curving shoot. But when the stimulus was made to act for a long time, differences were apparent. A young Phaseolus plant was placed so that the epicotyl was horizontal and was forced to grow in the horizontal direction by a thread attached to the end of the stem, passing over a pulley and fastened to a weight. Here the geotropic stimulus could continue to act for 24–36 hours, and under such conditions a marked change in the tissues was visible. The cells of the cortex on the upper side became densely filled with protoplasm, while the lower cortical cells were relatively poor in protoplasmic contents. The same changes in the membranes occur as those noticed in Phycomyces—that is to say, the walls of the cortex on the upper side are very much thicker than those on the lower side.[1]

Since the walls of the cortical cells have become more resisting on the upper than on the lower side, then (assuming the osmotic expanding force to be the same in both cases) the growth will be quicker on the lower side, and the shoot will curve upwards. Wortmann states that his observations account for the fact that the convex side grows quicker, not merely than the concave, but than a normal unbent shoot. But he does not seem to have compared the thickness of the convex cell-walls with the normal, although he states that they are poorer in protoplasm than is usual, and from this it may, according to his views, be perhaps assumed that the membranes are abnormally thin.

Wortmann points out that his views account for two well-known features in growth-curvatures, viz. the *latent period* and the *after-effect*. If a curvature can only occur when a difference in structure of cell-walls has arisen, it is certainly natural that some time should occur before the curvature is apparent. I do not lay much stress on this part of the subject, as I feel sure the whole question of latent period needs further investigation. With regard to after-effect it is true that Wortmann's views account for the continuance of curvature after the stimulus has ceased to act.

Wortmann attaches great importance to another point in his theory, which, could it be established, would be of the greatest interest, and would unite under a common point of view, not only acellular and multicellular organs, but also naked protoplasm, *e.g.* the plasmodia of myxomycetes. The view in question was tentatively suggested by Sachs ("Lehrbuch," 1874; Eng. trans., 1882, p. 841), and mentioned by Pfeffer ("Pflanzenphysiologie," ii. p. 331) in a similar spirit. The apogeotropic curvature of a Phycomyces-hypha is supposed to be due to the unequal thickening of the membrane on the upper and lower sides, and this to be due to the migration of protoplasm from the lower to the upper side of the cell. In the same way in a multicellular organ the protoplasm is supposed to migrate from the lower cortex and pith to the upper cortex and pith, such migration being rendered possible by the now generally admitted intercellular protoplasmic communication. Thus the apogeotropism of a cell or a multicellular part would be due to the apogeotropism or tendency to migrate vertically upwards of the protoplasm. There are great difficulties in the way of accepting this attractive theory.

Noll (Sachs's *Arbeiten*, 1888, p. 530) states that when a curved Phycomyces-hypha, in which protoplasm has accumulated in the upper (concave) side, is reversed so that the mass of protoplasm is below, it does not migrate upward again, as might be expected. Moreover, he points out that in Nitella and in Bryopsis the circulating protoplasm continues in movement, and does not accumulate in any part of the cell. Lastly, there seems, as Noll points out, a difficulty in believing in the migration of protoplasm through the very minute pores by which the plasma strands pass from cell to cell. There seems much probability in Noll's view that the plasmic strands only serve for the passage of impulses, or molecular changes, and that they consist of ectoplasm alone, not of the endoplasm which Wortmann describes as the migratory constituent of the cell.

Wortmann's theory has been criticized by Elfving (*Finska Vet. Soc. Förhand.*, Helsingfors, Bd. xxx., 1888). The essence of Elfving's paper is that appearances similar to those described by Wortmann can be produced by curvatures not due to stimulation. Thus, when Phycomyces is made to grow against a glass plate it is mechanically forced to bend. Yet here, where there is no question of stimulation, the plasma collects along the concave side of the cell. Elfving concludes that the visible changes are the result and not the cause of the curvature. Elfving also produced curvature in Phaseolus by bending the apex of the plant towards its base and tying in that position. Under these conditions the convex side of the shoot showed the changes described by Wortmann in geotropic plants. Here again Elfving gives reason to believe that the thickening of the cell-walls is a result, not of curvature, but of strain mechanically produced. When a plant is prevented from executing an apogeotropic movement it is clear that a longitudinal strain is put on the upper (concave) side. But the longitudinal strain in Elfving's plants is on the convex side. Therefore, if, as Elfving believes, the visible changes are due to strain, they should, as they do, occur on the convex side in his experiments, on the concave in Wortmann's.

Wortmann replied in the *Bot. Zeitung*, 1888, p. 469, and attempted to explain how Elfving's results might be explained and yet his own theory hold good. The reply is by no means so strong as the criticism, and it must be allowed that Elfving has seriously shaken Wortmann's argument.

Somewhat similar criticisms have been made by Noll (Sachs's *Arbeiten*, 1888, p. 496). In the acellular plants, Derbesia and Bryopsis, Noll studied growth-curvatures, and was quite unable to detect any thickening of the concave cell-walls, except when the curvatures were very sudden, and in these cases the result could equally well be produced by mechanical bending.

Noll further points out what is undoubtedly a fault in Wortmann's theory—namely, that he explains the retardation on the concave rather than acceleration on the convex side. This criticism is only partially just, for though Wortmann's description only shows a *relative* thinness of the walls on the convex side, yet it is clear he believed there to be an absolute diminution of resisting power on that side.

Noll's experiments with grass halms show clearly that acceleration of growth on the convex side is the primary change, rather than retardation along the concave half. When the halms are fixed in horizontal glass tubes, so that they are stimulated but unable to bend, the lower half of the pulvinus forms an irregular out-growth, increasing radially since it is not able to increase longitudinally.

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[1] Both protoplasmic change and thickening of cell-walls occur to some extent in the pith.

A similar argument may be drawn from Elfving's experiments. He found that the pulvini of grass halms placed on the klinostat increase in length. This experiment shows incidentally that the klinostat does not remove but merely distribute equally the geotropic stimulus: also that geotropic stimulus leads to increased, not to diminished growth. The same thing is proved by the simple fact that a grass halm shows no growth in its pulvinus while it is vertical, so that when curvature begins (on its being placed horizontal) it must be due to acceleration on the convex, since there is no growth on the concave side in which retardation could occur. Noll's view is that the primary change is an increase in extensibility of the tissues on the convex side. This view he proceeded to test experimentally. A growing shoot was fixed in a vertical position, and a certain bending force was applied to make it curve out of the vertical, first to the right and then to the left. If the cortical tissues are, at the beginning of the experiment, equally resisting all round, it is clear that the excursions from the vertical to the right and left will be equal. As a matter of fact the excursions to the right and left were nearly the same, and the difference was applied as a correction to the subsequent result. The shoot was then placed horizontally until geotropic or other curvature was just beginning, when the above bending experiment was repeated. It was then found that when it was bent so that the lower side was made convex, the excursion was greater than it had been. In the few experiments given by Noll the excursion in the opposite direction (stretching of the concave side) was less than it had been, and he states that all the other experiments showed a similar result The increased extensibility of the convex side is clearly the most striking part of the phenomenon, but I fail to see why Noll takes so little notice of the diminution in the extensibility of the concave side, which is only mentioned towards the end of his paper (*loc. cit.* p. 529). Yet such a diminution is a necessary factor in the mechanism of curvature. It should be noted that results like Noll's might be obtained under other conditions of growth-curvatures. Thus if De Vries's view were the true one, and the curvature were due to difference in osmotic force on the convex and concave sides, the shoot would react differently in the two directions; for instance, the concave side would be the more easily compressed. Noll and Wortmann's explanations differ in this: the former lays the greater stress on the increased extensibility of the convex side, the latter on the diminution of that of the concave side. Again, Wortmann explains the difference in extensibility as due to differences in thickness of the cell-walls. Noll gives no mechanical explanation, but assumes that the ectoplasm has the power of producing changes in the quality of the cell-wall in some unknown way.

In the early stages of curvature, a phenomenon takes place to which Noll attaches great importance as supporting his view. When a curved organ is plasmolyzed, it suffers a diminution of curvature, as De Vries showed, but Noll[1] has proved that in the early stages of curvature a contrary movement occurs—that is to say, the curvature is increased. This seems to show that the yielding of the convex side is owing to a ductility, which prevents its holding its own against the more perfect elasticity of the concave side. But this is only the beginning of the phenomenon; as the plasmolyzing agent continues to act, a reverse movement takes place, the well-known flattening of the curvature described by De Vries. It is to me incomprehensible how in a given condition of cell-walls these results can occur in different stages of plasmolysis. I can understand one occurring when the curvature is recent, and the other, the flattening of the curve, occurring when the ductile convex parts have reacquired elasticity. The fact undoubtedly is as Noll describes it: his explanation seems to me inadequate.

We have now seen that the most acceptable theory of the machinery of these curvatures is in its main features akin to Hofmeister's, the power of elongation supplying the motive force, while the varying extensibility of the membranes determines the nature and direction of the bend.

The question now arises: Is it possible by these means to account for all the facts that must be explained? Taking the theory for which there is most to be said on experimental grounds—viz. Noll's—it will be noted that it is essentially connected with the doctrine of growth by apposition. The question, therefore, whether the apposition-theory is sufficient to account for the phenomena of ordinary growth, may be applied *mutatis mutandis* to growth-curvature. This doctrine in its original purity absolutely requires turgescence to account for the elongation of growth. The older layers, separated from the ectoplasm by the younger layers of cell-wall, can *only* be elongated by traction. Growth by intussusception does not absolutely require this force; the theory that the micellæ are separated by traction, and thus allow intercalation of fresh micellæ, is a view for which Sachs is chiefly responsible.

Since surface-growth by apposition is absolutely dependent on the traction exercised by cell-pressure, it is a fair question—how far growth is influenced by forcible elongation. Baranetzky (*Mém. Acad. St. Pét.*, v. vol. xxvii. p. 20) states that when a plant is subject to traction, as by even a small weight attached to the free end, the rate of growth is lowered. Ambronn (Pringsheim's *Jahrb.*, xii.), as Zimmermann points out in the same connection, found no increased elongation of collenchyma when stretched for some days by means of a weight. A greater difficulty is that growth may be absolutely and at once stopped by placing the growing organ in an atmosphere free from oxygen (Wieler, Pfeffer's *Untersuch.*, Bd. i. p. 189). Such treatment apparently does not diminish turgescence, yet growth stops. If the cell-walls are increasing in length by mechanical stretching, and if the turgor is not interfered with, increase in length ought to continue. The same thing applies to curvatures. Wortmann has shown (*Bot. Zeit.*, 1884, p. 705) that in an atmosphere of pure hydrogen a geotropic curvature which has begun in ordinary air cannot continue; in other words, after-effect ceases. This seems to me inexplicable on Noll's or Wortmann's theories; the convex side has become more extensible than the concave, turgescence, as far as we know, continues, yet no after-effect is observed. The same result may be gathered from Askenasy's[1] interesting experiments on the growth of roots. He showed that lowering the temperature has an almost instantaneous inhibitive effect on growth. Thus maize roots (at a temperature of 26°·6) growing at the rate of 33 divisions of the micrometer per hour, were placed in water at 5°, and absolutely no growth occurred during the following ten minutes, in which the thermometer rose to 6°·5. This result is all the more valuable because we know from Askenasy's[2] other results that the turgor, as estimated by plasmolytic shortening, is about the same whether the root is in full growth or not growing at all. This is not conclusive, for if the growing cell-walls were ductile they might shorten but little although under great pressure, whereas the non-growing cells might shorten a good deal, owing to their more perfect elasticity;[3] therefore Askenasy's plasmolytic results are not in this particular connection of great importance, except as showing that the non-growing roots were certainly to some extent turgescent.

There are other facts which make it extremely difficult to understand how surface-growth can depend on cell-pressure. Nägeli ("Stärkekörner," p. 279) pointed out that the growth of cylindrical cells which elongate enormously without bulging outwards laterally, is not explicable by simple internal pressure. An internodal cell of Nitella increases to 2000 times its original length, while it only becomes ten times as wide as it was at first. The filaments of Spirogyra become very long, and keep their original width. Nägeli found that in Spirogyra the shortening produced by plasmolysis was practically the same in the longitudinal and in the transverse direction. He therefore concluded that the growth of Spirogyra cannot be accounted for by the cell-wall being differently extensible along different axes. But it must once more be pointed out that this type of plasmolytic experiment has not the force which Nägeli ascribes to it. If the cell-wall stretched like putty in one direction and like india-rubber in the other, there might be no plasmolytic shortening in the line of greatest growth. Nevertheless, in spite of this flaw in Nägeli's argument, great elongation in a single direction remains a problem for those who believe in surface-growth by apposition.

The point of special interest is that differences in extensibility in different directions cannot be supposed to exist in a homogeneous membrane. If *any* purely physical characters can explain the facts, they must be architectural characters. That is to say, we must be able to appeal to remarkable structural differences along different axes if we are to explain the facts.

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[1] The similar results obtained by Wiesner are noticed above.

[1] *Deutsch. Bot. Ges.*, 1890, p. 61. This paper contains an excellent discussion on the mechanics of growth, to which I am much indebted.

[2] *Loc. cit.* p. 71.

[3] Wiesner (*Sitz. Wien. Akad.*, 1884, vol. lxxxix.-xc., Abth. i. p. 223) showed that under certain conditions decapitated roots grow much more quickly than normal ones, yet the amount of plasmolytic shortening is less. Decapitated: growth 79 per cent.; plasmolytic shortening, 8 per cent.; normal: growth, 39 per cent.; shortening, 13 per cent.

Such structural differences do, of course, exist, but whether they are sufficient to account for the phenomena is a different question. Strasburger ("Zellhäute," p. 194) supposes that the elasticity of a cell-wall depends on the last-formed layers, and as in these the microsomes are seen arranging themselves in lines or patterns, we have a heterogeneity of structure which may or may not be sufficient.

We have now seen that it is difficult to believe, although it is not inconceivable, that the extending force of cell-turgor, combined with differences in extensibility of the membranes (depending on structural characters), may account for the phenomena of rectilinear growth. But, even if we allow that this is so, how are we to apply the same explanation to growth-curvatures? How are we to account for the rapid changes in extensibility necessary to produce geotropic or heliotropic curvatures? The influences which Strasburger and Noll suppose to act on the cell-walls and render them ductile cannot account for extensibility in one direction only. Nor does Wortmann's theory, that difference in extensibility depends on difference in thickness, meet the case completely. What we need is an increase in longitudinal, not in general extensibility. I presume that these writers might say that the excess in longitudinal extensibility is always present whether general extensibility is greater or less. In the meanwhile we must pass on to more recent researches on surface-growth by apposition.

In Strasburger's later work ("Histologische Beiträge," 1889), his views on growth have undergone considerable modification. The study of certain epidermic cells, of the folds in membranes, and the repetition of Krabbe's work on certain bast fibres, have convinced him that apposition does not account for all forms of growth. Krabbe (Pringsheim's *Jahrb.*, xviii.) showed that in full-grown sclerenchyma (*e.g.* in Oleander) local widenings occur without any such amount of thinning in the membrane as would occur if the bulging were due to stretching. The only possible explanation seems to be that there is a migration of new material into the cell-wall. Such intussusception might be, as Nägeli supposed, a flow of fluid out of which new micellæ crystallize; but it is now established that cellulose arises as a modification of protoplasm, so that it would harmonize with our knowledge of the origin of cellulose if we assume that intussusception was preceded by a wandering of protoplasm into the cell-wall. Such a state of things would render possible the regulation of longitudinal growth in the case of Nitella and Spirogyra, already alluded to, as well as in growth-curvatures. This view might also harmonize with Wiesner's theory (*Sitz. Wien. Akad.*, 1886, vol. xciii. p. 17) that the cell-wall contains protoplasm as long as it continues to grow.

For the sake of brevity I content myself with the above examples: I think it will be allowed that there is a focussing of speculation from many sides in favour of "active" surface-growth—or, what is perhaps a better way of putting it, in favour of a belief that the extension of cell membranes depends on physiological rather than physical properties, that it is in some way under the immediate control of the protoplasm. We may take our choice between Wiesner's wall-protoplasm (dermatoplasm), protoplasmic intussusception as conceived by Strasburger, or the action of the ectoplasm in the manner suggested by Vines,[1] who supposes that the crucial point is a change in the motility of the protoplasm, not of the cell membrane. The latter theory would undoubtedly meet the difficulties—if we could believe that so yielding a substance as protoplasm could resist the force of turgor.

The great difficulty is, as it seems to me, that since, *e.g.* in Caulerpa, surface-growth is clearly due to stretching, as Noll has demonstrated, and since in osmotic cell-pressure a stretching force does exist, it cannot be doubted that turgor, and ordinary physical extensibility are conditions of the problem. This remains true in spite of Klebs's (*Tübingen. Untersuchungen*, ii. p. 489) curious observations on the growth of plasmolyzed Algæ, or in spite of the fact that pollen tubes may grow without turgor, in spite of the same being perhaps true of young cells filled with protoplasm (see Noll, *Würzburg. Arbeiten*, iii. p. 530). In the face of all these facts, osmotic pressure in the cell must remain a *vera causa* tending to surface-growth.

If we accept some form of "active" surface-growth, we must deal with turgor in another way, although to do so may require a violent exercise of the imagination. Are we to believe, for instance, that the function of turgescence is the attaining of mechanical strength? If we hold that cell-walls increase in area independently of turgor, we shall be forced to invent a hypothesis such as the following—which I am far from intending to uphold. It is possible to imagine that the function of the force of turgor is merely to spread out the growing membrane to its full extent, and, as it were, to make the most of it. Turgor would in this respect play the part occupied by the frame used in embroidery, making it easier to carry on the work satisfactorily, but not being absolutely necessary. When mechanical strength is gained by turgor (as in Mucor), instead of by brute strength of material, as in a tree-trunk, a great economy in cellulose is effected. If turgor played our hypothetical part of smoothing out the membrane and insuring that it shall occupy as large a space as possible, it would effect the same kind of economy.

It is not necessary to inquire how far this hypothesis accords with our knowledge of cell mechanics. It is only put forth as an example of the difficulties in which we land if we seek for a new function for turgor. We are, indeed, surrounded by difficulties; for, though the theories which are classed together as protoplasmic have much in their favour, they, too, lead us into an *impasse.*

Circumnutation.

I shall conclude by saying a few words about the theory o growth-curvatures put forward in the "Power of Movement in Plants." I can here do no more than discuss the relation of circumnutation to curvature, which is the thesis of the book in question, without attempting to enter the arena with regard to the many objections which have been raised to other parts of our work.

A distinguished botanist, Prof. Wiesner, of Vienna, published in 1881 a book, "Das Bewegungsvermögen der Pflanzen," entirely devoted to a criticism of the "Power of Movement" (p. 8). It is founded on a long series of experiments, and is written throughout in a spirit of fairness and candour which gives it value, apart from its scientific excellence, as a model of scientific criticism. The words written on the title-page of the copy presented to my father are characteristic of the tone of the book: "In getreuer Opposition, aber in unwandelbarer Verehrung." A letter printed among my father's correspondence shows how warmly he appreciated his opponent's attack both as to matter and manner. Wiesner's opposition is far-reaching, and includes the chief theoretical conclusion of the book—namely, that movements such as heliotropism and geotropism are modifications of circumnutation. Neither will he allow that this revolving nutation is the widely-spread phenomenon we held it to be. According to Wiesner, many parts of plants which do not circumnutate are capable of curving geotropically, &c.; he is, therefore, perfectly justified, from his own point of view, in refusing to believe that such curvatures are derivations from circumnutation. He points out that our method of observing circumnutation is inaccurate, inasmuch as the movement is recorded in oblique projection. This we were aware of, and I cannot but think that Wiesner has unintentionally exaggerated its inaccuracy; and that, if used with reasonable discretion, it cannot lead to anything like such faulty records as in the supposititious cases given by our critic. However this may be, Wiesner's results are perhaps more trustworthy than ours, and should receive the most careful consideration.

Wiesner's conclusions, taken from his own summaries, are as follows:—

The movement described as circumnutation is not a wide-spread phenomenon in plants. Stems, leaves, and acellular fungi are to be found which grow in a perfectly straight line. Some roots grow for considerable periods of time without deviating from the vertical. When circumnutation does occur, it cannot be considered to have the significance given to it in the "Power of Movement." The movements observed by Wiesner are explained by him in three different ways:—

i. As the expression of a certain irregularity in growth depending on the want of absolute symmetry in structure, and on the fact that the component cells of the organ have not absolutely similar powers of growth.

ii. As the expression of opposing growth-tendencies. Thus certain organs have inherent tendencies to curve in definite planes—for instance, the bending of the hypocotyl in the plane of the cotyledons. Wiesner believes that such tendencies, when combined with others—heliotropic, geotropic, &c.—lead to alternate bendings in opposite directions, according as one or other of the components is temporarily the stronger.

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[1] Sachs's *Arbeiten*, 1878, and "Physiology," 1886. See also Gardiner, on protoplasmic contractility, in the *Annals of Botany*, i. p. 366. Pfeffer has, I think, shown that Vines's and Gardiner's theories assume the existence of too great strength in the ectoplasm. See Pfeffer in *Abhandl. der k. Sächs. Gesellsch.* xvi. 1890, p. 329.

iii. Wiesner allows that circumnutation does exist in some cases. This last class he considers a small one; he states, indeed, that "nearly all, especially the clearly perceptible circumnutations," are combined movements belonging to the second of the above categories.

Although I have perhaps no right to such an opinion without repeating Wiesner's work, yet I must confess that I cannot give up the belief that circumnutation is a widely-spread phenomenon, even though it may not be so general as we supposed.

If, then, circumnutation is of any importance, we are forced to ask what is its relation to growth-curvatures. It was considered by my father to be "the basis or groundwork for the acquirement, according to the requirements of the plant, of the most diversified movements" ("Power of Movement," p. 3). He also wrote (*loc. cit.*, p. 4):—"A considerable difficulty in the way of evolution is in part removed, for it might be asked how did all these diversified movements . . . . first arise? As the case stands, we know that there is always movement in progress, and its amplitude, direction, or both, have only to be modified for the good of the plant in relation to internal or external stimuli."

Those who have no belief in the importance of circumnutation, and who hold that movements may have arisen without any such basis, may doubtless be justified in their position. I quite agree that movement *might* be developed without circumnutation having anything to do with the matter. But in seeking the origin of growth-curvatures it is surely rational to look for a widely-spread movement existing in varying degrees. This, as I believe, we have in circumnutation: and here comes in what seems to me to be characteristic of the evolution of a quality such as movement. In the evolution of structure, each individual represents merely a single one of the units on which selection acts. But an individual which executes a number of movements (which may be purposeless) supplies in itself the material out of which various adapted movements may arise. I do not wish to imply that tentative movements are of the same order of importance as variations, but they are undoubtedly of importance as indication of variability.

The problem may be taken back a stage further; we may ask why circumnutation should exist. In the "Power of Movement" (p. 546) we wrote:—"Why every part of a plant whilst it is growing, and in some cases after growth has ceased, should have its cells rendered more turgescent and its cell-walls more extensile first on one side then on another . . . is not known. It would appear as if the changes in the cells required periods of rest." Such periods of comparative rest are fairly harmonious with any theory of growth; it is quite conceivable by intussusceptionists and appositionists alike that the two stages of elongation and fixation should go on alternately,[1] but this would not necessarily lead to circumnutation. It might simply result in a confused struggle of cells, in some of which extension, in others elongation, was in the ascendant; but such a plan would be an awkward arrangement, since each cell would hinder or be hindered by its neighbour. Perfection of growth could only be attained when groups of contiguous cells agreed to work together in gangs—that is, to pass through similar stages of growth synchronously. Then, if the different gangs were in harmony, each cell would have fair play, elongation would proceed equally all round, and the result would be circumnutation.[2] Whether or no any such origin of circumnutation as is here sketched may be conceived, there can be no doubt that it had its origin in the laws of growth apart from its possible utilization as a basis for growth-curvature.

It is, however, possible to look at it from a somewhat different point of view—namely, in connection with what Vöchting has called *rectipetality* ("Die Bewegung der Blüthen und Früchte," 1882). He made out the fact that when an organ has been allowed to curve geotropically, heliotropically, &c., and is then removed from further stimulation by being placed on the klinostat, it becomes straight again. This fact suggested to Vöchting his conception of rectipetality, a regulating power leading to growth in a straight line. It may be objected that such a power is nothing more than the heredity, which moulds the embryo into the likeness of its parent, and by a similar power insists that the shoot or root shall take on the straight form necessary to its specific character. But the two cases are not identical. The essence of rectipetality is the power of recovering from disturbance caused by external circumstances. When an organ has been growing more quickly on one side than another, the regulating power reverses this state of things and brings the curving organ back towards the starting-point. We have no means of knowing how this regulating power acts in undisturbed growth. It is possible to imagine a type of irritability which would insure growth being absolutely straight, but it is far more easy to conceive growth as normally made up of slight departures from a straight line, constantly corrected. In drawing a line with a pencil, or in walking towards a given point, we execute an approximately straight line by a series of corrections. If we may judge in such a manner by our own experience, it is far more conceivable that the plant should perceive the fact that it is not growing absolutely straight, and correct itself, than that it should have a mysterious power of growing as if its free end were guided by an external force along a straight-edge. The essence of the matter is this: we know from experiments that a power exists of correcting excessive unilateral growth artificially produced; is it not probable that normal growth is similarly kept in an approximately straight line by a series of aberrations and corrections? If this is so, circumnutation and rectipetality would be different aspects of the same thing.

This would have one interesting corollary: if we fix our attention on the regulating power instead of on the visible departures from the straight line, it is clear that we can imagine an irritability to internal growth-changes existing in varying intensities. With great irritability very small departures from the straight line would be corrected. With a lower irritability the aberrations would be greater before they are corrected. In one case the visible movement of circumnutation would be very small, in the other case large, but the two processes would be the same. The small irregular lateral curvatures which Wiesner allows to exist would therefore be practically of the same value as regular circumnutation, which he considers comparatively rare.

The relation between rectipetality and circumnutation may be exemplified by an illustration which I have sometimes made use of in lecturing on this point. A skilful bicycle-rider runs very straight, the deviations from the desired course are comparatively small; whereas a beginner "wobbles" or deviates much. But the deviations are of the same nature; both are symptoms of the regulating power of the rider.

We may carry the analogy one step further: just as growth-curvature is the continuance or exaggeration of a nutation in a definite direction, so when the rider curves in his course he does so by wilful exaggeration of a "wobble."

It may be said that circumnutation is here reduced to the rank of an accidental deviation from the right line. But this does not seem necessarily the case. A bicycle cannot be ridden at all unless it can "wobble," as every rider knows who has allowed his wheel to run into a frozen rut. In the same way it is possible that some degree of circumnutation is correlated with growth in the manner suggested above, owing to the need of regular pauses in growth. Rectipetality would thus be a power by which irregularities, inherent in growth, are reduced to order and made subservient to rectilinear growth. Circumnutation would be the outward and visible sign of the process.

I feel that some apology is due from me to my hearers for the introduction of so much speculative matter. It may, however, have one good result, for it shows how difficult is the problem of growth-curvature, and how much room there still is for work in this field of research.

NOTES.

THE German Leopold-Caroline Academy at Halle has conferred the degree of Doctor of Philosophy on the Director of the Royal Gardens, Kew.

MESSRS. MACMILLAN AND CO. hope to publish before Christmas a series of popular sketches in the history of astronomy from the earliest times to the present day, in the form of a volume containing three courses of lectures on astronomical biography by Prof. Oliver Lodge, F.R.S. The work will be fully illustrated, and will bear the title "Pioneers of Science."

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[1] Strasburger, "Histolog Beiträge," p. 195, speaks of the pause that must occur after the formation of a cellulose lamella. Hofmeister, *Württemburg. Jahreshefte*, 1874, describes the growth in length of Spirogyra as made up of short intervals of rapid growth alternating with long pauses of slow growth.

[2] I purposely omit the circumnutation of pulvini.

At the monthly meeting of the Field Naturalists' Club of Victoria, held on July 13 last, as we learn from the Melbourne *Argus* of July 14, Messrs. Luehman and French read a note and exhibited the skin of a tree-climbing kangaroo from Northern Queensland, new to science, to which they gave the name of *Dendrolagus muelleri*. This remarkable marsupial has a body about two feet in length, with a tail somewhat exceeding two feet. The disproportion between the fore legs and the hind legs is not nearly so great as that of the ordinary kangaroo and wallaby; the toes are strong and curved, to enable it to climb tall and straight trees, on the leaves of which it exists. This tree-kangaroo is more nearly allied to the species which was discovered a few years ago in Queensland than to the two species from New Guinea. The specimen described was got from a straight tree, about ninety feet above the ground.

In his letter on "Dredging Products" (Nature, August 13, p. 344), Mr. Alex. Meek, writing from Shetland, gave a short *résumé* of localities where Actinotrocha has been found. As the south coast of England was not mentioned, Mr. W. L. Calderwood writes to call attention to a paper by his predecessor at the M.B.A. Laboratory, Plymouth, Mr. G. C. Bourne, published in the Journal of the Marine Biological Association, vol. i., No. 1. After mentioning the occurrence of Tornaria, Mr. Bourne goes on to say:—"Actinotrocha, the larva of Phoronis, is common. . . . Several specimens of larval Amphioxus were taken in the tow-net towards the end of October." In vol. ii. No. 1, Mr. Garstang also has a note on the occurrence of the adult Phoronis. Actinotrocha has again appeared several times during the present summer.

M. Imfeld, the Swiss engineer, who has been engaged to examine the nature of the summit of Mont Blanc for the construction there of M. Janssen's proposed Observatory, recounts in a Zürich journal the difficulties he is experiencing in his preliminary survey. M. Imfeld is staying with eight workmen and two doctors at M. Vallot's Observatory, which has an altitude of 4400 metres, and thence they proceed daily to the summit, where they work for several hours a day in the endeavour to ascertain the depth of the snow for the purpose of getting the necessary foundation for the building. M. Eiffel has expressed the opinion that the construction of an Observatory will only be possible if the snow does not exceed a depth of 12 metres. M. Imfeld states that they have encountered traces of a ridge of rock 18 to 20 metres below the summit, and covered with about 1 metre of snow. They have therefore commenced to make a series of lateral tunnels on three sides, at a distance equal to 12 metres below the summit, to ascertain if the ridge extends to that height. Progress is necessarily slow. Most of the men are suffering from *mal de montagne*. Some, however, who are engaged at M. Vallot's cabin are able to work almost as long as in the valley, and they also eat and sleep well. In spite of two coke stoves, the thermometer of the cabin never rises above zero; even ink freezes, and water boils at 83°, and they cannot properly cook meat. For a day or two they were disturbed by violent storms.

Martinique has been visited by a terrible cyclone, the most violent that has been known in the island since 1817. It lasted four hours, and was followed by an earthquake; and many lives were lost. According to the latest information received in Paris from Martinique on Monday last, the number of persons known to have perished was 340; but that did not include the sailors lost in numerous shipwrecks along the coast and at sea. Besides the persons killed, very many were injured by the falling buildings, trees, and stones. All along the coast houses were completely demolished. The town of Morne Rouge is said to be a total wreck, and Fort de France is almost entirely destroyed. Much suffering prevails among the population.

Messrs. L. Reeve and Co. have in preparation a new work on the British Fungi, Phycomycetes, and Ustilagineæ, by George Massee, Lecturer on Botany for the London Society for the Extension of University Teaching; a work on the British Hemiptera Heteroptera, by Edward Saunders; a new work on the Lepidoptera of the British Islands, by Charles G. Barrett; and a new work on the physiology of the Invertebrata, by Dr. A. B. Griffiths.

Messrs. Whittaker and Co. are about to publish "A First Book of Electricity and Magnetism," by W. Perren Maycock. The work is intended for the use of elementary science and art and engineering students, and general readers.

Messrs. Cassell and Co. are issuing, in monthly parts, a new and revised edition of Sir R. Stawell Ball's well-known "Story of the Heavens." The first part has just been published.

The additions to the Zoological Society's Gardens during the past week include a Common Fox (*Canis vulpes*), British, presented by Captain H. S. Tunnard; five White-eared Conures (*Conurus leucotis*) from Brazil, presented by Mrs. Arthur Smithers; four Leopard Tortoises (*Testudo pardalis*), three Angulated Tortoises (*Chersina angulata*), a Galeated Pentonyx (*Pelomedusa galeata*), a Hoary Snake (*Coronella cana*), a Robben Island Snake (*Coronella phocarum*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; two Alligators (*Alligator mississippiensis*) from Carolina, presented by Mr. Charles Downs; a Gold Pheasant (*Thaumalea picta* ♀) from China, presented by Mr. R. Hudson; a Pig-tailed Monkey (*Macacus nemestrinus* ♂) from Java, two Water Vipers (*Cenchris piscivora*) from North America, deposited.

SOCIETIES AND ACADEMIES.

Paris.

Academy of Sciences, August 17.—M. Duchartre in the chair.—On a new blow-pipe, by M. Paquelin.—On "cyclic systems," by M. A. Ribaucour.—New researches on the solar atmosphere, by M. H. Deslandres. (See Our Astronomical Column.)—On the enormous velocity of a solar prominence observed on June 17, 1891, by M. Jules Fényi. M. Trouvelot has previously recorded a remarkable luminous outburst that occurred on the sun on June 17. The position-angle of the group of prominences observed by M. Fényi was about 282°. At one time the velocity of one portion of the group reached the high value of about 850 kilometres per second. And another portion was elevated through about 72′ 2″ in 210 seconds—the mean velocity being at least 485 kilometres per second. It is therefore concluded from the observations that matter can be projected from the sun into space with a velocity sufficient to prevent its falling back again.—Mechanical determination of the series of atoms of carbon in organic compounds, by M. G. Hinrichs.—On the arterial system of Isopods, by M. A. Schneider.—On the growth of the shell of *Helix aspersa*, by M. Moynier de Villepoix.

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THURSDAY, SEPTEMBER 3, 1891.

THE REPORT OF THE BOARD OF TRADE COMMITTEE ON ELECTRICAL STANDARDS.

TARDILY, and in a somewhat piecemeal if not grudging fashion, some small provision has been made by Her Majesty's Government for the regulation, under the Board of Trade, of the new but vigorous and rapidly-extending industry which recent developments of electrical science have brought into existence. In no previously-existing branch of trade has the problem of settling standards of measurement been so difficult of solution, and in no other has the problem been so completely solved without trouble, expense, or intervention on the part of the Government itself. For the last twenty-five or thirty years a Committee of the British Association has laboured at the gigantic task of building up a system of units, which involved as a mere preliminary the revision of the conceptions and units of dynamics in order that these might form a basis for the definition of units for the far more complex physical quantities concerned in electricity and magnetism, quantities many of which had previously been by no means clearly apprehended, and which then received for the first time precise statement and definition.

Much of the work of the British Association Committee has been thankless, tedious, and, from its very nature, of a kind fitted to excite the cheap scorn of the self-styled "practical man," but it has made applied electricity possible, and has reacted in no slight degree on the progress of theory itself. The problem of the determination of the ohm—in other words, the process of realizing a standard of resistance according to the theoretical definition—has suggested problems to the theorist in the solution of which the theoretical investigator has been led to both direct and side-results of the very greatest value to the progress of science, and, in an unexpected manner, to the facilitation of practical applications. In no science have theory and practice been so closely connected during the last quarter of a century, and in none has the union been so markedly productive of good. By far the most interesting chapters of the history of electricity during the nineteenth century will be those that refer to its last three decades; may they chronicle a still closer alliance of the engineer and the experimenter, the electrical man of action and the mathematician! Here union is strength and dominion over the forces of Nature; disunion is waste of energy and slow progress in all that relates to the material, and therefore also to the social, advancement of the human race by means of electrical invention.

The establishment of the nucleus of an electrical standardizing laboratory in London, and the appointment towards the end of last year of a Committee to decide upon and recommend for adoption electrical standards for use in trade, testify to the great importance which the electrical industries have attained in this country in spite of the mistakes which attended their inception, and the general discouragement and disfavour with which they were received by the various interests they threatened.

The proceedings and report of the Committee have just been published in a blue-book, which contains matter of great interest to all engaged in electrical work. The vista which it opens up as regards the future operations of the standardizing laboratory may well dismay Her Majesty's Government; although no doubt due provision will ultimately be made for all its work. But of this at another time; at present we wish to direct attention to the resolutions of the Committee, which will be found in another page.

In the first place the Committee signify their adherence to the units of length, mass, and time as fundamental units, and adopt the C.G.S. system. This was only to be expected, for, after all, though some people may think that a better system could be devised if the work had to be done afresh, and they had a share in it, still collectively the body of scientific opinion is distinctly conservative, and there is little danger that any ill-advised attempt to disarrange the accepted system of theoretical and practical units will succeed.

Their third resolution, that the standard of electrical resistance should be called the ohm and should have the value 1,000,000,000 in terms of the centimetre and second in the ordinary electromagnetic system, is of great importance. It seems to settle once for all the question which has been debated over and over again, whether after a standard ohm has been realized, it will, like the standard yard or metre, be ever after the standard; or whether, if in case of variations in the physical properties of the substance, it shows an unexpectedly large divergence from the definition, a new standard ought to be constructed. Those who have assumed the former alternative have forgotten that the ohm is a derived unit, depending on the already fixed units of length, mass, and time, and that, therefore, its derivation ought to be as exact as the ever-widening resources of science can make it. For practical purposes of trade the standard fixed upon now and its copies are likely to remain undisturbed for a long time, and will probably only be corrected if there is serious alteration with time in their resistances. But the ohm will still be defined as 10^9 C.G.S. in the ordinary electromagnetic system of measurement, in which the magnetic permeability of air is assumed to be unity.

The fourth and fifth resolutions provide the definition of a practical realized ohm (1) by means of a column of mercury, (2) by comparison with the British Association unit, which it is stated may be taken as ·9866 of the ohm.

The wording of Resolution 4 strikes one as curious. The mercury column is to have a "constant cross-sectional area of 1 square millimetre." If "constant" has its ordinary sense of invariableness with time, the specification of 1 square millimetre renders it unnecessary. It has here apparently the usual sense of "uniform," that is, the section is the same at every part of the tube.

We are glad to see that the length adopted for the tube is 106·3 centimetres, instead of 106 centimetres, the round number adopted at the Paris Conference, and proposed, by the British Association Committee in 1886, to be legalized for a period of ten years. All the latest and best determinations of the ohm point to 106·3 as a convenient number very closely agreeing with the true value, and its adoption now is probably only an anticipation of the decision which will be arrived at in a few years when the resolutions of that Conference are reconsidered.

In the adoption of a metallic working standard (announced in Resolution 5) the Committee only endorse an opinion long ago expressed by working electricians, that the mercury standards constructed in straight or spiral glass tubes are not practical instruments; they are difficult to handle, liable to breakage, and the only argument for their retention, the possible variability of metallic standards, has been shown to be almost baseless by the results of the continued and careful observation of the various metallic resistance coils deposited at Cambridge.

Passing over the resolutions which provide for copies, and multiples and submultiples of the ohm, with the remark that the long-felt want of trustworthy standards of low resistance will now at last be supplied, we come to the definition of the unit of current. Here again a theoretical definition corresponding to that of the ohm is given first; then for practical purposes it is stated "that an unvarying current which, when passed through a solution of nitrate of silver in water, in accordance with the specification attached to this report, deposits silver at the rate of 0·001118 of a gramme per second, may be taken as a current of 1 ampere." This is the most reasonable course that could have been adopted. The specification is practically one of the procedure adopted by Lord Rayleigh in his experiments on the electro-chemical equivalent of silver, and as Lord Rayleigh's absolute result was to be made the practical standard, it was right to recommend the same mode of experimenting.

Resolution 11, which defines the ampere in the case of an alternating current, was the subject of a good deal of discussion, and of some adverse comment by one of the witnesses examined on behalf of the electrical trades. The resolution states "that an alternating current of 1 ampere shall mean a current such that the square root of of the time-average of the square of its strength at each instant in amperes is unity." It was objected by the witness referred to, and by at least one member of the Committee, that this was giving a very special meaning to the term, which was inconsistent with the obvious definition, that of the simple time-average of the current. This latter average would, in the case of most periodic machines, be simply zero, unless the currents in the alternate half-periods were commutated so as to agree in sign with those in the other halves. But in the case of such a machine as the Brush, used for lighting incandescent lamps, the definition given in the resolution would have to be used; whereas if the machine were used for electro-plating, the simple time-average would have to be employed. This would give for the same current passing through the machine, from instant to instant, two different average values. The electric lighting application of periodic machines is, however, by far the most important, and the Committee did well, perhaps, to retain what is already the generally understood sense of the word *ampere* in connection with alternating currents. It ought to be, however, clearly understood that the main application of the definition will be to the measurements of currents in electric lighting, and that generally in other cases another definition will have to be employed.

Another important discussion took place over the definition of the standard unit of "pressure." In the first place, we should like to say here that we object entirely to the use of the term "pressure" in this connection. It has come as a sort of analogue of hydraulic pressure, and it has certainly led to very erroneous notions in the minds of the general public as to the functions of electric supply-mains, and also as to electricity itself. It is a pity that so many of the present pioneers of electricity, who are also leaders of physical science, should have countenanced by their example this misuse of a scientific term. We all know how strenuously some of these gentlemen have objected to the term "tension" as in "high-tension electricity"; surely "high-pressure instruments" and "electricity supplied at high pressure" are as objectionable, if not even more misleading. The use of the term *voltage*, or some such word, in the present Report, would have avoided the endorsement which it seems to give to what we think is a most unfortunate name for a physical quantity which is not a pressure at all; and it is to be hoped that the British Association Committee (who, by the way, were represented on the Committee of the Board of Trade) may be able to prevent this phrase from being added to the many other, though generally less objectionable terms which infest the literature of electricity.

A discussion arose as to whether the definition of the volt as the "pressure which, if steadily applied to a conductor whose resistance is one ohm, will produce a current of one ampere," was sufficiently definite. There might, it was argued, be an internal electromotive force in the conductor, and the "pressure" applied to the conductor might be regarded as that applied from the outside, or actually existent between its terminals, as shown by an electrometer. For example, the conductor might be the armature of a dynamo; the difference of potential might be considerable and the resistance only a small fraction of an ohm. In such a case it is, of course, well known that the electromotive force producing the current through any part of the armature resistance, according to Ohm's law, is the total internal electromotive force of that part, *minus* the difference of potential existing between its terminals, and it is this difference that is to be regarded as the applied "pressure" of the definition. In the same way in a voltameter, the electromotive force causing the current, according to Ohm's law, would be the existent or applied difference of potential, *minus* the internal back electromotive force developed by the chemical action. There were other difficulties about the specification of the ends of the conductor and the canalization of the current, and it was therefore thought desirable to adhere to the simple form of definition given in the report. It must be admitted that the definition leaves room for legal disputes in practice, and we think that it would have been perhaps better to have introduced on these points some kind of note or specification referred to in the resolution, so as to be taken along with it in the event of any dispute about the meaning of the definition.

A further question arose as to the provision of a practical standard of electromotive force in the form of a constant cell; and it was decided, partly in deference to the expressed wish of practical electricians, that the Clark cell should be adopted for this purpose. Its electromotive force, within certain limits of error to be determined by a sub-committee appointed for the purpose of preparing a specification for the construction and use of the cell, is stated to be 1·433 volts at the temperature 62° F. By means of this cell and known resistances, it will be possible to calibrate instruments without the use of electrolysis, and this to many persons would be the readiest and most easily carried out method. Of course, logically speaking, the standard of electromotive force is settled when those of resistance and current are fixed, and thus, if the order of definition is adhered to, the cell does not come in. But its electromotive force having been determined by careful measurement, and found to be so constant as it is, and so consistently the same in different specimens when the mode of construction is carefully attended to, it is too valuable a standard of reference to be set aside.

A very interesting discussion took place as to the mode of preparing these cells, and on the experience of different investigators as to their behaviour. Some of the divergences stated in the discussion were probably due to the different degrees of manipulative skill possessed by the various observers. A few careful experiments with different batches of cells carried out personally by the members of the committee interested in the matter would set the question at rest, and probably entirely confirm Lord Rayleigh's marvellously consistent results.

A side-point which came out in discussion is worthy of notice. We have not in this country any legal definition of temperature, whether Centigrade or Fahrenheit. In the definition of the standard yard 62° Fahrenheit is specified, but there is nothing to tell how that temperature is to be determined. It is well known (though apparently not to some of the text-book writers on heat) that mercurial thermometers, made with different kinds of glass, while agreeing at the freezing and boiling points, agree nowhere else, and all differ more or less from the air-thermometer. In very accurate work these discrepancies become very important, and thermometers must be calibrated by means of standards, if their indications are to be of any use for comparison. Some legal definition of temperature will, ere long, have to be given, and it seems rather a pity that the Committee did not practically settle this by saying what they meant by 62° Fahrenheit.

The definition of the volt for alternating currents, embodied in Resolution 15, is, of course, a mere consequence of Resolution 11, and these two definitions taken together are specially applicable to the measurement of the power spent in lighting incandescent lamps.

We have only to note that the Committee, in Resolutions 12 and 16, adopted instruments on the principle of the balance for the measurement of currents, and on the principle of Sir William Thomson's quadrant electrometer, used idiostatically, for the measurement of differences of potential, except for large differences, when an electrometer on the principle of the balance is to be employed. Thus the beautiful electrometers invented long ago by Sir William Thomson are likely to become at last, in a modified form, Board of Trade standards of exact measurement in industrial electricity. This is by no means the only striking example which could be cited of the thoroughly practical, because thoroughly theoretical, character of the instruments invented by one who understands all sides of the difficult problem involved in the invention and construction of such apparatus.

No resolutions were framed by the Committee on the very important subject of the measurement of power and energy. This must, however, come to the front before very long, and will tax the resources of the standardizing laboratory and its officials, assisted, as no doubt they will be, by Committees such as this which has just reported. We congratulate the Committee on the results of its labours, and trust that the requisite Order in Council will be passed before long confirming its resolutions. The laboratory will then be able to get to work, the necessary standards which have been asked for so long will be made accessible to those engaged in the electrical industries, and some serious difficulties under which they have laboured, in supplying electric light and power to the public, will be at last removed.

THE CONGRESS OF HYGIENE.

We print to-day a report of the important discussion in Section II. (Bacteriology) of the Congress of Hygiene, on "Immunity, Natural and Acquired":—

Dr. Roux, of the Institut Pasteur, in an introductory address, indicated the scope of the discussion. He began by saying that, in inviting a pupil of M. Pasteur to open the discussion on this subject, the Organizing Committee had reminded the Section that the great amount of interesting work which had recently been done on the subject had one point in common—namely, the attenuation of virus, and preventive inoculation, the two subjects with which M. Pasteur's name would for all time be honourably associated. With the single notable exception of vaccination, the only way of conferring immunity against any disease was the inoculation of the virus of the disease. To the old dangerous method of producing immunity by inoculation, Pasteur had added the less dangerous one of preventive inoculation by means of an attenuated virus, to which he had applied the term vaccination. The designation "attenuated" virus ought to be reserved for virus weakened without being attenuated—for example, by artificially lowering the vitality of the organisms for producing it.

Methods of Attenuation.—Two methods of attenuation had been described by M. Pasteur—namely, the prolonged exposure of a culture to air at a suitable temperature, and the passage of the micro-organisms through the bodies of different species of animals. Other methods had also been employed—for example, the action of heat, the use of antiseptics, of compressed oxygen and light.

In all cases, whatever the method employed, it was found to be necessary that the attenuation should be effected slowly and gradually; rapid attenuation rendered a virus altogether inactive without impressing on it any hereditary weakness. In whatever way the virus was prepared, it must, in order to confer immunity, be brought into direct contact with the tissues of the animal. In the early experiments the virus employed was always living; the living microbe, itself attenuated as to its virulence, was used. Another possible method of conferring immunity was the inoculation of the chemical substances produced by the micro-organisms.

Phagocytosis.—Dr. Roux next dealt with the doctrine of phagocytosis associated with the name of Dr. Metchnikoff. This observer had proved, by the study of the amœboid movement of certain cells that they possessed the power of including other cells and bodies in their substance. The phagocyte cells originated in the mesoderm. They possessed, further, the property of being able to digest the bodies which they had ingested. They were, in fact, the only cells which manifested in the human body any intracellular digestion. If the history of a bacterium in the interior of a phagocyte were followed, it would be seen that it underwent a peculiar series of alterations, very different from what took place when a microbe died in cultivating fluids. Whether a virulent virus was introduced into the bodies of animals which resisted inoculation, or whether attenuated microbes were injected into sensitive animals, the greater the degree of refractoriness shown by the animal, the more rapidly the microbes were consumed by the leucocytes. In a non-resistant animal the microbes remained free; no such phenomenon as phagocytosis could be observed. It seemed, therefore, that the phagocytes were charged with the defence of the human organism, and entered into conflict with the parasites which infected the human frame. It might be said that there were diseases in which the microbes were to be met with in the cells specially, and that these microbes nevertheless proved fatal to the animal. In tuberculosis and in leprosy the bacilli were to be found in the cells, and the results were of the most serious kind, in spite of the intense phagocytosis induced by the microbes of these diseases. This fact proved that the phagocytes and all the other means of defence were, under certain conditions, and at certain times, powerless to effect any good results; they had done their best to take up the microbes, but these had adapted themselves to the interior of the cells, and had conquered. It was not sufficient that the microbes should be eaten up, it was essential that they should also be digested by the phagocytes. Even in those cases where the struggle was going against the human organism, these cells still were the aggressors. It had been frequently observed in tuberculosis and leprosy that the bacilli had been killed in the interior of certain of these cells. The theory asserted that a struggle occurred between the microbes and the cells, but it did not imply that the bacilli always won the day. Phagocytosis only occurred in immune animals; in animals susceptible to the disease it was either not to be observed, or it was incomplete.

He then proceeded to discuss the questions whether immunity was the consequence of this power of the cells to digest the virulent microbes. As had been said, the cells of a refractory animal took up the microbes, which, it would appear, under favourable circumstances remained inert in the interior of the cells.

Numerous facts had been alleged to show that the microbes at the time they were taken up by the phagocytes were not degenerated, but were, on the contrary, in a condition of full activity. Thus, to take only one example, it had been found that in frogs the bacilli which had been taken up by the lecocytes remained alive within the protoplasm of the cell; this was apparent from their movements. In lymph taken from the body of a pigeon, numerous bacilli were to be seen imprisoned in the leucocytes, and these bacilli could be watched growing, actually under the eye of the observer, within the interior of dead phagocytes; they could be seen to elongate, to push out the protoplasm, distort the form of the cell, and finally to make their escape. Another demonstration of the importance of the action of the phagocytes was afforded by the fact that even in immune animals the microbes were found to increase when kept out of the reach of the leucocytes; thus, if a rabbit were inoculated in the anterior chamber of the eye, where there were no cells, the bacteria grew freely, and their development was only checked when the leucocytes had after a time migrated in large numbers, and began to take the microbes into their interior. It thus appeared that phagocytosis was a very general phenomenon, and one which was very efficacious in checking the advance of the organisms; when it failed, the individual succumbed to the virulence of the bacteria. The question remained, What was the mysterious force which attracted the cells towards the microbes? Why were the leucocytes, which in immune animals destroyed the microbes, incapable of seizing upon them in non-immune animals?

In 1883, Metchnikoff propounded his theory of phagocytosis. This theory rested on two assumptions: first, that the cells were attracted to the microbes in virtue of a special sensibility manifested towards all foreign bodies introduced into the tissues; the second was that this power of seizing upon the virulent microbes in immune animals originated in a habit formed during the earlier struggle with the attenuated virus with which the animal had been previously inoculated. The behaviour of the leucocytes might be more readily explained by assuming that leucocytes had the property, analogous to that possessed by the zoosperms of the myxomycetes—namely, that of being attracted by certain bodies and repelled by others. MM. Massart and Bordet had proved that the products of the microbes exerted a very marked chemical action on the phagocytes. When a virus was introduced into the body, it proliferated, and secreted a substance which attracted the leucocytes; the more active the virus, the more energetic were the poisons elaborated by it, and the cells which penetrated to the point of inoculation were paralyzed in their action, and rendered incapable of taking up the microbes, which therefore proliferated without hindrance. Further, in certain diseases the virus produced a substance which was still more poisonous. In chicken cholera, for instance, the poison secreted by the microbes repelled the leucocytes from the point of inoculation; it thus came about that phagocytes were never found in this particular affection. This, however, was not the case with animals which had been rendered immune either by inoculation of the attenuated virus, or by the injection of a suitable dose of bacterial products. If the animal were given a strong virus, phagocytes were attracted to the point of inoculation, and these possessed the power of taking up the microbes before they had time to elaborate effective doses of their toxic material. It was, therefore, at the commencement of the disease that the critical struggle took place. If the leucocytes could not accomplish this at the beginning of the malady, their action at a later period would be useless, since the microbes would have produced enough poison to paralyze their activity. Every cause, therefore, that prevented the access of leucocytes to the point of inoculation facilitated infection. The theory of immunity propounded by M. Metchnikoff did not exclude the possibility of there being other means of protecting the organism, but it simply proved that phagocytosis had a wider sphere of action, and was more efficacious, than any other means of protecting the organism. It seemed to explain all the facts, and was, moreover, eminently suggestive. It was in this way that the knowledge of microbic poisons and chemical inoculation had thrown light on what would otherwise have been obscure. Far from being shaken by the theories which were opposed to it, this theory of Metchnikoff's had gained by the opposition which it has met, and that was a guarantee of its soundness.

Dr. Buchner, of Munich, after giving a general account of the various theories of immunity, criticized freely Metchnikoff's views. The main objections he brought forward were as follows:—

(1) Many observers failed to notice any destruction of bacilli by phagocytes, when naturally immune animals, such as white rats or pigeons, were inoculated with anthrax.

(2) In diseases ending fatally, such as tuberculosis, mice-septicæmia, &c., the micro-organisms were frequently found in the interior of phagocytes.

(3) The experiments of Petruchky, Baumgarten, Pekelharing, and others seemed to show that the bacilli of anthrax perished in the living fluids of immune animals even when the bacilli were protected against the attacks of white corpuscles.

Metchnikoff, however, denied this, and proved that the living fluids of immune white rats form a most excellent cultivating medium for the bacilli of anthrax. These observations of Metchnikoff, according to Buchner, might be explained by the fact that Metchnikoff in his experiments introduced more bacilli than could be destroyed by the living fluids of white rats, as a certain quantity of serum was able to destroy only a very small quantity of micro-organisms. Speaking of the experiments made by his pupils Ibener and Roeder, he stated that, when a certain kind of micro-organisms were placed into a given quantity of serum, the micro-organisms might either be destroyed *in toto*, or reproduce themselves in large numbers according to the number of micro-organisms introduced in the first place into the serum. When, instead of placing the micro-organisms directly in contact with the serum, the micro-organisms were wrapped up in sterilised cotton-wool, it was found that the bacilli, so protected against the temporary harmful influence of serum, began to grow luxuriantly at the end of twenty-four hours. The bactericidal power of serum disappeared, therefore, shortly after death.

Massart, Bordet, and Gabritchewsky had previously proved that the emigration of leucocytes to the spot where the virus was introduced was due to the attracting influence (positive chemotaxis) of the chemical poisons secreted by micro-organisms, but he (Buchner) was of opinion that the substances dissolved in the cultures have hardly any action on leucocytes, but that this attracting influence on leucocytes was due to the protein present in bacterial cells themselves. Whereas the products of the metabolism of micro-organisms had little or no attracting influence on the leucocytes, the proteins themselves attracted the cells most powerfully.

As long as the bacterial cells were active and capable of reproducing themselves actively, the proteins were contained in the cells, and these poisons only left the cells when the latter became diseased or old. Hence these proteins were chiefly found in old cultures, the filtered and sterilised extracts of which always possessed a strong attracting influence on leucocytes. Hence it followed that, "The more a given micro-organism is harmfully influenced by the living fluids of a given species of animals, the more proteins will be excreted. This, as a natural consequence, is followed by a corresponding increase in the number of cells which emigrate to the point of inoculation." In every case the living fluids of the body exert a harmful influence on micro-organisms, and then, when in consequence of this the excretion of proteins takes place, the amœboid cells emigrate to the spot.

Turning now to the characteristics of this germicidal substance present in serum, he thought that this germicidal power gradually disappeared, so that after a few days the serum had no bactericidal power. This germicidal action was destroyed by the micro-organisms themselves, for, unless the latter were completely destroyed, they soon began to grow freely in serum. This germicidal substance was easily destroyed by heat. Serum which had been maintained at 55° C. during half an hour, or at 52° C. during six hours, lost its bactericidal power completely. A moderate degree of warmth (37° C.) intensified the germicidal action of the blood or serum.

Turning now to the question as to whether this bactericidal action of the blood had any share in the production of immunity, he gave the following facts as proving that there was some connection between the immunity of a given animal against a given infectious disease, and the bactericidal action of its blood on the micro-organism producing the disease:—

(*a*) The blood and serum of animals, such as mice and guinea-pigs, which readily succumbed to anthrax had no bactericidal power on anthrax-bacilli.

(*b*) The serum of animals which took anthrax readily never possessed such a strong bactericidal action as the serum of white rats, which were immune against anthrax.

(*c*) The blood and serum of animals rendered artificially immune possessed stronger bactericidal powers than the blood and serum of normal animals.

(*d*) The blood and serum of animals rendered artificially immune against a given micro-organism lessened the virulence of the specific micro-organism causing the disease.

(*e*) Whenever blood and serum possessed no bactericidal action on micro-organisms, this absence of bactericidal action might be due to the fact that, owing to the necessary manipulations, this bactericidal substance had been altered or even destroyed.

As further proving that the immunity of animals depended on some substance present in the serum, he mentioned the facts described by Behring, Kitasato, Ogata, and Emmerich, in which the injection of blood or serum of an animal immune against a given bacillus, cured another animal afflicted with the same disease. This curative power he attributed to the presence in the blood of immune animals of a protective substance, probably proteid in its nature, to which he gave the name of "alexine" (from ἀλεξεῖν, to protect). These alexines were not ordinary oxidation products of the tissues, as they were quite specific in their action. They were not simply enzymes, as they had no hydrolytic properties, but they were most probably proteid substances. These alexines were probably formed in the cells; but, when formed, their action was quite independent from that of cells, and they were probably always present in immune animals.

Mr. E. H. Hankin, of Cambridge, after giving a *résumé* of the work done by various observers, said that theoretical considerations led him to suspect that a particular ferment-like proteid, known as cell globulin B, was a substance possessing bactericidal power. He tested its action on anthrax bacilli, and found that it had the power of destroying these microbes.

He further found that similar substances were present, not only in animals that were naturally immune against anthrax, but also in those that were susceptible to this disease. To these substances he had given the name of *defensive proteids*. In his published papers on this subject he had noted various similarities in the bactericidal action of these substances, and that possessed by blood-serum, and these resemblances were such as to leave little room for doubt that the bactericidal action of blood-serum was due to the presence of these defensive proteids.

The serum of white rats contained a proteid body possessing a well-marked alkaline reaction, and a power of destroying anthrax bacilli. Further, when injected into mice along with fully virulent anthrax spores, it would prevent the development of the disease. On the other hand, defensive proteids of animals susceptible to anthrax did not exert such protective power, and consequently these experiments indicated a difference in the mode of action of defensive proteids of immune and non-immune animals respectively. Further, the amount of defensive proteid present in a rat could be diminished by the causes which were known to be capable of lowering the animal's power of resisting anthrax. For instance, Feser stated that rats become susceptible to anthrax when fed on a vegetarian diet. Mr. Hankin obtained similar results with wild rats. The ordinary white rat he found to be generally refractory to anthrax on any diet, and the defensive proteid could always be obtained from its spleen and blood-serum. This was not the case with wild rats. In one experiment eight wild rats were used; of these, four were fed on bread and meat, the others on plain bread, for about six weeks. Then one rat of each lot was inoculated with anthrax; of these, the one that had been subjected to a bread diet succumbed. The remaining rats were killed, and it was found that while the spleens of the flesh-fed rats contained abundance of the defensive proteid, only traces of this substance could be obtained from the spleens of the rats that had been fed on bread alone. A similar result was obtained in other experiments.

Very young rats were known to be susceptible to anthrax, and so far as could be judged from the litmus test (after dialysis and addition of NaCl), their serum appeared to contain less of the defensive proteid than did that of the adult rat. Further, Mr. Hankin found that a young rat could be preserved from anthrax by an injection of its parent's blood-serum.

These facts appeared to prove that the defensive proteid of the rat deserved its name, in that it preserves the animal from the attack of the anthrax microbe; in other words, that this substance was at any rate a part cause of the rat's immunity against anthrax.

Defensive proteids appeared to be ferment like, albuminous bodies, and it was extremely unlikely that we should for a considerable time be able to classify them by any other than physiological tests. From this point of view it was possible to divide them into two classes; first, those occurring naturally in normal animals, and secondly, those occurring in animals that have artificially been made immune. For these two classes Mr. Hankin proposed the names of *sozins* and *phylaxins*. A "sozin" was a defensive proteid that occurred naturally in a normal animal. They had been found in all animals yet examined, and appear to act on numerous kinds of microbes or on their products. A "phylaxin" was a defensive proteid which was only found in an animal that had been artificially made immune against a disease, and which (so far as is yet known) only acted on one kind of microbe or on its products.

Each of these classes of defensive proteids could obviously be further subdivided into those that acted on the microbe itself, and those that acted on the poisons it generated. These subclasses he proposed to denote by adding the prefixes myco- and toxo- to the class name. Thus myco-sozins were defensive proteids occurring in the normal animal, which had the power of acting on various species of microbe. Toxo-sozins were defensive proteids, also occurring in the normal animal, having the power of destroying poisons produced by various microbes. Myco-phylaxins and toxo-phylaxins similarly would denote the two sub-classes of the phylaxin group.

The classification might be represented by the following scheme:—

- Defensive proteids (Hankin). Alexins (Buchner).
  - Sozins:— Defensive proteids present in the normal animal.
    - Myco-sozins:— Alkaline globulins from rat (Hankin), destroying anthrax bacillus.
    - Toxo-sozins:— Of rabbit, destroying *V. metchnikovi* poison (Gamaleia).
  - Phylaxins:— Defensive proteids present in the animal after it has been made artificially immune.
    - Myco-phylaxins:— Of rabbit, destroying pig typhoid bacillus (Emmerich).
    - Toxo-phylaxins:— Of rabbit, &c., destroying diphtheria and tetanus poisons (Behring and Kitasato, antitoxin of Tizzoni and Cattani).

Prof. Emmerich, of Munich, read a paper on "The Artificial Production of Immunity against Croupous Pneumonia and the

Cure of this Disease." He stated that his previous experiments on swine fever had proved that in immune animals the bacilli of swine fever were destroyed, not by the cells of the animal, but by a bactericidal substance present in the blood. It had been clearly proved by his experiments that the bacilli of swine fever were destroyed almost immediately after their introduction under an immune animal's skin. Applying these researches to the disease produced in rabbits by the inoculation of the *Diplococcus pneumoniæ* of Fraenkel, he showed that non-immune rabbits died within twenty-four to forty-eight hours after the introduction of the virus. But if such animals had been previously treated with the blood or serum of animals rendered artificially immune against the diplococcus of Fraenkel, such animals did not die, but recovered after the introduction of extremely virulent diplococci. Moreover, when the *Diplococcus pneumoniæ* was inoculated into an animal, it was possible to cure it by injecting shortly afterwards some of the serum of an animal rendered artificially immune. In the blood of animals rendered artificially immune against pneumonia we possessed an excellent cure for the disease. Not only would it be possible to cure men afflicted with pneumonia by these injections, but we could, by preventive inoculations applied in time, put a stop to the spread of an epidemic in a school or a prison for instance. His experiments, together with Dr. Doenissen's, had a great practical as well as a theoretical value.

Dr. Ehrlich, of Berlin, stated that he had lately made a number of experiments with ricin which threw great light on the question of immunity. According to Kobert and Stillmark, ricin was an extremely poisonous body, for it acted fatally when such small doses as 0·03 mg. were injected into an animal's veins. When absorbed through the alimentary canal, a dose 100 times larger could be easily tolerated. Nevertheless, even then, it was so toxic that, according to Kobert's reckoning, a dose of 0·18 gr. would prove fatal to a full-grown man. It had a harmful influence on the blood, producing coagulation of the red blood-corpuscles, and thromboses, more especially of the vessels of the alimentary canal.

In his opinion the toxicity of ricin greatly depended on the species of animals used for experiments, the animals most susceptible to its action being guinea-pigs. Thus, a guinea-pig weighing 385 grammes died eleven days after the inoculation of 0·7 cc. of a 1 in 150,000 solution of ricin, the *post-mortem* examination showing characteristic hæmorrhages in the alimentary tract. One gramme of this substance might therefore prove fatal to 1,500,000 guinea-pigs. White mice, on the other hand, did not die after much larger doses, and this immunity of mice against this poison might be increased by subcutaneous injections of ricin. The same result might be obtained, however, far more easily and without any chances of failure, by feeding mice with ricin. It was best to begin with small, harmless doses, gradually increasing the amount until the organism was accustomed to the poisonous substance. In ten days a mouse might then be inoculated with a deadly or even larger dose without suffering any evil effects. Thus, whilst doses of 1/200000 gramme was absolutely fatal in normal animals, mice fed daily and in increasing quantities with ricin suffered no harm after the injection of 1/1000 gr. or 1/500 gr., or, occasionally, of 1/250 gr.

Whilst a 0·5 or 1 per cent. solution of ricin applied to the eye of a normal animal produced severe inflammation and panophthalmitis, the application of a 10 per cent. solution of ricin produced no effect on the eye of an animal previously fed with ricin. In other words, this was distinct proof of the existence of a local as well as of a general immunity against the poison. Strangely enough it was almost impossible to render the subcutaneous tissue immune against ricin, and even in exceedingly immune animals the subcutaneous injection of ricin produced distinct necrosis of the subcutaneous tissue.

It was a remarkable fact that this immunity appeared quite suddenly on the sixth day, and then increased slowly, so that on the twenty-first day the animal could stand a dose which was 400 times higher than that fatal to a normal animal.

This immunity against ricin appeared to be permanent, for it was still present in immune mice which had not taken ricin for a period of six months previously.

He had been able to extract from the blood of animals rendered immune against ricin a body which had the power of counteracting the toxic action of ricin, so that a powerful solution of ricin was rendered harmless by admixture with the blood of immune mice. It was also possible to render animals immune against ricin by injecting the blood of immune animals.

He had obtained similar results with abrin, which would be shortly published.

Dr. Kitasato, of Tokio, shortly summarized the results which he and Dr. Behring had obtained with the virus of tetanus. According to these observers, the blood of a normal rabbit has no influence on the toxines secreted by the bacillus of tetanus. But when a rabbit had been rendered artificially immune against that disease, its blood had the power of destroying the toxines secreted by the specific bacillus. Nay, more, the blood of rabbits made artificially immune against tetanus with trichloride of iodine, rendered mice not only refractory to tetanus but also cured the disease when already in progress. The blood, however, did not appear to act on the tetanus bacillus itself, but on the toxines secreted by the bacillus.

Dr. Adami, of Cambridge, thought that it was impossible to doubt that in a large number of infectious diseases the process of phagocytosis was extremely marked. He was of opinion that it was quite possible to accept both views of the question. The controversy had taken place chiefly as to the phenomena observed in the rat; in that animal phagocytosis was only to be observed with difficulty, and the serum of rat's blood undoubtedly possessed bacteria-killing properties to a high degree.

Dr. Klein, of London, stated that frogs and rats were insusceptible to anthrax, but that these animals could be made susceptible to the disease by a variety of means, indicating that their normal power of resistance was due to certain chemical conditions of the blood. If the bacillus of anthrax was introduced into the lymph-sac of a chloroformed frog, this animal always died of anthrax. Rats inoculated with anthrax and kept under the influence of an anæsthetic also died of anthrax. He had been unable to find any evidence to show that in these cases the leucocytes had lost their power of swallowing up bacteria, and therefore the susceptibility of chloroformed animals to anthrax could only be explained by some chemical changes taking place in the serum of the chloroformed rat or frog.

Dr. Metchnikoff, of Paris, who was greeted with loud and prolonged cheering, said that, of all the objections which have been raised against the theory of phagocytes, doubtless by far the most important was that formulated by Behring and Nissen: namely, the fact that the serum of guinea-pigs vaccinated against the vibrio of Metchnikoff had bactericidal powers on the same vibrio. Whilst the serum of normal guinea-pigs allowed the free development of a large number of these microbes, the serum of vaccinated animals killed the micro-organisms at the end of a few hours. MM. Behring and Nissen were convinced that this fact formed a complete explanation of the acquired immunity of guinea-pigs against the *Vibrio Metchnikofi*, and that it might serve as a model for a theory of immunity. His own researches, however, proved the contrary. If one studied the phenomena as they occurred in the living animal, one noticed at once that the bacilli inoculated into immune guinea-pigs remained alive for a very long time. Some vibrios were taken into the interior of leucocytes at the point of inoculation, whilst others developed perfectly in the liquid exudation. To show this, one had only to take a drop of the latter, and place it in the warm chamber; the leucocytes perished when taken out of the organism, and allowed the bacilli contained in their interior to develop freely. The vibrions thus multiplied and filled the leucocytes, which swelled and eventually burst, allowing the microbes to pass freely into the liquid part of the exudation. Here the development continued, and one obtained very abundant cultures from the liquid exudation of the immune guinea-pig. If one extracted a small quantity of such a culture, and introduced it into the dead serum of an immune guinea-pig, this serum not only did not kill the bacilli, but also gave a more abundant development than the serum of a non-immune animal could do. The study of the phenomena in living animals made artificially immune against the vibrio of Metchnikoff, instead of overthrowing the theory of phagocytosis, furnished on the contrary an evident proof in its favour. The theories of the attenuation of virus in the bodies of immune animals, and of the neutralization of the toxines, could not be applied to his case, as the vibrios remained very virulent, and because the immune guinea-pigs are as sensitive to the toxine of the bacillus as the non-immune animal.

This example showed yet once more that one must not be content with studying the phenomena of immunity outside the organism. This criticism also applied to M. Buchner's experiments, which he had communicated to this meeting; he insisted on the fact that, in order to assure one's self thoroughly of the bactericidal property of the serum, it was necessary to take a small quantity of the culture, and spread it in a tube filled with serum. If, according to Dr. Buchner, one introduced a little of the culture wrapped in cotton-wool, the serum could no longer exercise its bactericidal power, and the microbe developed freely. Now, when one inoculated the bacillus under the skin of an animal, one introduced at the same time a small mass which did not spread freely in the blood or exudation, but remained localized at one spot. The experiments of Mr. Buchner, instead of furnishing an objection to the phagocyte theory, rather supported it.

Referring to the curative properties of the serum of white rats against anthrax, he had come to the conclusion that, whereas the living serum of white rats had no bactericidal action on anthrax, the dead serum of the same animals had marked bactericidal powers on the same micro-organism. When a mouse was inoculated with a mixture of the dead serum of a rat and anthrax bacilli, it nearly always died, although the disease lasted somewhat longer than usual. On examination of the point of inoculation it was found that the bacilli of anthrax did not grow quite so readily, and that an enormous number of leucocytes emigrated to the point of inoculation and took the bacilli into their interior and digested them. In tetanus, again, the leucocytes ate up considerable quantities of tetanus-spores and bacilli. Summing up his researches, he stated that whenever an animal recovered from an infectious disease this recovery was accompanied by a process of phagocytosis; whenever an animal died of an infectious disease the process of phagocytosis was absent or insufficient. The theory of phagocytes was strictly based on the principles of evolution as laid down by Darwin and Wallace.

After some remarks by Dr. Fodor, Dr. Cartwright Wood, Prof. Babes, Dr. Wright, and Dr. Arloing,

Dr. Roux, answering some remarks made by Prof. Emmerich, stated that, far from the preventive inoculations against anthrax and swine fever having been proved to be unsuccessful, agriculturists in France and other countries were making use of them daily, and the use of the various vaccins manufactured at the Institut Pasteur was increasing day by day.

Dr. Buchner congratulated Dr. Metchnikoff on his most important paper. He was of opinion, however, that the time for framing a complete theory of immunity had not come yet.

Sir Joseph Lister then stated that if anything were required to justify the existence of this Congress it would have been their sitting that day. The immense amount of valuable material which they had had on this most important subject had been such as to make all the members exceedingly grateful to those who had brought these matters before them.

THE BRITISH ASSOCIATION.

THE following is a list of the grants of money appropriated to scientific purposes by the General Committee at the Cardiff meeting, August 1891. The names of the members entitled to call on the General Treasurer for the respective grants are prefixed:

A.—*Mathematics and Physics.*

	£	s.	d.
*Foster, Prof. Carey—Electrical Standards (partly renewed)	27	4	6
*McLaren, Lord—Meteorological Observations on Ben Nevis	50	0	0
*Symons, Mr. G. J.—Photographs of Meteorological Phenomena	15	0	0
*Cayley, Prof.—Pellian Equation Tables (partly renewed)	15	0	0
*Rayleigh, Lord—Tables of Mathematical Functions	15	0	0
*Fitzgerald, Prof. G. F.—Electrolysis	5	0	0
*Lodge, Prof.—Discharge of Electricity from Points	50	0	0
*Thomson, Sir W.—Seismological Phenomena of Japan	10	0	0
B.—*Chemistry and Mineralogy.*			
*Roberts-Austen, Prof.—Analysis of Iron and Steel (renewed)	8	16	0
*Armstrong, Prof. H. E.—Formation of Haloids from Pure Materials (partly renewed)	25	5	0
*Tilden, Prof. W. A.—Properties of Solutions ...	10	0	0
*Thorpe, Prof.—Action of Light upon Dyed Colours (partly renewed)	10	0	0
C.—*Geology.*			
*Prestwich, Prof.—Erratic Blocks (partly renewed)	15	0	0
*Wiltshire, Rev. T.—Fossil Phyllopoda (renewed)	10	0	0
*Geikie, Prof. J.—Photographs of Geological Interest	20	0	0
*Woodward, Dr. H.—Registration of Type Specimens of British Fossils (renewed)	5	0	0
*Hull, Prof. E.—Underground Waters	10	0	0
*Davis, Mr. J. W.—Investigation of Elbolton Cave	25	0	0
Jones, Prof. R.—Faunal Contents of *Sowerbyi* Zone	10	0	0
*Evans, Dr. J.—Excavations at Oldbury Hill ...	25	0	0
*Woodward, Dr. H.—Cretaceous Polyzoa... ...	10	0	0
D.—*Biology.*			
*Sclater, Dr. P. L.—Table at the Naples Zoological Station	100	0	0
*Lankester, Mr. E. R.—Table at Plymouth Biological Laboratory (renewed)	17	10	0
*Haddon, Prof. A. C.—Improving a Deep-sea Tow-net (partly renewed)	40	0	0
*Newton, Prof.—Fauna of Sandwich Islands (renewed)	100	0	0
*Sclater, Dr. P. L.—Zoology and Botany of the West India Islands (renewed)	100	0	0
E.—*Geography.*			
Ravenstein, Mr. E. G.—Climatology and Hydrography of Tropical Africa	75	0	0
H.—*Anthropology.*			
*Flower, Prof.—Anthropometric Laboratory ...	5	0	0
Garson, Dr. J. G.—Prehistoric Remains in Mashonaland	50	0	0
*Tylor, Dr. E. B.—North-western Tribes of Canada	100	0	0
*Turner, Sir W.—Habits, Customs, &c., of Natives of India (renewed)	10	0	0
*Flower, Prof.—New Edition of Anthropological Notes and Queries	20	0	0
*Symons, Mr. G. J.—Corresponding Societies' Committee...	25	0	0
	£1013	15	6

* Reappointed.

SECTION E.

GEOGRAPHY.

OPENING ADDRESS BY E. G. RAVENSTEIN, F.R.G.S., F.S.S., PRESIDENT OF THE SECTION.

The Field of Geography.[1]

IT behoves every man from time to time to survey the field of his labours, and to render an account unto himself of the work he has accomplished, and of the tasks which still await him, in order that he may perceive whether the means employed hitherto are commensurate with the magnitude of his undertaking, and likely to lead up to the desired results. Such a survey of the "Field of Geography" I propose to make the subject of my address to-day. . . .

Whatever changes may have taken place respecting the aims of the geographer, it is very generally acknowledged that the portraiture of the earth's surface in the shape of a map lies within his proper and immediate domain. And there can be no doubt that a map possesses unique facilities for recording the fundamental facts of geographical knowledge, and that with a

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[1] Pressure on our space compels us to omit some parts of this address.

clearness and perspicuity not attainable by any other method. You will not, therefore, think it strange if I deal at considerable length with the development of cartography, more especially as my own labours have in a large measure been devoted to that department of geographical work. An inspection of the interesting collection of maps of all ages which I am able to place before you will serve to illustrate what I am about to say on this subject. . . .

Ptolemy, like all great reformers, stood upon the shoulders of the men who had preceded him, for before a map like his could be produced much preliminary work had been accomplished. Parmenides of Elea (460 B.C.) had demonstrated that our earth was a globe, and Eratosthenes (276–196 B.C.) had approximately determined its size. Hipparchus (190–120), the greatest astronomer of antiquity, the discoverer of the precession of the equinoxes, and the author of a catalogue of stars, had transferred to our earth the auxiliary lines drawn by him across the heavens. He had taught cartographers to lay down places according to their latitude and longitude, and how to project a sphere upon a plane. It is to him we are indebted for the stereographic and orthographic projections of the sphere. Ptolemy himself invented the tangential conical projection.

The gnomon or sun-dial, an instrument known to the Chinese 600 years before Christ, had long been used for the determination of latitudes, and the results were relatively correct, although uniformly subject to an error of 16 minutes, which was due to the observers taking the altitude of the upper limb of the sun, when measuring the shadow cast by their dial, instead of that of the sun's centre.

It was known, likewise, that differences of longitude could be determined by the simultaneous observation of eclipses of the sun or moon, or of occultations of stars, and Hipparchus actually calculated ephemerides for six years in advance to facilitate computations. Ptolemy himself suggested the use of lunar distances. But so imperfect were the astrolabes and other instruments used by the ancient astronomers, and especially their time-keepers, that precise results are quite out of the question.

Ptolemy, in fact, contented himself with accepting eight latitudes determined by actual observation, of which four were in Egypt, whilst of the three longitudes known to him he only utilized one in the construction of his map. Unfortunately, the one selected proved the least accurate, being erroneous to the extent of 32 per cent., whilst the error of the two which he rejected did not exceed 13 per cent.[1] This want of judgment—pardonable, no doubt, under the circumstances—vitiated Ptolemy's delineation of the Mediterranean to a most deplorable extent, far more so than did his assumption that a degree only measured five hundred stades, when in reality it measures six hundred. For whilst the breadth of his Mediterranean, being dependent upon the relatively correct latitudes of Alexandria, Rhodes, Rome, and Massilia, fairly approximates the truth, its length is exaggerated to the extent of nearly 50 per cent., measuring 62° instead of 41° 40′. This capital error of Ptolemy is due therefore to the unfortunate acceptance of an incorrect longitude, quite as much as to an exaggeration of itinerary distances. It is probable that Ptolemy would have presented us with a fairer likeness of our great inland sea had he rejected observed latitudes and longitudes altogether, and trusted exclusively to itineraries and to such bearings as the mariners of the period could have supplied him with.

No copy of Ptolemy's original set of maps has reached us, for the maps drawn by Agathodæmon in the fifth century are, under the most favourable circumstances, merely reductions of Ptolemy's originals, or they are compiled from Ptolemy's "Geography," which, apart from a few explanatory chapters, consists almost wholly of lists of places, with their latitudes and longitudes. I am almost inclined to adopt the latter view—firstly, because of the very crude delineation of Egypt, for which country an accurate cadastral survey was available; and secondly, on account of the cylindrical projection on which these maps are drawn, although from Ptolemy's own statements we are justified in believing that he made use of a conical projection in the construction of his maps.

An examination of Ptolemy's maps shows very clearly that they were almost wholly compiled from itineraries, the greater number of which their author borrowed from his predecessor Marinus. It shows, too, that Ptolemy's critical acumen as a compiler cannot be rated very high, and that he failed to utilize much information of a geographical nature which was available in his day. His great merit consisted in having taught cartographers to construct their maps according to a scientific method. This lesson, however, they were slow to learn, and centuries elapsed before they once more advanced along the only correct path, which Ptolemy had been the first to tread.

During the "Dark Ages" which followed the dismemberment of the Roman Empire there was no lack of maps, but they were utterly worthless from a scientific point of view. The achievements of the ancients were ignored, and the principal aim of the map-makers of the period appears to have been to reconcile their handiwork with the orthodox interpretation of the Holy Scriptures. Hence those numerous "wheel maps," upon which Jerusalem is made to represent the hub, whilst the western half of the disk is assigned to Europe and Africa, and the eastern to Asia.

As it is not my intention to introduce you to the archæological curiosities of an uncritical age, but to give you some idea of the progress of cartography, I at once pass on to the Arabs.

The Arabs were great as travellers, greater still as astronomers, but contemptible as cartographers. Their astronomers, fully possessed of the knowledge of Ptolemy, discovered the error of the gnomon; they improved the instruments which they had inherited from the ancients, and carefully fixed the latitudes of quite a number of places. Zarkala, the Director of the Observatory of Toledo, even attempted to determine the difference of longitude between that place and Bagdad; and if his result differed to the extent of 3° from the truth, it nevertheless proved a great advance upon Ptolemy, whose map exhibits an error amounting to 18°. Had there existed a scientific cartographer among the Arabs, he would have been able, with the aid of these observations, and of the estimates of distances made by careful observers like Abul Hasan, to effect most material corrections in the map of the known world. If Edrisi's map (1154) is better than that of others of his Arab contemporaries, this is simply due to his residence at Palermo, where he was able to avail himself of the knowledge of the Italians.

Quite a new epoch in the history of cartography begins with the introduction of the magnetic needle into Europe. Hitherto the seaman had governed his course by the observation of the heavens; thenceforth an instrument was placed in his hands which made him independent of the state of the sky. The property of the magnet or "loadstone" to point to the north first became known in the eleventh century, and in the time of Alexander Neckam (1185) it was already poised upon a pivot. It was, however, only after Flavio Gioja of Amalfi (1302) had attached to it a compass-card, exhibiting the direction of the winds, that it became of such immediate importance to the mariner. It is only natural that the Italians, who were the foremost seamen of that age, should have been the first to avail themselves of this new help to navigation. At quite an early date, as early probably as the twelfth century, they made use of it for their maritime surveys, and in course of time they produced a series of charts upon which the coasts frequented by them, from the recesses of the Black Sea to the mouth of the Rhine, are delineated for the first time with suprising fidelity to nature. The appearance of these so-called compass-charts, with gaily coloured roses of the winds and a bewildering number of rhumb-lines, is quite unmistakable. A little consideration will show you that if the variation of the compass had been taken into account in the construction of these charts, they would actually have developed into a picture of the world on Mercator's projection. But to deny them all scientific value because they do not fulfil this condition, is going too far. As correct delineations of the contours of the land they were a great advance upon Ptolemy's maps, and it redounds little to the credit of the "learned" geographers of a later time that they rejected the information so laboriously collected and skilfully combined by the chart makers, and returned to the deformities of Ptolemy. The adjustment of these charts to positions ascertained by astronomical observations could have been easily effected. An inspection of my diagrams

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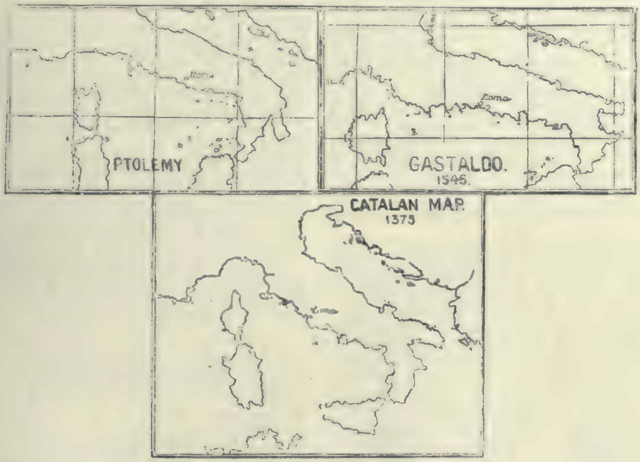
[1] The three longitudes are the following:—

	Result of ancient observations.		Adopted by Ptolemy.		Actual difference of longitude.
Arbela ...	45° E. of Carthage	...	45°	...	34°
Babylon ...	12° 30′ E. of Alexandria	...	18° 30′	...	14° 18′
Rome ...	20° E. of Alexandria	...	23° 50′	...	17° 24′

will prove this to you. The delineation of Italy, on the so-called Catalan map, is surprisingly correct; whilst Gastaldo, whose map of Italy is nearly two hundred years younger, has not yet been able to emancipate himself from the overpowering authority of Ptolemy. And in this he did not sin alone, for Italian and other cartographers of a much later time still clung pertinaciously to the same error.

There were others, however, who recognized the value of these charts, and embodied them in maps of the entire world. Among such were Marino Sanuto (1320) and Fra Mauro (1453), both of whom made their maps the repository of much information gathered from the Arabs or from their own countrymen who had seen foreign parts. Fra Mauro, more especially has transmitted to us a picture of Abyssinia marvellously correct in its details, though grossly exaggerated in its dimensions.

Another step in the right direction was taken when the cartographers and pilots of Portugal and Spain returned to the crude projection of Dicæarch, Eratosthenes, and Marinus, which enabled them to lay down places according to latitude and longitude upon their "plane charts."



Germany, debarred from taking a share in the great maritime discoveries of the age, indirectly contributed to their success by improvements in mathematical geography and the introduction of superior instruments. The navigators of the early middle ages still made use of an astrolabe when they desired to determine a latitude, but this instrument, which in the hands of an expert observer furnished excellent results on land, was of little use to a pilot stationed on the unsteady deck of a vessel. Regiomontanus consequently conferred an immense service upon the mariners of his time when, in 1471, he adapted to their use an instrument already known to the ordinary surveyors. It was this cross-staff which Martin Behaim introduced into the Portuguese navy, and which quickly made its way among the navigators of all countries. Most observations at sea were made with this simple instrument, variously modified in the course of ages, until it was superseded by Hadley's sextant. In the hands of the more skilful navigators of the seventeenth century, such as Baffin, James, and Tasman, the results obtained with the cross-staff were correct within two or three minutes.

Far greater difficulties were experienced in the observations of longitudes. Lunar eclipses were most generally made use of, but neither the ephemerides of Regiomontanus, for the years 1474 to 1506, which Columbus carried with him on his voyages, nor those of Peter Apianus, for 1521–70, were sufficiently accurate to admit of satisfactory results, even though the actual observation left nothing to be desired. Errors of 30° in longitude were by no means rare, and it was only when Kepler had published his "Rudolphine Tables" (1626), which according to Lalande formed the basis of all astronomical calculations during a century, that more exact results were obtained. The suggestion to determine longitude by means of lunar distances or occultations of stars bore no fruit at that time, as the knowledge of the complicated motion of the moon was still very imperfect. Still less was known about the movements of the satellites of Jupiter, which Galileo had first espied in 1610 when looking at that planet through his telescope. They became available only after tables of their revolutions and eclipses had been published by Cassini in 1668.

Another suggestion for the determination of longitude was made by Gemma Frisius in 1530—namely, that a clock or timekeeper should be employed for the purpose. One of Huygens's pendulum clocks was actually carried by Holmes to the Gulf of Guinea, but the results obtained were far from encouraging.

The difficulties which still attended the determination of longitude in the sixteenth century are conspicuously illustrated by the abortive attempts of a Congress of Spanish and Portuguese navigators who met at Badajoz and Yelves in 1524 for the purpose of laying down the boundary line, which Pope Alexander VI. had drawn at a distance of 370 Spanish leagues to the west of Cape Verde Islands, to separate the dominions of Spain from those of Portugal. Not being able to agree either as to the length of a degree, nor even as to that of a league, they separated without settling the question placed before them.

So uncertain were the results of observations for longitude made during the sixteenth and seventeenth centuries, that it was thought advisable to trust to the results of dead-reckoning rather than to those of celestial observations. But the method of dead-reckoning is available only when we have a knowledge of the size of the earth, and this knowledge was still very imperfect, notwithstanding the renewed measurement of an arc of the meridian by Snellius, the Dutch mathematician (1615). This measurement, however, is remarkable on account of its having for the first time applied the exact method of triangulation to a survey.

The problem of measuring the ship's way had been attempted by the Romans, who dragged paddle-wheels behind their ships, the revolutions of which enabled them to estimate the distance which the ship had travelled. But time, the strength of the wind, and the pilot's knowledge of the qualities of his ship, still constituted the principal elements for calculations of this kind, for the "catena a poppa" (which Magellan attached to the stern of his ship was merely intended to indicate the ship's leeway, and not the distance which it had travelled. The log, which for the first time enabled the mariner to carry out his dead-reckoning with confidence, is first described in Bourne's "Regiment for the Sea," which was published in 1577.

The eminent position which Italian cartographers occupied during the fourteenth and fifteenth centuries had to be surrendered by them, in the beginning of the sixteenth, to their pupils, the Portuguese and Spaniards, upon whom extensive voyages and discoveries had conferred exceptional advantages. These, in turn, had to yield to the Germans, and later on to the Dutch, who were specially qualified to become the reformers of cartography by their study of mathematics and of the ancient geographers, as also by the high degree of perfection which the arts of engraving on wood and copper had attained among them. German mathematicians first ventured to introduce the long-neglected geographical projections of Hipparchus and Ptolemy, and devised others of their own. Werner of Nürnberg (1514) invented an equivalent heart-shaped projection, whilst both Apianus and Staben (1520 and 1522) suggested equivalent projections. Still greater were the services of Gerhard Cremer, or Mercator (1512–94), the Ptolemy of the sixteenth century, who not only introduced the secant conical projection, but also invented that still known by his name, which was calculated to render such great service to the navigator, but was nevertheless not universally accepted until the middle of the fifteenth century, when the mediæval compass and plane charts finally disappeared.

The German cartographers of that age are to be commended, not because they copied Ptolemy's maps—for in this they had been preceded by others—but because they adopted his scientific methods in producing maps of their own. Their reforms began at home, as all reforms should. They were amply supported in their efforts by the many astronomers of note of whom Germany then boasted, and by quite a staff of local "geographers," of whom nearly every district of the empire boasted the possession of one. Among these local maps, that of Bavaria, by Philipp Bienewitz, or Apianus (1566), holds a distinguished rank, for it is the first map on a large scale (1 : 144,000) based upon a regular survey. Its errors in latitude do not exceed 1′, and those in longitude 3′, which is marvellously correct considering the age of its production. Like most maps of the period, it is engraved on wood, for though the art of engraving on copper was invented in Germany before 1446, and the first map was engraved there in 1450, copper engraving only became general at a much later date.

Perhaps the earliest general map of Germany, and certainly one of the most interesting, was that which the famous Cardinal Nicolas of Cues or Cusa completed in 1464, the only existing copy of which is to be found in the British Museum, where it was "discovered" by Baron Nordenskiöld. Mercator's map of Germany, published more than a century after that of the learned Cardinal (in 1585), was naturally far more complete in all respects, and was certainly far superior to the maps of any other country existing at that time. This fact is brought home to us by an inspection of a collection of maps to be found in the well-known "Theatrum Orbis" of Ortelius (first published in 1570), where we may see that the maps supplied by Humphrey Lloyd and other British cartographers are still without degree lines.

But when we follow Mercator, or, in fact, any other cartographer of the period, into regions the successful delineation of which depended upon an intelligent interpretation of itineraries and of other information collected by travellers, they are found to fail utterly. Nowhere is this utter absence of the critical faculty more glaringly exhibited than in the maps of Africa of that period.

Among the Dutch cartographers of that age one of the foremost places must be accorded to Waghenaer of Enkhuisen, whose "Mirror of the Sea," a collection of charts published in 1583, enjoyed a considerable reputation among British seamen. Other famous Dutch publishers of charts were Ortelius, Janssen, Blaeuw, and Vischer, who accumulated large stocks of copper plates, which constituted valuable heirlooms, and, not unlike the plates of certain modern map-publishers, supplied edition after edition without undergoing any change, except perhaps that of the date.

The age of great discoveries was past. All blanks upon our maps had not yet been filled up, but the contours of the great continents stood out distinctly, and in the main correctly. Discoveries on a large scale had become impossible, except in the Polar regions and in the interior of some of the continents; but greater preciseness had to be given to the work already done, and many details remained to be filled in. In this "age of measurements," as Peschel significantly calls it, better instruments, and methods of observation superior to those which had sufficed hitherto, were needed, and were readily forthcoming.

Picard, by making use of the telescope in measuring angles (1667), obtained results of a degree of accuracy formerly quite unattainable, even with instruments of huge proportions. For the theodolite, that most generally useful surveying instrument, we are indebted to Jonathan Sission (1737 or earlier). More important still, at all events to the mariner, was the invention of the sextant, generally ascribed to Hadley (1731), but in reality due to the genius of Newton. Equally important was the production of a trustworthy chronometer by John Harrison (1761), which first made possible the determination of meridian distances, and is invaluable whenever a correct knowledge of the time is required. One other instrument, quite recently added to the apparatus of the surveyor, is the photographic camera, converted for his especial benefit into a photogrammeter. This instrument has not as yet been utilized for ascertaining the relative positions of celestial bodies, but has already done excellent service in ordinary surveying, especially when it is required to portray the sides of inaccessible mountains.

But the full fruits of these inventions could be enjoyed only after Bradley had discovered the aberration of light (1728) and the nutation of the earth's axis (1747); Domenique Cassini had furnished trustworthy tables of the refraction of light; and the complicated movement of the moon had been computed by Euler (1746), Tobias Mayer (1753), Bradley (1770), and, more recently, by Hansen.

Positively novel methods for determining the latitude and longitude of a place can scarcely be said to have been proposed during this period, but many of the older methods only became really available after the improvements in the instruments indicated above had taken place, and the computations had been freed from the errors which vitiated them formerly.

Real progress, however, has been made in the determination of altitudes. Formerly they could be ascertained only by trigonometrical measurement, or by a laborious process of levelling, but since physicists have shown how the decrease of atmospheric pressure with the altitude, and the boiling-point of water depending upon this decrease, afforded a ready means of determining heights, the barometer, aneroid, and boiling-point thermometer have become the indispensable companions of the explorer, and our knowledge of the relief of the land has advanced rapidly.

Equally rapid have been the improvements in our instruments for measuring the depth of the ocean, since a knowledge of the configuration of its bed was demanded by the practical requirements of the telegraph engineers.

And in proportion as the labours of the surveyors and explorers gained in preciseness, so did the cartographer of the age succeed in presenting the results achieved in a manner far more satisfactory than had been done by his predecessors. His task was comparatively easy so long as he only dealt with horizontal dimensions, though even in the representation of these a certain amount of skill and judgment are required to make each feature tell in proportion to its relative importance. The delineation of the inequalities of the earth's surface, however, presented far greater difficulties. The mole-hills or serrated ridges, which had not yet quite disappeared from our maps in the beginning of this century, failed altogether in doing justice to our actual knowledge. The first timid attempt to represent hills as seen from a bird's-eye view, and of shading them according to the steepness of their slopes, appear on a map of the Breisgau, published by Homann in 1718. We find this system fully developed on La Condamine's map of Quito, published in 1751, and it was subsequently popularized by Arrowsmith. In this crude system of hill shading, however, everything was left to the judgment of the draughtsman, and only after Lehmann (1783) had superimposed it upon a groundwork of contours, and had regulated the strength of the hatching in accordance with the degree of declivity to be represented did it become capable of conveying a correct idea of the configuration of the ground.

The first to fully recognize the great importance of contours was Philip Buache, who had prepared a contoured map of the Channel in 1737, and suggested that the same system might profitably be extended to a delineation of the relief of the land; and this idea, subsequently taken up by Ducarla of Vabres, was for the first time carried into practice by Dupain-Triel, who published a contoured map of France in 1791. Up to the present time more than eighty methods of showing the hills have been advocated, but it may safely be asserted that none of these methods can be mathematically correct unless it is based upon horizontal contours.

The credit of having done most towards the promotion of cartography in the course of the eighteenth century belongs to France. It was France which first equipped expeditions to determine the size of the earth; France which produced the first topographical map based upon scientific survey—a work begun by César François Cassini in 1744, and completed by his son five years after his father's death; it was France, again, which gave birth to D'Anville, the first critical cartographer whom the world had ever seen.

Delisle (1675–1726), a pupil of Cassini's, had already been able to rectify the maps of the period by utilizing the many astronomical observations which French travellers had brought home from all parts of the world. This work of reform was carried further by D'Anville (1697–1782), who swept away the fanciful lakes from off the face of Africa, thus forcibly bringing home to us the poverty of our knowledge; who boldly refused to believe in the existence of an Antarctic continent covering half the southern hemisphere, and always brought sound judgment to bear upon the materials which the ever-increasing number of travellers placed at his disposal. And whilst France led the way, England did not lag far behind.

In that country the discoveries of Cook and of other famous navigators, and the spread of British power in India, gave the first impulse to a more diligent cultivation of the art of representing the surface of the earth on maps. There, to a greater extent than on the Continent, the necessities of the navigator called into existence a vast number of charts, amongst which are many hundreds of sheets published by Dalrymple and Joseph Desbarres (1776). Faden, one of the most prolific publishers of maps, won distinction, especially for his county maps, several of which, like that of Surrey by Linley and Gardner, are based upon trigonometrical surveys carried on by private individuals. England was the first to follow the lead of France in undertaking a regular topographical survey (1785). Nor did she lack critical cartographers. James Rennell (born 1742) sagaciously arranged the vast mass of important information collected by British travellers in India and Africa; but it is chiefly the name of Aaron Arrowsmith (died 1823) with which the glory of the older school of English cartographers is most intimately connected. Arrowsmith became the founder of a family of geographers, whose representative in the third generation, up to the date of his death in 1873, worthily upheld the ancient reputation of the family. Another name which deserves to be gratefully remembered is that of John Walker, to whom the charts published by our Admiralty are indebted for that perspicuous, firm, and yet artistic execution which, whilst it enhances their scientific value, also facilitates their use by the mariner.

Since the beginning of the present century Germany has once more become the head-quarters of scientific cartography; and this is due as much to the inspiriting teachings of a Ritter and a Humboldt as to the general culture and scientific training, combined with technical skill, commanded by the men who more especially devoted themselves to this branch of geography, which elsewhere was too frequently allowed to fall into the hands of mere mechanics. Men like Berghaus, Henry Kiepert, and Petermann, the best known pupil of the first of these, must always occupy a foremost place in the history of our department of knowledge. Berghaus, who may be truly described as the founder of the modern school of cartography, and who worked under the immediate inspiration of a Ritter and a Humboldt, presented us with the first comprehensive collection of physical maps (1837). Single maps of this kind had, no doubt, been published before—Kircher (1665) had produced a map of the ocean currents, Edmund Halley (1686) had embodied the results of his own researches in maps of the winds and of the variation of the compass (1686), whilst Ritter himself had compiled a set of physical maps (1866)—but no work of the magnitude of Berghaus's famous "Physical Atlas" had seen the light before. Nor could it have been published even then had it not been for the unstinted support of a firm like that of Justus Perthes, already the publisher of Stieler's "Atlas" (1817–23), and subsequently of many other works which have carried its fame into every quarter of the globe.

And now, at the close of this nineteenth century, we may fairly boast that the combined science and skill of surveyors and cartographers, aided as they are by the great advance of the graphic arts, are fully equal to the production of a map which shall be a faithful image of the earth's surface. Let us imagine for one moment that an ideal map of this kind were before us, a map exhibiting not merely the features of the land and the depth of the sea, but also the extent of forests and of pasture-lands, the distribution of human habitations, and all those features the representation of which has become familiar to us through physical and statistical atlases. Let us then analyze the vast mass of facts thus placed before us, and we shall find that they form quite naturally two well-defined divisions—namely, those of physical and political geography—whilst the third department of our science, mathematical geography, deals with the measurement and survey of our earth, the ultimate outcome of which is the production of a perfect map.

I shall abstain from giving a laboured definition of what I consider geography should embrace, for definitions of this kind help practical workers but little, and will never deter anyone who feels disposed and capable from straying into fields which an abuse of logic has clearly demonstrated to lie outside his proper domain. But I wish to enforce the fact that topography and chorography, the description of particular places or of entire countries, should always be looked upon as integral portions of geographical research. It is they which furnish many of the blocks needed to rear our geographical edifice, and which constitute the best training school for the education of practical geographers, as distinguished from mere theorists.

That our maps, however elaborate, should be supplemented by descriptions will not even be gainsaid by those who are most reluctant to grant us our independent existence among the sciences which deal with the earth and man who inhabits it. This concession, however, can never content us. We cannot allow ourselves to be reduced to the position of collectors of facts. We claim the right to discuss ourselves the facts we have collected, to analyze them, to generalize from them, and to trace the correlations between cause and effect. It is thus that geography becomes comparative; and whilst comparative physical geography, or morphology, seeks to explain the origin of the existing surface features of our earth, comparative political geography, or anthropo-geography, as it is called by Dr. Ratzel, one of the most gifted representatives of geographical science in Germany, deals with man in relation to the geographical conditions which influence him. It is this department of geography which was so fruitfully cultivated by Karl Ritter.

Man is indeed in a large measure "the creature of his environment," for who can doubt for a moment that geographical conditions have largely influenced the destinies of nations, have directed the builders of our towns, determined the paths of migrations and the march of armies, and have impressed their stamp even upon the character of those who have been subjected to them for a sufficiently extended period? . . . .

It must not, however, be assumed for one moment that the dependence of man upon Nature is absolute. The natural resources of a country require for their full development a people of energy and capacity; and instances in which they have been allowed to lie dormant, or have been wasted, are numerous. . . .

Perhaps one of the most instructive illustrations of the complex human agencies which tend to modify the relative importance of geographical conditions is presented to us by the Mediterranean. The time when this inland sea was the centre of civilization and of the world's commerce, whilst the shores of Western Europe were only occasionally visited by venturesome navigators or conquering Roman hosts, does not lie so very far behind us. England, at that period, turned her face towards Continental Europe, of which it was a mere dependency. The prosperity of the Mediterranean countries survived far into the middle ages, and Italy at one time enjoyed the enviable position of being the great distributor of the products of the East, which found their way across the Alps into Germany, and through the gates of Gibraltar to the exterior ocean. But a change was brought about, partly through the closing of the old Oriental trade routes, consequent upon the conquests of the Turks, partly through the discovery of a new world and of a maritime highway to India. When Columbus, himself an Italian, returned from the West Indies in 1493, and Vasco da Gama brought the first cargo of spices from India in 1499, the star of Italy began to fade. And whilst the spices of the Indies and the gold of Guinea poured wealth into the lap of Portugal, and Spain grew opulent on the silver mines of Mexico and Peru, Venice was vainly beseeching the Sultan to reopen the old trade route through the Red Sea. The dominion of the sea had passed from Italy to Spain and Portugal, and passed later on to the Dutch and English. But mark how the great geographical discoveries of that age affected the relative geographical position of England! England no longer lay on the skirts of the habitable world, it had become its very centre. And this natural advantage was enhanced by the colonial policies of Spain and Portugal, who exhausted their strength in a task far beyond their powers, took possession of tropical countries only, and abandoned to England the less attractive but in reality far more valuable regions of North America. England was thus enabled to become the founder of real colonies, the mother of nations: and her language, customs, and political institutions found a home in a new world.

And now, when the old highway through the Red Sea has been reopened, when the wealth flowing through the Canal of Suez is beginning to revivify the commerce of Italy, England may comfort herself with the thought that in her own colonies and in the States which have sprung up across the Atlantic she may find ample compensation for any possible loss that may accrue to her through geographical advantages being once more allowed to have full play.

I am afraid I have unduly tried your patience. I believe you will agree with me that no single individual can be expected to master all those departments which are embraced within the wide field of geography. Even the master-mind of a Humboldt fell short of this, and facts have accumulated since his time at an appalling rate. All that can be expected of our modern geographer is that he should command a comprehensive general view of his field, and that he should devote his energies and capacities to the thorough cultivation of one or more departments that lie within it.

SECTION H.

ANTHROPOLOGY.

Opening Address by Prof. F. Max Müller, President of the Section.

It was forty-four years ago that for the first and for the last time I was able to take an active part in the meetings of the British Association for the Advancement of Science. It was at Oxford, in 1847, when I read a paper on the "Relation of Bengali to the Aryan and Aboriginal Languages of India," which received the honour of being published in full in the Transactions of the Association for that year. I have often regretted that absence from England and pressure of work have prevented me year after year from participating in the meetings of the Association. But, being a citizen of two countries—of Germany by birth, of England by adoption—my long vacations have generally drawn me away to the Continent, so that to my great regret I found myself precluded from sharing either in your labours or in your delightful social gatherings.

I wonder whether any of those who were present at that brilliant meeting at Oxford in 1847 are present here to-day. I almost doubt it. Our President then was Sir Robert Inglis, who will always be known in the annals of English history as having been preferred to Sir Robert Peel as Member of Parliament for the University of Oxford. Among other celebrities of the day I remember Sir Roderick Murchison, Sir David Brewster, Dean Buckland, Sir Charles Lyell, Prof. Sedgwick, Prof. Owen, and many more—a galaxy of stars, all set or setting. Young Mr. Ruskin acted as Secretary to the Geological Section. Our Section was then not even recognized as yet as a Section. We ranked as a sub-Section only of Section D, *Zoology* and *Botany*. We remained in that subordinate position till 1851, when we became Section E, under the name of *Geography* and *Ethnology*. From 1869, however, Ethnology seems almost to have disappeared again, being absorbed in Geography, and it was not till the year 1884 that we emerged once more as what we are to-day, Section H, or *Anthropology*.

In the year 1847 our sub-Section was presided over by Prof. Wilson, the famous Sanskrit scholar. The most active debaters, so far as I remember, were Dr. Prichard, Dr. Latham, and Mr. Crawfurd, well known then under the name of the Objector-General. I was invited to join the meeting by Bunsen, then Prussian Minister in London, who also brought with him his friend Dr. Karl Meyer, the Celtic scholar. Prince Albert was present at our debates, so was Prince Louis Lucien Bonaparte. Our Ethnological sub-Section was then most popular, and attracted very large audiences.

When looking once more through the debates carried on in our Section in 1847, I was very much surprised when I saw how very like the questions which occupy us to-day are to those which we discussed in 1847. I do not mean to say that there has been no advance in our science. Far from it. The advance of linguistic, ethnological, anthropological, and biological studies, all of which claim a hearing in our Section, has been most rapid. Still that advance has been steady and sustained; there has been no cataclysm, no deluge, no break in the advancement of our science, and nothing seems to me to prove its healthy growth more clearly than this uninterrupted continuity, which unites the past with the present, and will, I hope, unite the present with the future.

No paper is in that respect more interesting to read than the address which Bunsen prepared for the meeting in 1847, and which you will find in the Transactions of that year. Its title is "On the Results of the recent Egyptian Researches in reference to Asiatic and African Ethnology, and the Classification of Languages." But you will find in it a great deal more than what this title would lead you to expect.

There are passages in it which are truly prophetic, and which show that, if prophecy is possible anywhere, it is possible, nay, it ought to be possible, in the temple of Science, and under the inspiring influence of knowledge and love of truth.

Allow me to dwell for a little while on this remarkable paper. It is true, we have travelled so fast that Bunsen seems almost to belong to ancient history. This very year is the hundredth anniversary of his birth, and this very day the centenary of his birth is being celebrated in several towns of Germany. In England also his memory should not be forgotten. No one, not being an Englishman by birth, could, I believe, have loved this country more warmly, and could have worked more heartily than Bunsen did to bring about that friendship between England and Germany which must for ever remain the corner-stone of the peace of Europe, and the *sine quâ non* of that advancement of science to which our Association is devoted. His house in Carlton Terrace was a true international academy, open to all who had something to say, something worth listening to, a kind of sanctuary against vulgarity in high places, a neutral ground where the best representatives of all countries were welcome and felt at home. But this also belongs to ancient history. And yet, when we read Bunsen's paper, delivered in 1847, it does not read like ancient history. It deals with the problems which are still in the foreground, and if it could be delivered again to-day by that genial representative of German learning, it would rouse the same interest, provoke the same applause, and possibly the same opposition also, which it roused nearly half a century ago. Let me give you a few instances of what I mean.

We must remember that Darwin's "Origin of Species" was published in 1859, his "Descent of Man" in 1871. But here in the year 1847 one of the burning questions which Bunsen discusses is the question of the possible descent of man from some unknown animal. He traces the history of that question back to Frederick the Great, and quotes his memorable answer to D'Alembert. Frederick the Great, you know, was not disturbed by any qualms of orthodoxy. "In my kingdom," he used to say, "everybody may save his soul according to his own fashion." But when D'Alembert wished him to make what he called the *salto mortale* from monkey to man, Frederick the Great protested. He saw what many have seen since, that there is no possible transition from reasonlessness to reason, and that with all the likeness of their bodily organs there is a barrier which no animal can clear, or which, at all events, no animal has as yet cleared. And what does Bunsen himself consider the real barrier between man and beast? "It is language," he says, "which is unattainable, or, at least, unattained, by any animal except man." In answer to the argument that, given only a sufficient number of years, a transition by imperceptible degrees from animal cries to articulate language is at least conceivable, he says:—"Those who hold that opinion have never been able to show the possibility of the first step. They attempt to veil their inability by the easy but fruitless assumption of an infinite space of time, destined to explain the gradual development of animals into men; as if millions of years could supply the want of the agent necessary for the first movement, for the first step, in the line of progress! No numbers can effect a logical impossibility.

How, indeed, could reason spring out of a state which is destitute of reason? How can speech, the expression of thought, develop itself, in a year, or in millions of years, out of inarticulate sounds, which express feelings of pleasure, pain, and appetite?"

He then appeals to Wilhelm von Humboldt, whom he truly calls the greatest and most acute anatomist of almost all human speech. Humboldt goes so far as to say:—"Rather than assign to all language a uniform and mechanical march that would lead them step by step from the grossest beginnings to their highest perfection, I should embrace the opinion of those who ascribe the origin of language to an immediate revelation of the Deity. They recognize at least that divine spark which shines through all idioms, even the most imperfect and the least cultivated."

Bunsen then sums up by saying: "To reproduce Monboddo's theory in our days, after Kant and his followers, is a sorry anachronism, and I therefore regret that so low a view should have been taken of the subject lately in an English work of much correct and comprehensive reflection and research respecting natural science." This remark refers, of course, to the "Vestiges of Creation" (see an article in the *Edinburgh Review*, July, 1845), which was then producing the same commotion which Darwin's "Origin of Species" produced in 1859.

Bunsen was by no means unaware that in the vocal expression of feelings, whether of joy or pain, and in the imitation of external sounds, animals are on a level with man. "I believe with Kant," he says, "that the formation of ideas or notions, embodied in words, presupposes the action of the senses and impressions made by outward objects on the mind. But," he adds, "what enables us to see the genus in the individual, the whole in the many, and to form a word by connecting a subject with a predicate, is the power of the mind, and of this the brute creation exhibits no trace."

You know how for a time, and chiefly owing to Darwin's predominating influence, every conceivable effort was made to reduce the distance which language places between man and beast, and to treat language as a vanishing line in the mental evolution of animal and man. It required some courage at times to stand up against the authority of Darwin, but at present all serious thinkers agree, I believe, with Bunsen, that no animal has developed what we mean by rational language, as distinct from mere utterances of pleasure or pain, from imitation of sounds and from communication by means of various signs, a subject that has lately been treated with great fullness by my learned friend Prof. Romanes in his "Mental Evolution of Man." Still, if all true science is based on facts, the fact remains that no animal has ever formed what we mean by a language; and we are fully justified, therefore, in holding with Bunsen and Humboldt, as against Darwin and Prof. Romanes, that there *is* a specific difference between the human animal and all other animals, and that that difference consists in language as the outward manifestation of what the Greeks meant by *Logos*.

Another question which occupies the attention of our leading anthropologists is the proper use to be made of the languages, customs, laws, and religious ideas of so-called savages. Some, as you know, look upon these modern savages as representing human nature in its most primitive state, while others treat them as representing the lowest degeneracy into which human nature may sink. Here, too, we have learnt to distinguish. We know that certain races have had a very slow development, and may, therefore, have preserved some traces of those simple institutions which are supposed to be characteristic of primitive life. But we also know that other races have degenerated and are degenerating even now. If we hold that the human race forms but one species, we cannot, of course, admit that the ancestors even of the most savage tribes, say of the Australians, came into the world one day later than the ancestors of the Greeks, or that they passed through fewer evolutions than their more favoured brethren. The whole of humanity would be of exactly the same age. But we know its history from a time only when it had probably passed already through many ups and downs. To suppose, therefore, that the modern savage is the nearest approach to primitive man would be against all the rules of reasoning. Because in some countries, and under stress of unfavourable influences, some human tribes have learnt to feed on human flesh, it does not follow that our first ancestors were cannibals. And here, too, Bunsen's words have become so strikingly true that I may be allowed to quote them: "The savage is justly disclaimed as the prototype of natural, original man; for linguistic inquiry shows that the languages of savages are degraded and decaying fragments of nobler formations."

I know well that in unreservedly adopting Bunsen's opinion on this point also I run counter to the teaching of such well-known writers as Sir John Lubbock, Reclus, and others. It might be supposed that Mr. Herbert Spencer also looked upon savages as representing the primitive state of mankind. But if he ever did so, he certainly does so no longer, and there is nothing I admire so much in Mr. Herbert Spencer as this simple love of truth, which makes him confess openly whenever he has seen occasion to change his views. "What terms and what conceptions are truly primitive," he writes, "would be easy if we had an account of truly primitive men. But there are sundry reasons for suspecting that existing men of the lowest type forming social groups of the simplest kind do not exemplify men as they originally were. Probably most of them, if not all, had ancestors in a higher state" (*Open Court*, No. 205, p. 2896).

Most important also is a hint which Bunsen gives that the students of language should follow the same method which has been followed with so much success in geology; that they should begin with studying the modern strata of speech, and then apply the principles, discovered there, to the lower or less accessible strata. It is true that the same suggestion had been made by Leibniz, but many suggestions are made and are forgotten again, and the merit of rediscovering an old truth is often as great as the discovery of a new truth. This is what Bunsen said: "In order to arrive at the law which we are endeavouring to find (the law of the development of language) let us first assume, as geology does, that the same principles which we see working in the (recent) development were also at work at the very beginning, modified in degree and in form, but essentially the same in kind." We know how fruitful this suggestion has proved, and how much light an accurate study of modern languages and of spoken dialects has thrown on some of the darkest problems of the science of language. But fifty years ago it was Sanskrit only, or Hebrew, or Chinese, that seemed to deserve the attention of the students of comparative philology. Still more important is Bunsen's next remark, that language begins with the sentence, and that in the beginning each word was a sentence in itself. This view also has found strong supporters at a later time—for instance, my friend Prof. Sayce—though at the time we are speaking of it was hardly thought of. I must here once more quote Bunsen's own words: "The supreme law of progress in all language shows itself to be the progress from the substantial isolated word, as an undeveloped expression of a whole sentence, towards such a construction of language as makes every single word subservient to the general idea of a sentence, and shapes, modifies, and dissolves it accordingly."

And again: "Every sound in language must originally have been significative of something. The unity of sound (the syllable, pure or consonantised) must therefore originally have corresponded to a unity of conscious plastic thought, and every thought must have had a real or substantial object of perception. . . . Every single word implies necessarily a complete proposition, consisting of subject, predicate, and copula."

This is a most pregnant remark. It shows as clearly as daylight the enormous difference there is between the mere utterance of the sound *Pah* and *Mah*, as a cry of pleasure or distress, and the pronunciation of the same syllable as a sentence, when *Pah* and *Mah* are meant for "This is *Pah*," "This is *Mah*"; or, after a still more characteristic advance of the human intellect, "This is *a Pah*," "This is *a Mah*," which is not very far from saying, "This man belongs to the class or genus of fathers."

Equally important is Bunsen's categorical statement that everything in language must have been originally significant, that everything formal must originally have been substantial. You know what a bone of contention this has been of late between what is called the old school and the new school of comparative philology. The old school maintained that every word consisted of a root and of certain derivative suffixes, prefixes, and infixes. The modern school maintained that there existed neither roots by themselves nor suffixes, prefixes, and infixes by themselves, and that the theory of agglutination—of gluing suffixes to roots—was absurd. The old school looked upon these suffixes as originally independent and significative words; the modern school declined to accept this view except in a few irrefragable instances. I think the more accurate reasoners are coming back to the opinion held by the old school, that all formal elements of language were originally substantial, and therefore significative; that they are the remnants of predicative or demonstrative words. It is true we cannot always prove this as clearly as in the case of such words as *hard-ship*, *wis-dom*, *man-hood*, where *hood* can be traced back to *hâd*, which in Anglo-Saxon exists as an independent word, meaning state or quality. Nor do we often find that a suffix like *mente*, in *claramente*, *clairement*, continues to exist by itself, as when we say in Spanish *clara, concisa y elegantemente*. It is perfectly true that the French, when they say that a hammer falls *lourdement*, or heavily, do not deliberately take the suffix *ment*—originally the Latin *mente*, "with a mind"—and glue it to their adjective *lourd*. Here the new school has done good service in showing the working of that instinct of analogy which is a most important element in the historical development of human speech. One compound was formed in which *mente* retained its own meaning; for instance, *forti mente*, "with a brave mind." But when this had come to mean *bravely*, and no more, the working of analogy began; and if *fortement*, from *fort*, could mean "bravely," then why not *lourdement*, from *lourd*, "heavily?" But in the end there is no escape from Bunsen's fundamental principle that everything in language was originally language—that is, was significative, was substantial, was material—before it became purely formal.

But it is not only with regard to these general problems that Bunsen has anticipated the verdict of our own time. Some of his answers to more special questions also show that he was right when many of his contemporaries, and even successors, were wrong. It has long been a question, for instance, whether the Armenian language belonged to the Iranic branch of the Aryan family, or whether it formed an independent branch, like Sanskrit, Persian, or Greek. Bunsen, in 1847, treated Armenian as a separate branch of Aryan speech; and that it is so was proved by Prof. Hübschmann in 1883.

Again, there has been a long controversy whether the language of the Afghans belonged to the Indic or the Iranic branch. Dr. Trumpp tried to show that it belonged, by certain peculiarities, to the Indic or Sanskritic branch. Prof. Darmesteter has proved but lately that it shares its most essential characteristics in common with Persian. Here, too, Bunsen guessed rightly—for I do not mean to say that it was more than a guess—when he stated that "Pushtu, the language of the Afghans, belongs to the Persian branch."

I hope you will forgive me for having detained you so long with a mere retrospect. I could not deny myself the satisfaction of paying this tribute of gratitude and respect to my departed friend, Baron Bunsen. To have known him belongs to the most cherished recollections of my life. But though I am myself an old man—much older than Bunsen was at our meeting in 1847—do not suppose that I came here as a mere *laudator temporis acti*. Certainly not. If one tries to recall what anthropology was in 1847, and then considers what it is now, its progress seems most marvellous. I do not think so much of the new materials which have been collected from all parts of the world. These last fifty years have been an age of discovery in Africa, in Central Asia, in America, in Polynesia, and in Australia, such as can hardly be matched in any previous century.

But what seems to me even more important than the mere increase of material is the new spirit in which anthropology has been studied during the last generation. I do not mean to depreciate the labours of so-called *dilettanti*. After all, *dilettanti* are lovers of knowledge, and in a study such as the study of anthropology the labours of these volunteers, or *franc-tireurs*, have often proved most valuable. But the study of man in every part of the world has ceased to be a subject for curiosity only. It has been raised to the dignity, but also to the responsibility, of a real science, and it is now guided by principles as strict and as rigorous as any other science—such as zoology, botany, mineralogy, and all the rest. Many theories which were very popular fifty years ago are now completely exploded; nay, some of the very principles by which our science was then guided have been discarded. Let me give you one instance—perhaps the most important one—as determining the right direction of anthropological studies.

At our meeting in 1847 it was taken for granted that the study of comparative philology would be in future the only safe foundation for the study of anthropology. Linguistic ethnology was a very favourite term used by Bunsen, Prichard, Latham, and others. It was, in fact, the chief purpose of Bunsen's paper to show that the whole of mankind could be classified according to language. I protested against this view at the time, and in 1853 I published my formal protest in a letter to Bunsen, "On the Turanian Languages." In a chapter called "Ethnology *versus* Phonology" I called, if not for a complete divorce, at least for a judicial separation between the study of philology and the study of ethnology. "Ethnological race," I said, "and phonological race are not commensurate, except in antehistorical times, or, perhaps, at the very dawn of history. With the migration of tribes, their wars, their colonies, their conquests and alliances, which, if we may judge from their effects, must have been much more violent in the ethnic than ever in the political periods of history, it is impossible to imagine that race and language should continue to run parallel. The physiologist should pursue his own science, unconcerned about language. Let him see how far the skulls, or the hair, or the colour, or the skin of different tribes admit of classification; but to the sound of their words his ear should be as deaf as that of the ornithologist's to the notes of caged birds. If his Caucasian class includes nations or individuals speaking Aryan (Greek), Turanian (Turkish), and Semitic (Hebrew) languages, it is not his fault. His system must not be altered to suit another system. There is a better solution both for his difficulties and for those of the phonologist than mutual compromise. The phonologist should collect his evidence, arrange his classes, divide and combine as if no Blumenbach had ever looked at skulls, as if no Camper had ever measured facial angles, as if no Owen had ever examined the basis of a cranium. His evidence is the evidence of language, and nothing else; this he must follow, even though in the teeth of history, physical or political. . . . There ought to be no compromise between ethnological and phonological science. It is only by stating the glaring contradictions between the two that truth can be elicited."

At first my protest met with no response; nay, curiously enough, I have often been supposed to be the strongest advocate of the theory which I so fiercely attacked. Perhaps I was not entirely without blame, for, having once delivered my soul, I allowed myself occasionally the freedom to speak of the Aryan or the Semitic race, meaning thereby no more than the people, whoever and whatever they were, who spoke Aryan or Semitic languages. I wish we could distinguish in English as in Hebrew between *nations* and *languages*. Thus in the Book of Daniel, iii. 4, "the herald cried aloud, . . . O people, nations, and languages." Why then should we not distinguish between nations and languages? But to put an end to every possible misunderstanding, I declared at last that to speak of "an Aryan skull would be as great a monstrosity as to speak of a dolichocephalic language."

I do not mean to say that this old heresy, which went by the name of linguistic ethnology, is at present entirely extinct. But among all serious students, whether physiologists or philologists, it is by this time recognized that the divorce between ethnology and philology, granted if only for incompatibility of temper, has been productive of nothing but good.

Instead of attempting to classify mankind as a whole, students are now engaged in classing skulls, in classing hair, and teeth, and skin. Many solid results have been secured by these special researches; but, as yet, no two classifications, based on these characteristics, have been made to run parallel.

The most natural classification is, no doubt, that according to the colour of the skin. This gives us a black, a brown, a yellow, a red, and a white race, with several subdivisions. This classification has often been despised as unscientific; but it may still turn out far more valuable than is at present supposed.

The next classification is that by the colour of the eyes, as black, brown, hazel, grey, and blue. This subject also has attracted much attention of late, and, within certain limits, the results have proved very valuable.

The most favourite classification, however, has always been that according to the skulls. The skull, as the shell of the brain, has by many students been supposed to betray something of the spiritual essence of man; and who can doubt that the general features of the skull, if taken in large averages, do correspond to the general features of human character? We have only to look round to see men with heads like a cannon-ball and others with heads like a hawk. This distinction has formed the foundation for a more scientific classification into *brachycephalic*, *dolichocephalic*, and *mesocephalic* skulls. The proportion of 80 : 100 between the transverse and longitudinal diameter gives us the ordinary or mesocephalic type, the proportion of 75 : 100 the dolichocephalic, the proportion of 85 : 100 the brachycephalic type. The extremes are 70 : 100 and 90 : 100.

If we examine any large collection of skulls, we have not much difficulty in arranging them under these three classes; but if, after we have done this, we look at the nationality of each skull, we find the most hopeless confusion. Pruner Bey, as Peschel tells us in his "Völkerkunde," has observed brachycephalic and dolichocephalic skulls in children born of the same mother; and if we consider how many women have been carried away into captivity by Mongolians in their inroads into China, India, and Germany, we cannot feel surprised if we find some longheads among the roundheads of those Central Asiatic hordes.

Only we must not adopt the easy expedient of certain anthropologists who, when they find dolichocephalic and brachycephalic skulls in the same tomb, at once jump to the conclusion that they must have belonged to two different races. When, for instance, two dolichocephalic and three brachycephalic skulls were discovered in the same tomb at Alexanderpol, we were told at once that this proved nothing as to the simultaneous occurrence of different skulls in the same family; nay, that it proved the very contrary of what it might seem to prove. It was clear, we were assured, that the two dolichocephalic skulls belonged to Aryan chiefs and the three brachycephalic skulls to their non-Aryan slaves, who were killed and buried with their masters, according to a custom well known to Herodotus. This sounds very learned, but is it really quite straightforward?

Besides the general division of skulls into dolichocephalic, brachycephalic, and mesocephalic, other divisions have been undertaken, according to the height of the skull, and, again, according to the maxillary and the facial angles. This latter division gives us *orthognathic*, *prognathic*, and *mesognathic* skulls.

Lastly, according to the peculiar character of the hair, we may distinguish two great divisions, the people with woolly hair (*Ulotriches*) and people with smooth hair (*Lissotriches*). The former are subdivided into *Lophocomi*, people with tufts of hair, and *Eriocomi*, or people with fleecy hair. The latter are divided into *Euthycomi*, straight-haired, and *Euplocami* (not *Euplocomic*, wavy-haired, as Brinton gives it), wavy-haired. It has been shown that these peculiarities of the hair depend on the peculiar form of the hair-tubes, which, in cross-sections, are found to be either round or elongated in different ways.

Now all these classifications, to which several more might be added, those according to the orbits of the eyes, the outlines of the nose, the width of the pelvis, are by themselves extremely useful. But few of them only, if any, run strictly parallel. It has been said that all dolichocephalic races are prognathic, and have woolly hair. I doubt whether this is true without exception; but, even if it were, it would not allow us to draw any genealogical conclusions from it, because there are certainly many dolichocephalic people who are not woolly-haired, as, for instance, the Eskimos (Brinton's "Races of People," p. 249).

Now, let us consider whether there can be any organic connection between the shape of the skull, the facial angle, the conformation of the hair, or the colour of the skin on one side, and what we call the great families of language on the other. That we speak at all may rightly be called a work of nature, *opera naturale*, as Dante said long ago; but that we speak thus or thus, *cosi o cosi*, that, as the same Dante said, depends on our pleasure—that is our work. To imagine, therefore, that as a matter of necessity, or as a matter of fact, dolichocephalic skulls have anything to do with Aryan, mesocephalic with Semitic, or brachycephalic with Turanian speech, is nothing but the wildest random thought; it can convey no rational meaning whatever. We might as well say that all painters are dolichocephalic, and all musicians brachycephalic, or that all lophocomic tribes work in gold, and all lissocomic tribes in silver.

If anything must be ascribed to prehistoric times, surely the differentiation of the human skull, the human hair, and the human skin, would have to be ascribed to that distant period. No one, I believe, has ever maintained that a mesocephalic skull was split or differentiated into a dolichocephalic and a brachycephalic variety in the bright sunshine of history.

But let us, for the sake of argument, assume that in prehistoric times all dolichocephalic people spoke Aryan, all mesocephalic, Semitic, all brachycephalic, Turanian languages: how would that help us?

So long as we know anything of the ancient Aryan, Semitic, and Turanian languages, we find foreign words in each of them. This proves a very close and historical contact between them. For instance, in Babylonian texts of 3000 B.C. there is the word *sindhu* for cloth made of vegetable fibres, linen. That can only be the Sk. *sindhu*, the Indus, or *saindhava* what comes from the Indus. It would be the same word as the Homeric *σινδών*, fine cloth ("Physical Religion," p. 87). In Egyptian we find so many Semitic words that it is difficult to say whether they were borrowed or derived from a common source. I confess I am not convinced, but Egyptologists of high authority assure us that the names of several Aryan peoples, such as the Sicilians and Sardinians, occur in the fourteenth century B.C., in the inscriptions of the time of Menephthah I. Again, as soon as we know anything of the Turanian languages—Finnish, for instance—we find them full of Aryan words. All this, it may be said, applies to a very recent period in the ancient history of humanity. Still, we have no access to earlier documents, and we may fairly say that this close contact which existed then existed, probably, at an earlier time also.

If, then, we have no reason to doubt that the ancestors of the people speaking Aryan, Semitic, and Turanian languages, lived in close proximity, would there not have been marriages between them so long as they lived in peace, and would they not have killed the men and carried off the women in time of war? What, then, would have been the effect of a marriage between a dolichocephalic mother and a brachycephalic father? The materials for studying this question of *metissage*, as the French call it, are too scanty as yet to enable us to speak with confidence. But whether the paternal or the maternal type prevailed, or whether their union gave rise to a new permanent variety, still it stands to reason that the children of a dolichocephalic captive woman might be found, after fifty or sixty years, speaking the language of the brachycephalic conquerors.

It has been the custom to speak of the early Aryan, Semitic, and Turanian races as large swarms—as millions pouring from one country into another. It has been calculated that these early nomads would have required immense tracts of meadow land to keep their flocks, and that it was the search for new pastures that drove them, by an irresistible force, over the whole inhabitable earth.

This may have been so, but it may also have not been so. Anyhow, we have a right to suppose that, before there were millions of human beings, there were at first a few only. We have been told of late that there never was a first man; but we may be allowed to suppose, at all events, that there were at one time a few first men and a few first women. If, then, the mixture of blood by marriage and the mixture of language in peace or war took place at that early time, when the world was peopled by some individuals, or by some hundreds, or by some thousands only, think what the necessary result would have been. It has been calculated that it would only require 600 years to populate the whole earth with the descendants of one couple, the first father being dolichocephalic and the first mother brachycephalic. They might, after a time, all choose to speak an Aryan language, but they could not choose their skulls, but would have to accept them from nature, whether dolichocephalic or brachycephalic.

Who, then, would dare at present to lift up a skull and say this skull must have spoken an Aryan language, or lift up a language and say this language must have been spoken by a dolichocephalic skull? Yet, though no serious student would any longer listen to such arguments, it takes a long time before theories that were maintained for a time by serious students, and were then surrendered by them, can be completely eradicated. I shall not touch to-day on the hackneyed question of the "home of the Aryans" except as a warning. There are two quite distinct questions concerning the home of the Aryans.

When students of philology speak of Aryans, they mean by Âryas nothing but people speaking an Aryan language. They affirm nothing about skulls, skins, hair, and all the rest. Ârya with them means speakers of an Aryan language. When, on the contrary, students of physiology speak of dolichocephalic, orthognathic, euthycomic people, they speak of their physiological characteristics only, and affirm nothing whatever about language.

It is clear, therefore, that the home of the Âryas, in the proper sense of that word, can be determined by linguistic evidence only, while the home of a blue-eyed, blond-haired, long-skulled, fair-skinned people can be determined by physiological evidence only. Any kind of concession or compromise on either side is simply fatal, and has led to nothing but a promiscuous slaughter of innocents. Separate the two armies, and the whole physiological evidence collected by D'Omalius d'Halloy, Latham, and their followers will not fill more than an octavo page; while the linguistic evidence collected by Benfey and his followers will not amount to more than a few words. Everything else is mere rhetoric.

The physiologist is grateful, no doubt, for any additional skull whose historical antecedents can be firmly established; the philologist is grateful for any additional word that can help to indicate the historical or geographical whereabouts of the unknown speakers of Aryan speech. On these points it is possible to argue. They alone have a really scientific value in the eyes of a scholar, because, if there is any difference of opinion on them, it is possible to come to an agreement. As soon, however, as we go beyond these mere matters of fact, which have been collected by real students, everything becomes at once mere vanity and vexation of spirit. I know the appeals that have been made for concessions and some kind of compromise between physiology and philology; but honest students know that on scientific subjects no compromise is admissible. With regard to the home of the Âryas, no honest philologist will allow himself to be driven one step beyond the statement that the unknown people who spoke Aryan languages were, at one time, and before their final separation, settled somewhere in Asia. That may seem very small comfort, but for the present it is all that we have a right to say. Even this must be taken with the limitations which, as all true scholars know, apply to speculations concerning what may have happened, say, five thousand or ten thousand years ago. As to the colour of the skin, the hair, the eyes of those unknown speakers of Aryan speech, the scholar says nothing; and when he speaks of their blood he knows that such a word can be taken in a metaphorical sense only. If we once step from the narrow domain of science into the vast wilderness of mere assertion, then it does not matter what we say. We may say, with Penka, that all Âryas are dolichocephalic, blue-eyed, and blond, or we may say, with Piétrement, that all Âryas are brachycephalic, with brown eyes and black hair (V. d. Gheyn, 1889, p. 26). There is no difference between the two assertions. They are both perfectly unmeaning. They are *vox et præterea nihil*.

My experiences during the last forty years have only served to confirm the opinion which I expressed forty years ago, that there ought to be a complete separation between philology and physiology. And yet, if I were asked whether such a divorce should now be made absolute, I should say, No. There have been so many unexpected discoveries of new facts, and so many surprising combinations of old facts, that we must always be prepared to hear some new evidence, if only that evidence is brought forward according to the rules which govern the court of true science. It may be that in time the classification of skulls, hair, eyes, and skin may be brought into harmony with the classification of language. We may even go so far as to admit, as a postulate, that the two must have run parallel, at least in the beginning of all things. But with the evidence before us at present, mere wrangling, mere iteration of exploded assertions, mere contradictions, will produce no effect on the true jury, which hardly ever consists of more than twelve trusty men, but with whom the final verdict rests. The very things that most catch the popular ear will by them be ruled out of court. But every single new word, common to all the Aryan languages, and telling of some climatic, geographical, historical, or physiological circumstance in the earliest life of the speakers of Aryan speech, will be truly welcome to philologists quite as much as a skull from an early geological stratum is to the physiologist, and both to the anthropologist, in the widest sense of that name.

But, if all this is so, if the alliance between philology and physiology has hitherto done nothing but mischief, what right, it may be asked, had I to accept the honour of presiding over this Section of Anthropology? If you will allow me to occupy your valuable time a little longer, I shall explain, as shortly as possible, why I thought that I, as a philologist, might do some small amount of good as President of the Anthropological Section.

In spite of all that I have said against the unholy alliance between physiology and philology, I have felt for years—and I believe I am now supported in my opinion by all competent anthropologists—that a knowledge of languages must be considered in future as a *sine quâ non* for every anthropologist.

Anthropology, as you know, has increased so rapidly that it seems to say now, "*Nihil humani a me alienum puto.*" So long as anthropology treated only of the anatomy of the human body, any surgeon might have become an excellent anthropologist. But now, when anthropology includes the study of the earliest thoughts of man, his customs, his laws, his traditions, his legends, his religions, ay, even his early philosophies, a student of anthropology without an accurate knowledge of languages, without the conscience of a scholar, is like a sailor without a compass.

No one disputes this with regard to nations who possess a literature. No one would listen to a man describing the peculiarities of the Greek, the Roman, the Jew, the Arab, the Chinese, without knowing their languages, and being capable of reading the master-works of their literature. We know how often men who have devoted the whole of their life to the study, for instance, of Hebrew, differ, not only as to the meaning of certain words and passages, but as to the very character of the Jews. One authority states that the Jews, and not only the Jews, but all Semitic nations, were possessed of a monotheistic instinct. Another authority shows that all Semitic nations, not excluding the Jews, were polytheistic in their religion, and that the Jehovah of the Jews was not conceived at first as the Supreme Deity, but as a national god only, as the God of the Jews, who, according to the latest view, was originally a fetish or a totem, like all other gods.

You know how widely classical scholars differ on the character of Greeks and Romans, on the meaning of their customs, the purpose of their religious ceremonies—nay, the very essence of their gods. And yet there was a time, not very long ago, when anthropologists would rely on the descriptions of casual travellers, who, after spending a few weeks, or even a few years, among tribes whose language was utterly unknown to them, gave the most marvellous accounts of their customs, their laws, and even of their religion. It may be said that anybody can describe what he sees, even though unable to converse with the people. I say, Decidedly no; and I am supported in this opinion by the most competent judges. Dr. Codrington, who has just published his excellent book on the "Melanesians: their Anthropology and Folk-lore," spent twenty-four years among the Melanesians, learning their dialects, collecting their legends, and making a systematic study of their laws, customs, and superstitions. But what does he say in his preface? "I have felt the truth," he says, "of what Mr. Fison, late missionary in Fiji, has written: 'When a European has been living for two or three years among savages, he is sure to be fully convinced that he knows all about them; when he has been ten years or so amongst them, if he be an observant man, he knows that he knows very little about them, and so begins to learn.'"

How few of the books in which we trust with regard to the characteristic peculiarities of savage races have been written by men who have lived among them for ten or twenty years, and who have learnt their languages till they could speak them as well as the natives themselves.

It is no excuse to say that any traveller who has eyes to see and ears to hear can form a correct estimate of the doings and sayings of savage tribes. It is not so, and anthropologists know from sad experience that it is not so. Suppose a traveller came to a camp where he saw thousands of men and women dancing round the image of a young bull. Suppose that the dancers were all stark naked, that after a time they began to fight, and that at the end of their orgies there were three thousand corpses lying about weltering in their blood. Would not a casual traveller have described such savages as worse than the Negroes of Dahomey? Yet these savages were really the Jews, the chosen people of God. The image was the golden calf, the priest was Aaron, and the chief who ordered the massacre was Moses. We may read the 32nd chapter of Exodus in a very different sense. A traveller who could have conversed with Aaron and Moses might have understood the causes of the revolt and the necessity of the massacre. But without this power of interrogation and mutual explanation, no travellers, however graphic and amusing their stories may be, can be trusted; no statements of theirs can be used by the anthropologist for truly scientific purposes.

From the day when this fact was recognized by the highest authorities in anthropology, and was sanctioned by some at least of our Anthropological, Ethnological, and Folk-lore Societies, a new epoch began, and philology received its right place as the handmaid of anthropology. The most important paragraph in our new charter was this, that in future no one is to be quoted or relied on as an authority on the customs, traditions, and more particularly on the religious ideas of uncivilized races who has not acquired an acquaintance with their language, sufficient to enable him to converse with them freely on these difficult subjects.

No one would object to this rule when we have to deal with civilized and literary nations. But the languages of Africa, America, Polynesia, and even Australia, are now being studied as formerly Greek, Latin, Hebrew, and Sanskrit only were studied. You have only to compare the promiscuous descriptions of the Hottentots in the works of the best ethnologists with the researches of a real Hottentot scholar like Dr. Hahn to see the advance that has been made. When we read the books of Bishop Callaway on the Zulu, of William Gill and Edward Tregear on the Polynesians, of Horatio Hale on some of the North American races, we feel at once that we are in safe hands, in the hands of real scholars. Even then we must, of course, remember that their knowledge of the languages cannot compare with that of Bentley, or Hermann, or Burnouf, or Ewald. Yet we feel that we cannot go altogether wrong in trusting to their guidance.

I venture to go even a step further, and I believe the time will come when no anthropologist will venture to write on anything concerning the inner life of man without having himself acquired a knowledge of the language in which that inner life finds its truest expression.

This may seem to be exacting too much, but you have only to look, for instance, at the description given of the customs, the laws, the legends, and the religious convictions of the people of India about a hundred years ago, and before Sanskrit began to be studied, and you will be amazed at the utter caricature that is often given there of the intellectual state of the Brahmans compared with what we know of it now from their own literature.

And if that is the case with a people like the Indians, who are a civilized race, possessed of an ancient literature, and well within the focus of history for the last two thousand years, what can be expected in the case of really savage races? One can hardly trust one's eyes when one sees the evidence placed before us by men whose good faith cannot be questioned, and who nevertheless contradict each other flatly on the most ordinary subjects. We owe to one of our Secretaries, Mr. Roth, a most careful collection of all that has been said on the Tasmanians by eye-witnesses. Not the least valuable part of this collection is that it opens our eyes to the utter untrustworthiness of the evidence on which the anthropologist has so often had to rely. In an article on Mr. Roth's book in NATURE, I tried to show that there is not one essential feature in the religion of the Tasmanians on which different authorities have not made assertions diametrically opposed to each other. Some say that the Tasmanians have no idea of a Supreme Being, no rites or ceremonies; others call their religion Dualism, a worship of good and evil spirits. Some maintain that they had deified the powers of Nature, others that they were Devil-worshippers. Some declare their religion to be pure monotheism, combined with belief in the immortality of the soul, the efficacy of prayers and charms. Nay, even the most recent article of faith—the descent of man from some kind of animal—has received a religious sanction among the Tasmanians. For Mr. Horton, who is not given to joking, tells us that they believed "they were originally formed with tails, and without knee-joints, by a benevolent being, and that another descended from heaven, and, compassionating the sufferers, cut off their tails, and with grease softened their knees."

I would undertake to show that what applies to the descriptions given us of the now extinct race of the Tasmanians applies with equal force to the descriptions of almost all the savage races with whom anthropologists have to deal. In the case of large tribes, such as the inhabitants of Australia, the contradictory evidence may, no doubt, be accounted for by the fact that the observations were made in different localities. But the chief reason is always the same—ignorance of the language, and therefore want of sympathy and impossibility of mutual explanation and correction.

Let me, in conclusion, give you one of the most flagrant instances of how a whole race can be totally misrepresented by men ignorant of their language, and how these misrepresentations are at once removed if travellers acquire a knowledge of the language, and thus have not only eyes to see, but ears to hear, tongues to speak, and hearts to feel.

No race has been so cruelly maligned for centuries as the inhabitants of the Andaman Islands. An Arab writer of the ninth century states that their complexion was frightful, their hair frizzled, their countenance and eyes terrible, their feet very large, and almost a cubit in length, and that they go quite naked. Marco Polo (about 1285) declared that the inhabitants are no better than wild beasts, and he goes on to say: "I assure you all the men of this island of Angamanain have heads like dogs, and teeth and eyes likewise; in fact, in the face they are just like big mastiff dogs."

So long as no one could be found to study their language, there was no appeal from these libels. But when, after the Sepoy mutiny in 1857, it was necessary to find a habitation for a large number of convicts, the Andaman Islands, which had already served as a penal settlement on a smaller scale, became a large penal colony under English officers. The havoc that was wrought by this sudden contact between the Andaman Islanders and these civilized Indian convicts was terrible, and the end will probably be the same as in Tasmania—the native population will die out. Fortunately one of the English officers (Mr. Edward Horace Man) did not shrink from the trouble of learning the language spoken by these islanders, and, being a careful observer and perfectly trustworthy, he has given us some accounts of the Andaman aborigines which are real masterpieces of anthropological research. If these islanders must be swept away from the face of the earth, they will now, at all events, leave a good name behind them. Even their outward appearance seems to become different in the eyes of a sympathizing observer from what it was to casual travellers. They are, no doubt, a very small race, their average height being 4 feet 10¾ inches. But this is almost the only charge brought against them which Mr. Man has not been able to rebut. Their hair, he says, is fine, very closely curled, and frizzly. Their colour is dark, but not absolutely black. Their features possess little of the most marked and coarser peculiarities of the Negro type. The projecting jaws, the prominent thick lips, the broad and flattened nose of the genuine Negro, are so softened down as scarcely to be recognized.

But let us hear now what Mr. Man has to tell us about the social, moral, and intellectual qualities of these so-called savages, who had been represented to us as cannibals; as ignorant of the existence of a deity; as knowing no marriage; except what by a bold euphemism has been called communal marriage; as unacquainted with fire; as no better than wild beasts, having heads, teeth, and eyes like dogs—being, in fact, like big mastiffs.

"Before the introduction into the islands of what is called European civilization, the inhabitants," Mr. Man writes, "lived in small villages, their dwellings built of branches and leaves of trees. They were ignorant of agriculture, and kept no poultry or domestic animals. Their pottery was hand-made, their clothing very scanty. They were expert swimmers and divers, and able to manufacture well-made dug-out canoes and outriggers. They were ignorant of metals, ignorant, we are told, of producing fire, though they kept a constant supply of burning and smouldering wood. They made use of shells for their tools, had stone hammers and anvils, bows and arrows, harpoons for killing turtle and fish. Such is the fertility of the island that they have abundance and variety of food all the year round. Their food was invariably cooked, they drank nothing but water, and they did not smoke. People may call this a savage life. I know many a starving labourer who would gladly exchange the benefits of European civilization for the blessings of such savagery."

These small islanders, who have always been represented by a certain class of anthropologists as the lowest stratum of humanity, need not fear comparison, so far as their social life is concerned, with races who are called civilized. So far from being addicted to what is called by the self-contradictory name of communal marriage, Mr. Man tells us that bigamy, polygamy, polyandry, and divorce are unknown to them, and that the marriage contract, so far from being regarded as a merely temporary contract, to be set aside on account of incompatibility of temper or other such causes, is never dissolved. Conjugal fidelity till death is not the exception but the rule, and matrimonial differences, which occur but rarely, are easily settled with or without the intervention of friends. One of the most striking features of their social relations is the marked equality and affection which exist between husband and wife, and the consideration and respect with which women are treated might, with advantage, be emulated by certain classes in our own land. As to cannibalism or infanticide, they are never practised by them.

It is easy to say that Mr. Man may be prejudiced in favour of these little savages, whose language he has been at so much pains to learn. Fortunately, however, all his statements have lately been confirmed by another authority, Colonel Cadell—the Chief Commissioner of these islands. He is a Victoria Cross man, and not likely to be given to over-much sentimentality. Well, this is what he says of these fierce mastiffs, with feet a cubit in length :—

"They are merry little people," he says. "One could not imagine how taking they were. Everyone who had to do with them fell in love with them [these fierce mastiffs]. Contact with civilization had not improved the morality of the natives, but in their natural state they were truthful and honest, generous and self-denying. He had watched them sitting over their fires cooking their evening meal, and it was quite pleasant to notice the absence of greed and the politeness with which they picked off the tit-bits and thrust them into each other's mouths. The forest and sea abundantly supplied their wants, and it was therefore not surprising that the attempts to induce them to take to cultivation had been quite unsuccessful, highly though they appreciated the rice and Indian corn which were occasionally supplied to them. All was grist that came to their mill in the shape of food. The forest supplied them with edible roots and fruits. Bats, rats, flying foxes, iguanas, sea-snakes, mollusks, wild pig, fish, turtle, and last, though not least, the larvæ of beetles, formed welcome additions to their larder. He remembered one morning landing by chance at an encampment of theirs, under the shade of a gigantic forest tree. On one fire was the shell of a turtle, acting as its own pot, in which was simmering the green fat delicious to more educated palates ; on another its flesh was being broiled, together with some splendid fish ; on a third a wild pig was being roasted, its drippings falling on wild yams, and a jar of honey stood close by, all delicacies fit for an alderman's table."

These are things which we might suppose anybody who has eyes to see, and who is not wilfully blind, might have observed. But when we come to traditions, laws, and particularly to religion, no one ought to be listened to as an authority who cannot converse with the natives. For a long time the Mincopies have been represented as without any religion, without even an idea of the Godhead. This opinion received the support of Sir John Lubbock, and has been often repeated without ever having been re-examined. As soon, however, as these Mincopies began to be studied more carefully—more particularly as soon as some persons resident among them had acquired a knowledge of their language, and thereby a means of real communication—their religion came out as clear as daylight. According to Mr. E. H. Man, they have a name for God—*Pûluga*. And how can a race be said to be without a knowledge of God if they have a name for God? *Pûluga* has a very mythological character. He has a stone house in the sky ; he has a wife, whom he created himself, and from whom he has a large family, all, except the eldest, being girls. The mother is supposed to be green (the earth ?), the daughters black ; they are the spirits, called *Môrowin ;* his son is called *Pîjchor*. He alone is permitted to live with his father, and to convey his orders to the *Môrowin*. But *Pûluga* was a moral character also. His appearance is like fire, though nowadays he has become invisible. He was never born, and is immortal. The whole world was created by him, except only the powers of evil. He is omniscient, knowing even the thoughts of the heart. He is angered by the commission of certain sins—some very trivial, at least to our mind—but he is pitiful to all who are in distress. He is the judge from whom each soul receives its sentence after death.

According to other authorities, some Andamanese look on the sun as the fountain of all that is good, the moon as a minor power ; and they believe in a number of inferior spirits, the spirits of the forest, the water, and the mountain, as agents of the two higher powers. They believe in an evil spirit also, who seems to have been originally the spirit of the storm. Him they try to pacify by songs, or to frighten away with their arrows.

I suppose I need say no more to show how indispensable a study of language is to every student of anthropology. If anthropology is to maintain its high position as a real science, its alliance with linguistic studies cannot be too close. Its weakest points have always been those where it trusted to the statements of authorities ignorant of language and of the science of language. Its greatest triumphs have been achieved by men such as Dr. Hahn, Bishops Callaway and Colenso, Dr. W. Gill, and last, not least, Mr. Man, who have combined the minute accuracy of the scholar with the comprehensive grasp of the anthropologist, and were thus enabled to use the key of language to unlock the perplexities of savage customs, savage laws and legends, and, particularly, of savage religions and mythologies. If this alliance between anthropology and philology becomes real, then, and then only, may we hope to see Bunsen's prophecy fulfilled, that anthropology will become the highest branch of that science for which this British Association is instituted.

Allow me in conclusion once more to quote some prophetic words from the address which Bunsen delivered before our Section in 1847 :—

"If man is the apex of the creation, it seems right, on the one side, that a historical inquiry into his origin and development should never be allowed to sever itself from the general body of natural science, and in particular from physiology. But, on the other side, if man is the apex of the creation, if he is the end to which all organic formations tend from the very beginning, if man is at once the mystery and the key of natural science, if that is the only view of natural science worthy of our age, then ethnological philology (I should prefer to say anthropology), once established on principles as clear as the physiological are, is the highest branch of that science for the advancement of which this Association is instituted. It is not an appendix to physiology or to anything else ; but its object is, on the contrary, capable of becoming the end and goal of the labours and transactions of a scientific Association."

Much has been achieved by anthropology to justify these hopes and fulfil the prophecies of my old friend Bunsen. Few men live to see the fulfilment of their own prophecies, but they leave disciples whose duty it is to keep their memory alive, and thus to preserve that vital continuity of human knowledge which alone enables us to see in the advancement of all science the historical evolution of eternal truth.

ELECTRICAL STANDARDS.

THE Queen's Printers are now issuing the Report (dated July 23, 1891) to the President of the Board of Trade, of the Committee appointed to consider the question of constructing standards for the measurement of electricity. The Committee included Mr. Courtenay Boyle, C.B., Major P. Cardew, R.E., Mr. E. Graves, Mr. W. H. Preece, F.R.S., Sir W. Thomson, F.R.S., Lord Rayleigh, F.R.S., Prof. G. Carey Foster, F.R.S., Mr. R. T. Glazebrook, F.R.S., Dr. John Hopkinson, F.R.S., Prof. W. E. Ayrton, F.R.S.

In response to an invitation, the following gentlemen attended and gave evidence :—On behalf of the Association of Chambers of Commerce, Mr. Thomas Parker and Mr. Hugh Erat Harrison ; on behalf of the London Council, Prof. Silvanus Thompson ; on behalf of the London Chamber of Commerce, Mr. R. E. Crompton. The Committee were indebted to Dr. J. A. Fleming and Dr. A. Muirhead for valuable information and assistance ; and they state that they had the advantage of the experience and advice of Mr. H. J. Chaney, the Superintendent of Weights and Measures. The Secretary to the Committee was Sir T. W. P. Blomefield, Bart.

The following are the resolutions of the Committee :—

Resolutions.

(1) That it is desirable that new denominations of standards for the measurement of electricity should be made and approved by Her Majesty in Council as Board of Trade standards.

(2) That the magnitudes of these standards should be determined on the electro-magnetic system of measurement with reference to the centimetre as unit of length, the gramme as unit of mass, and the second as unit of time, and that by the terms centimetre and gramme are meant the standards of those denominations deposited with the Board of Trade.

(3) That the standard of electrical resistance should be denominated the ohm, and should have the value 1,000,000,000 in terms of the centimetre and second.

(4) That the resistance offered to an unvarying electric current

by a column of mercury of a constant cross sectional area of 1 square millimetre, and of a length of 106·3 centimetres at the temperature of melting ice may be adopted as 1 ohm.

(5) That the value of the standard of resistance constructed by a committee of the British Association for the Advancement of Science in the years 1863 and 1864, and known as the British Association unit, may be taken as ·9866 of the ohm.

(6) That a material standard, constructed in solid metal, and verified by comparison with the British Association unit, should be adopted as the standard ohm.

(7) That for the purpose of replacing the standard, if lost, destroyed, or damaged, and for ordinary use, a limited number of copies should be constructed, which should be periodically compared with the standard ohm and with the British Association unit.

(8) That resistances constructed in solid metal should be adopted as Board of Trade standards for multiples and submultiples of the ohm.

(9) That the standard of electrical current should be denominated the ampere, and should have the value one-tenth (0·1) in terms of the centimetre, gramme, and second.

(10) That an unvarying current which, when passed through a solution of nitrate of silver in water, in accordance with the specification attached to this report, deposits silver at the rate of 0·001118 of a gramme per second, may be taken as a current of 1 ampere.

(11) That an alternating current of 1 ampere shall mean a current such that the square root of the time-average of the square of its strength at each instant in amperes is unity.

(12) That instruments constructed on the principle of the balance, in which by the proper disposition of the conductors, forces of attraction and repulsion are produced, which depend upon the amount of current passing, and are balanced by known weights, should be adopted as the Board of Trade standards for the measurement of current, whether unvarying or alternating.

(13) That the standard of electrical pressure should be denominated the volt, being the pressure which, if steadily applied to a conductor whose resistance is 1 ohm, will produce a current of 1 ampere.

(14) That the electrical pressure at a temperature of 62° F. between the poles or electrodes of the voltaic cell known as Clark's cell, may be taken as not differing from a pressure of 1·433 volts, by more than an amount which will be determined by a sub-committee appointed to investigate the question, who will prepare a specification for the construction and use of the cell.

(15) That an alternating pressure of 1 volt shall mean a pressure such that the square root of the time-average of the square of its value at each instant in volts is unity.

(16) That instruments constructed on the principle of Sir W. Thomson's quadrant electrometer used idiostatically, and for high-pressure instruments on the principle of the balance, electrostatic forces being balanced against a known weight, should be adopted as Board of Trade standards for the measurement of pressure, whether unvarying or alternating.

We have adopted the system of electrical units originally defined by the British Association for the Advancement of Science, and we have found in its recent researches, as well as in the deliberations of the International Congress on Electrical Units, held in Paris, valuable guidance for determining the exact magnitudes of the several units of electrical measurement, as well as for the verification of the material standards.

We have stated the relation between the proposed standard ohm and the unit of resistance originally determined by the British Association, and have also stated its relation to the mercurial standard adopted by the International Conference.

We find that considerations of practical importance make it undesirable to adopt a mercurial standard, we have, therefore, preferred to adopt a material standard constructed in solid metal.

It appears to us to be necessary that in transactions between buyer and seller, a legal character should henceforth be assigned to the units of electrical measurement now suggested, and with this view, that the issue of an Order in Council should be recommended, under the Weights and Measures Act, in the form annexed to this report.

Specification referred to in Resolution 10.

In the following specification the term silver voltameter means the arrangement of apparatus by means of which an electric current is passed through a solution of nitrate of silver in water. The silver voltameter measures the total electrical quantity which has passed during the time of the experiment, and by noting this time the time-average of the current, or if the current has been kept constant, the current itself, can be deduced.

In employing the silver voltameter to measure currents of about 1 ampere, the following arrangements should be adopted. The kathode on which the silver is to be deposited should take the form of a platinum bowl not less than 10 cm. in diameter, and from 4 to 5 cm. in depth.

The anode should be a plate of pure silver some 30 square cm. in area and 2 or 3 millimetres in thickness.

This is supported horizontally in the liquid near the top of the solution by a platinum wire passed through holes in the plate at opposite corners. To prevent the disintegrated silver which is formed on the anode from falling on to the kathode, the anode should be wrapped round with pure filter paper, secured at the back with sealing-wax.

The liquid should consist of a neutral solution of pure silver nitrate, containing about 15 parts by weight of the nitrate to 85 parts of water.

The resistance of the voltameter changes somewhat as the current passes. To prevent these changes having too great an effect on the current, some resistance besides that of the voltameter should be inserted in the circuit. The total metallic resistance of the circuit should not be less than 10 ohms.

Method of making a Measurement.—The platinum bowl is washed with nitric acid and distilled water, dried by heat, and then left to cool in a desiccator. When thoroughly dry, it is weighed carefully.

It is nearly filled with the solution, and connected to the rest of the circuit by being placed on a clean copper support, to which a binding screw is attached. This copper support must be insulated.

The anode is then immersed in the solution, so as to be well covered by it, and supported in that position; the connections to the rest of the circuit are made.

Contact is made at the key, noting the time of contact. The current is allowed to pass for not less than half an hour, and the time at which contact is broken is observed. Care must be taken that the clock used is keeping correct time during this interval.

The solution is now removed from the bowl, and the deposit is washed with distilled water and left to soak for at least six hours. It is then rinsed successively with distilled water and absolute alcohol, and dried in a hot-air bath at a temperature of about 160° C. After cooling in a desiccator, it is weighed again. The gain in weight gives the silver deposited.

To find the current in amperes, this weight, expressed in grammes, must be divided by the number of seconds during which the current has been passed, and by 0·001118.

The result will be the time-average of the current, if during the interval the current has varied.

In determining by this method the constant of an instrument the current should be kept as nearly constant as possible, and the readings of the instrument taken at frequent observed intervals of time. These observations give a curve from which the reading corresponding to the mean current (time-average of the current) can be found. The current, as calculated by the voltameter, corresponds to this reading.

NOTES.

THE International Meteorological Conference at Munich was opened on August 26. Dr. C. Lang, Director of the Bavarian Meteorological Service, was unanimously elected President. Prof. M. W. Harrington (Chief of the United States Weather Bureau) and Prof. E. Mascart (Director of the French Meteorological Service) were elected Vice-Presidents. Mr. R. H. Scott (Secretary of the Meteorological Office), Dr. F. Erk (Munich), and M. L. Teisserenc de Bort (Paris) were elected Secretaries. Thirty members were present, including representatives from Brazil, Queensland, and the United States. We hope in a future number to give some account of the proceedings.

DR. BARCLAY, whose death at Simla has been announced, was working on the Leprosy Commission, and his loss is described by the Indian press as not only a severe one to India, but for the whole scientific world. His special study was cryptogamic botany. He made important researches in diseases of Indian plants, and had gained a continental reputation. Several of his papers were published in the Linnean Society's Transactions. His great ambition was to solve Indian wheat disease, and he was to have studied coffee disease in Southern India next winter.

PARTLY owing to Dr. Barclay's death, the Indian Leprosy Report will be delayed a short time. The practical work is virtually completed, and the Draft Report for the Government of India is in type. The chief work now consists in correcting the proofs and the preparation of the plates, maps, and statistics. On the two main questions with which they were to deal, viz. the contagiousness and hereditary transmission of the disease, the Commission have come to a unanimous decision, but their conclusions will not be known till the Report is published by the National Leprosy Fund.

THE statutory ninth meeting of the International Congress of Orientalists began in the hall of the Inner Temple on Tuesday, when an address was delivered by the Master of St. John's College, Cambridge.

AN election to the Coutts Trotter Studentship, at Trinity College, Cambridge, will take place next month. Applications from candidates must be sent in to the College office, addressed to the Secretary of the Coutts Trotter Studentship Committee, on or before October 15. The studentship is tenable for two years, and is for original research in physiology or in physics.

WE are glad to learn that a number of the friends of the late Mr. N. R. Pogson are thinking of raising a memorial to him in Madras.

WITH reference to a recent note, we learn from New South Wales that the Minister for Mines and Agriculture (the Hon. Sydney Smith) has appointed Mr. Niel Harper, formerly a dairy farmer of excellent repute in the South Coast District, to take charge of the travelling dairy, which is to be sent to the different districts of the colony under the control of the Department of Agriculture. It will be necessary for the Agricultural Society, or a local Committee, to provide the requirements of the dairy such as a building suitable for its operations, and giving accommodation sufficient for ten pupils, who will be thoroughly instructed in all dairying operations. Also, for the carriage of the plant to and from the nearest railway station or wharf to the scene of operations, together with the necessary labour to assist in the rough work of cleaning up, &c. The Society, or Committee, will need to provide also a sufficient supply of milk, say about fifty gallons daily, for the operations of the dairy, and plenty of clean water for washing butter and cleaning up. Each Society, or Committee, undertaking to furnish these requirements will be entitled to nominate at least ten pupils (either male or female) for the full course of instruction in dairy operations, who will afterwards be examined with a view to receiving a dairy certificate in the event of their showing a satisfactory knowledge of the course of instruction. Of course the general public will be admitted to see all the operations of the dairy, which will work for ten days at each place where set up. All district Societies and Committees desiring to have the benefit of this course of instruction for their localities should make early application to the Director of Agriculture, from whom regulations and instructions can be obtained. Is our Minister of Agriculture doing anything similar?

AT the request of the Russian Ambassador in London, the Secretary of State for India has asked the Government of India to afford facilities to Prof. Tichomiroff, who is about to visit certain parts of India, Ceylon, and China, with the view of studying the administration of botanical gardens and cinchona plantations, and to M. Gondatti, who is about to study tea and silkworm cultivation in India, Ceylon, and China.

CAPTAIN WAHAB, R.E., will have charge of a party which is to make a survey of the country round Aden during the coming winter.

MR. GRIESBACH, of the Geological Survey of India, has proceeded with a survey party to Upper Burmah, where he will remain about two years to examine thoroughly the geological condition of the country.

AN important resolution of the Government of India on the reorganization of the superior staff of the Indian Forest Department has been issued. At an extra yearly cost of three lakhs of rupees, the Imperial and Provincial Services are to be separated. The Imperial is to be recruited solely under covenant with the Secretary of State, and the average pay raised 6 per cent. The Provincial Service gives 126 appointments, up to 600 rupees a month, to natives of India. The Forest Department is the first to introduce a complete scheme under the Public Service Commission.

NINE members of the *Kite* Arctic Expedition arrived at Halifax, N.S., on August 30. The Expedition reached 77° 43′ N., and 70° 20′ W. They have brought with them immense collections of flowers, herbs, and butterflies, some of which were previously unknown. It is stated that "they found all the published charts of Greenland to be incorrect."

EXPERIMENTS for the production of artificial rain are now being made in Texas. They are conducted by members of the Signal Corps, acting under the direction of the Minister of Agriculture, and have been undertaken in accordance with a vote of the United States Congress. Adequate reports on the subject have not yet reached this country, but it is claimed that the experiments have been attended by remarkable success.

MR. GEORGE FORBES, writing to the *Times* on August 31, gave the following account of a meteor which he had seen at Maidenhead on the previous evening at 8h. 22m.:—"It was brighter than Jupiter when I first saw it; it lasted three seconds from the time I first saw it, steadily increasing in size and brightness, becoming pear-shaped, and blue showing in its rear part when at its brightest—*i.e.* just before extinction. There was no train, the luminosity not extending more than 1° behind it. At the end it became intensely bright, and then disappeared suddenly. It passed a little south of α Cassiopeiæ, and also a little south of γ Andromedæ. I first saw it at 1h. 45m. R.A. and 50° N. Decl., and it ended at 2h. 0m. R.A. and 39° N. Decl."

IN the *Meteorologische Zeitschrift* for July, Prof. H. Mohn discusses the present methods of reduction of meteorological observations; after the completion of twenty-five years of observations at the Norwegian stations, he has decided upon making certain more or less important alterations, commencing from January 1 last. (1) As regards pressure, to introduce the correction for standard gravity at sea-level, in latitude 45°, which amounts to 0·16 inch between the equator and the Poles, and to as much as 0·03 inch between two extreme stations of the Norwegian system. And to apply a correction due to diurnal range (to be determined from hourly observations) to the monthly means obtained and published from two or three observations daily. (2) Similarly, for temperature and humidity, to apply corrections to the published monthly values obtained from two or three daily observations. He fully explains the methods he has adopted for obtaining the corrections to be applied, and we think the matter is worthy of the attention of meteorologists who publish their results. Prof. W. von Bezold gives an interesting summary of his paper on the theory of cyclones, which he laid before the Berlin Academy in December last, and in which he treated of the more recent views regarding the laws of atmospheric circulation; he also refers to various points which have to be dealt with for the further advancement of the science.

M. LANCASTER has recently indicated in *Ciel et Terre* the divergences from normal temperature in Europe in the five years 1886–90. It appears (and is shown in a map) that the centre of the "island of cold" lies over the north of France, the south of Belgium, and the most western parts of Germany. From this centre the cold decreases pretty regularly outwards on all sides to a nearly circular line of *nil* divergence, which, embracing the whole of Great Britain, crosses the south of Sweden, then goes along the German-Russian frontier, through Hungary, the south of Italy, the north of Africa, and across Spain. Throughout this inclosed region abnormally low temperatures have prevailed. Siberia, too, shows thermal depression, which M. Lancaster thinks may be connected with that in Western Europe.

SR. H. MORIZE, astronomer at the Observatory of Rio de Janeiro, has just published a "Sketch of the Climatology of Brazil," which will be welcome to meteorologists, as hitherto systematic observations have only been published for a very few points of that immense country, covering 39° of latitude. The present sketch has been drawn up mainly from the observations of travellers and private observers. We can only extract a few brief notes. Thunderstorms are very frequent all along the coast, and are mostly harmless; regular cyclones are very rare—the most dangerous winds are the pamperos, which blow from the south-west, and have been fully described by the late Admiral Fitz-Roy, and a still more rare and dangerous wind which blows from the south-east. As regards temperature, the author has divided the country into three zones, and some valuable data are given for various localities. Parts of the country are subject to prolonged drought; it is said that at Pernambuco no rain fell during the whole year 1792, and a third of the population died from its effects; droughts have recurred during the present century with some regularity, the last being in the year 1888–89. The most complete series of observations is that for Rio de Janeiro, which dates from 1781, with occasional interruptions. The highest shade temperature was 99°·5 in November 1883, and the lowest 50°·4 in September 1882. There are also good series of observations for Rio Grande do Sul and São-Paulo.

ONE of the most important contributions made of late years to our knowledge of the embryology of flowering plants is to be found in a paper by a lady, Mdlle. C. Sokolowa, in the *Bulletin* of the Imperial Society of Naturalists of Moscow. It relates especially to the formation of the endosperm within the embryo-sac of Gymnosperms, the particulars of which are described in great detail. The process is somewhat intermediate between that of ordinary cell-division and that known as free cell-formation. It is a group of short cells belonging to the parietal layer of this endosperm that ultimately develop into the corpuscles or secondary embryo-sacs, the homologues of the archegones of Vascular Cryptogams. In the tendency displayed by *Pinus* and *Cephalotaxus* towards the early differentiation of these cells, Mdlle. Sokolowa sees the foreshadowing of the process which is universal in Angiosperms, the formation of the embryonic vesicles before that of the endosperm. *Ephedra* exhibits a still closer approximation in this respect to Angiosperms than to the Conifers. In the same number of the *Bulletin* is an interesting and important paper by Prof. Goroschankin on the "Structure and Reproduction of *Chlamydomonas*." The former paper is written in French, the latter in German.

THE survey of the cañon of the Colorado has now been completed, and Mr. R. B. Stanton has given a full account of it in the American *Engineering News*. In spite of the great depths of the cañon and the cliffs of sandstone, marble, and granite composing it, a railway can in his opinion be built through it without much tunnelling, thus opening up some of the grandest scenery of the world. In many places the cañon expands into wide valleys, and even where it narrows there are terraces along the sides like the "parallel roads" of Glen Roy in Scotland, which seem designed by nature for track and rail. The tributaries which enter the cañon laterally are as a rule small, and can be easily bridged. The distance of 1019 miles through the cañon district will only comprise 20 miles of tunnelling and 99 miles of granite cutting.

AT the meeting of the Linnean Society of New South Wales, on June 24, Mr. C. Darley exhibited some very large examples of the shells of the mud oyster (*Ostræa edulis*, var. *angasi*) obtained during dredging operations in Rozelle Bay, Sydney Harbour. They occur in great numbers at a depth of 10 to 12 feet below low water-mark beneath a layer of black mud 3 to 4 feet thick, and are much larger than specimens now to be found living in the harbour. The two valves of one pair weigh 3 pounds 12 ounces, and measure about 8 × 6 inches.

IN *Nature Notes* for August Mr. R. T. Lewis, on the authority of a correspondent in whose trustworthiness he has entire confidence, gives a curious account of the appreciation with which the song of the Cicada is heard by insects other than those of its own genus. The correspondent has frequently observed in Natal that when the Cicada is singing at its loudest, in the hottest portion of the day, it is attended by a number of other insects with lovely, gauze-like, iridescent wings, whose demeanour has left no doubt on his mind that the music is the attraction. The Cicada, when singing, usually stations itself upon the trunk of a tree with its head uppermost, and the insects in question, to the number sometimes of fifteen or sixteen, form themselves into a rough semicircle at a short distance around its head. During a performance one of the insects was observed occasionally to approach the Cicada and to touch it upon its front leg or antennæ, which proceeding was resented by a vigorous stroke of the foot by the Cicada, without, however, any cessation of its song. The insects composing the audience are extremely active; and so wary that they take flight at the least alarm on the too near approach of any intruder. Some of them, however, have been captured; and on examination these "proved to belong to the same family as that most beautiful of British insects—the lace-wing fly, which, indeed, they closely resemble except as to size, their measurement across the expanded wings being a little over two inches; they have since been identified by Mr. Kirby at the British Museum as *Nothochrysa gigantea*."

ACCORDING to a telegram through Dalziel's agency from Vancouver, the Canadian Pacific steamer *Japan*, which arrived there from Hong Kong and Yokohama on August 30, has reported a terrific typhoon at Kobe on the 16th inst. All the steamers in the harbour dragged their anchors, and many native boats were cast ashore and their crews were drowned. A German steamship was driven ashore and eight of the crew were drowned, and an Indian barque *Singlas* was wrecked, and all on board were lost. Her Majesty's gunboat *Tweed* sank. Altogether among natives and foreigners it is believed that 250 lives were lost. The wind did much damage inshore. In one coast town forty-five persons were killed by falling houses.

THE Science and Art Department has issued its Directory (revised to June 1891), with regulations for establishing and conducting science and art schools and classes.

THE University College, Bristol, has issued its Calendar for the session 1891–92. While the College supplies for persons of either sex above the ordinary school age the means of continuing their studies in science, languages, history, and literature, it claims especially to afford appropriate and systematic instruction in those branches of applied science which are more nearly connected with the arts and manufactures.

SIR WILLIAM MACGREGOR, Governor of British New Guinea, recently ascended Mount Yule, or Kovio, as he prefers to call it. The Kovio range is volcanic and isolated from the main chain, of which Mount Owen Stanley is the culmination. The Kovio range is under 11,000 feet high, and is wooded to the very summit. Native tracks lead through the forest to the top of Mount Yule, on the south-west front of which there is a magnificent series of cascades, having a height of 4000 feet in all. A new river and a new lake were also discovered; but the animal life of the region was far from abundant.

THE last Bulletin of the Geographical Society of the United States contains an interesting paper on the curious discovery of human remains under the Tuolumne Table Mountain of California. Bones of men and grinding instruments were there found by Prof. Whitney, embedded in auriferous gravel under lava at the foot of the mountain. Remains of plants belonging to the Tertiary age, and the bones of extinct Mammalia, such as the rhinoceros of the West and the American mastodon, are also met with in the same strata. Pestles, mortars, and broken spear-heads are the most remarkable of the implements discovered.

FROM the last Report of the Council of the North China Asiatic Society of Shanghai we learn that the printers have now in hand a most valuable work by Dr. Bretschneider on the "Botany of the Chinese Classics," the publication of which, on account of its length and technical difficulties, has been much delayed. Some time, however, must yet elapse before it can be issued. Mr. Faber has undertaken the difficult task of correcting the printer's proofs and adding many notes, which will render the work the most comprehensive and useful book which has yet appeared on Chinese botany.

THE new number of the *Internationales Archiv für Ethnographie* (Band iv., Heft 4) opens with an interesting paper by Prof. A. C. Haddon, on the Tugeri head-hunters of New Guinea. Mr. J. J. M. de Groot has an article on the wedding garments of a Chinese woman, and Dr. Julius Jacobs discusses (in Dutch) the ideas of Dr. Ploss on the origin of circumcision.

MESSRS. WEST, NEWMAN, AND CO., have reprinted from the *Journal of Botany* for 1891, a "Key to the Genera and Species of British Mosses," by the Rev. H. G. Jameson. The author explains that his work is not intended to take the place of a more detailed text-book, but merely to serve as a clue by which the student may ascertain in what part of his book he should look for the description of any unknown specimen.

WE have received a Report on Astronomical Observations for 1886, by George H. Boehmer. Directors of observatories, and astronomers generally, are earnestly requested by Mr. Boehmer to criticize his work freely, and to send him such corrections and additions as may seem to them necessary or desirable.

MESSRS. W. WESLEY AND SON have published a catalogue of botanical books which they are offering for sale.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♀) from India, a Pinche Monkey (*Midas œdipus* ♂) from New Granada, presented by Mr. H. Wather; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Mrs. Any Jones, F.Z.S.; a Slender-billed Cockatoo (*Licmetis tenuirostris*) from South Australia, presented by Miss Caplen; a Marbled Polychrus (*Polychrus marmoratus*), a Thick-necked Tree-Boa (*Epicrates cenchris*) from Trinidad, presented by Messrs R. R. Mole and F. W. Urich; a —— Salamander (*Amblystoma punctatum*) from North America, presented by Mr. J. H. Thomson; a Smooth Snake (*Coronella lævis*), European, presented by Mr. F. C. Adams; a Great Kangaroo (*Macropus giganteus*), a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, deposited.

OUR ASTRONOMICAL COLUMN.

STARS HAVING PECULIAR SPECTRA.—From a communication by Prof. E. C. Pickering to *Astronomische Nachrichten*, No. 3054, it appears that the hydrogen lines G and *h* are bright in a photograph of the spectrum of a third-type star, D.M. + 39° 4851 (R.A. 22h. 24·7m., Decl. + 39° 48′, 1900), taken on July 6. And an examination of the photographs of this region taken on different dates has confirmed the long-period variability of which this spectroscopic appearance is now recognized as a distinctive feature. The seventh magnitude star D.M. − 10° 5057, whose approximate position for 1900 is R.A. 19h. 17·7m., Decl. − 10° 54′, has been previously announced as having a spectrum of the fourth type, but later photographs show that the lines in the spectrum are not those due to hydrogen, but are sometimes seen to be broad bands, and at other times as doubles. These peculiarities, however, cannot be made out in the visible spectrum of the star.

PHOTOGRAPHY OF SOLAR PROMINENCES.—At the meeting of the Paris Academy of Sciences on August 17, M. Deslandres exhibited some of the results he has obtained since May in the photography of bright lines in solar prominence spectra. The negatives exhibit good reversals of H and K, and the first two lines of the ultra-violet hydrogen series. And M. Deslandres finds from a direct comparison with a Giessler tube that the bright line a little less refrangible than H is really due to hydrogen. It is proposed to construct an apparatus by means of which the prominences at all points on the sun's limb may be photographed and their velocities determined. That two observers, Prof. Hale and M. Deslandres, should have been simultaneously working to attain the same object is somewhat remarkable. From the various papers published by the former gentleman, it appears that he obtained the first reversals of H and K in prominence spectra about the middle of April, and the first photograph showing the form of a prominence on May 7.

ENCKE'S COMET (*c* 1891).—The following ephemeris is from one given by Dr. Backlund in the *Bulletin Astronomique* for August:—

Ephemeris for Berlin Midnight.

1891.	R.A.			Decl.			Log *r*.	Log Δ.
	h.	m.	s.	°	′	″		
Aug. 28	5	2	29	+35	8	0	0·0563	0·0454
Sept. 1	6	31	22	35	9	5	0·0316	0·0229
,, 5	7	2	24	34	43	5	0·0045	0·0025
,, 9	7	35	36	33	40	9	9·9749	9·9850
,, 13	8	10	25	31	58	4	9·9424	9·9719
,, 17	8	45	49	29	29	7	9·9060	9·9638
,, 21	9	20	59	26	16	9	9·8655	9·9626
,, 25	9	55	0	22	25	7	9·8200	9·9677
,, 29	10	27	27	18	4	7	9·7689	9·9727
Oct. 3	10	58	18	13	22	6	9·7120	9·9983
,, 7	11	27	55	8	27	2	9·6503	0·0223
,, 11	11	57	2	+ 3	23	2	9·5897	0·0498
,, 15	12	26	30	− 1	44	4	9·5744	0·0783
,, 19	12	56	53	6	46	1	9·5336	0·1050
,, 23	13	27	41	11	24	3	9·5634	0·1278
,, 27	13	58	6	15	26	8	9·6187	0·1472
,, 31	14	27	27	−18	49	3	9·6809	0·1646

The comet is now in Auriga, which is in the north-east about 10 p.m. On September 8 it passes about 2° north of Castor.

A NEW ASTEROID (312).—On August 12, Dr. Palisa observed what may be a new asteroid, or, according to Dr. Berberich, it may turn out to be identical with (149) or (291).

JUPITER AND HIS MARKINGS.

DURING the last few years, Jupiter has been situated so far south of the equator that telescopic observations have had to be pursued under all the disadvantages inseparable from viewing an object at a low altitude. But the conditions are now much improved; the planet, though still in south declination, will be some 11° north of his position in 1890, and will therefore remain much longer above the horizon, and present a better defined and larger disk than during the few preceding oppositions, so that the study of his surface-markings may be resumed under very encouraging circumstances.

The great red spot has been visible and its appearance and movements closely watched during thirteen years, for it was in July 1878 that it was first announced as a striking object. But it probably existed long before this, for the drawings of previous observers include forms which have a very suggestive resemblance to the red spot, though they are under a less conspicuous aspect. There is, in fact, little doubt that this marking is an old feature, but it is liable to considerable variations of tint, inducing obvious changes in its general appearance as presented to telescopic observers. Layers of cloud, moving with unequal velocities and at different elevations above the surface of the planet, probably overlap the spot and partially obliterate it at times, but its definite elliptical outline has been always preserved, and its dimensions have not varied materially. It is the colouring of the spot that has exhibited inconstancy, and especially that of the central region, which changed from a brick-red in 1878–81 to a very light tint, differing little, if at all, from the other parts of the planet's disk in the same latitude. But the margin of the spot has been more durable, and it was visible for several years as a pink ellipse, offering a great similarity to the ellipse seen by Gledhill in 1869–70.

After a somewhat precarious existence, the spot appears to be recovering prominence, though its present aspect will not bear comparison with the features it presented about twelve years ago. Still it is now a fairly conspicuous marking, with a depth of tint far more pronounced than in the years 1884–85. The central part of the spot appears to have regained the reddish hue, and the general appearance of the object is sufficiently marked to recall the grand views it afforded at the period of its best display.

The variable motion of the spot has formed one of its most interesting attributes, and I give below a table of the mean rotation-period deduced from observations during the last eleven oppositions of Jupiter:—

Limiting dates.	Rotations.	Period.		
		h.	m.	s.
1879 July 10—1880 Feb. 7	512	9	55	34·2
1880 Sep. 27—1881 Mar. 17	413	9	55	35·6
1881 July 8—1882 Mar. 30	640	9	55	38·2
1882 July 29—1883 May 4	674	9	55	39·1
1883 Aug. 23—1884 June 12	710	9	55	39·1
1884 Sep. 21—1885 July 8	700	9	55	39·2
1885 Oct. 24—1886 July 24	659	9	55	41·1
1886 Nov. 23—1887 Aug. 2	609	9	55	40·5
1888 Feb. 12—1888 Aug. 22	462	9	55	40·2
1889 May 28—1889 Nov. 26	439	9	55	40·0
1890 May 22—1890 Nov. 25	451	9	55	40·2

On August 7, 1891, I re-observed the spot with a 10-inch reflector, power 252, and found it well-defined and fairly conspicuous. It passed the central meridian of the planet at 11h. 32m., so that it followed Marth's zero meridian (System II.) only 3 minutes. This nearly agrees with two observations by Mr. A. S. Williams in May last, which placed the spot 4 minutes behind the zero meridian. Mr. Marth's computations are to be found in the *Monthly Notices* for March 1891, and they supply a valuable guide to all students of Jovian phenomena.

Apart from the red spot, it is desirable that the white spots near the planet's equator, and the similar markings which verge the northern side of the north equatorial belt, should be assiduously followed, and their individual rotation periods ascertained from a number of fresh observations. These markings are severally controlled by proper motions of very irregular character, and some singular alternations of visibility also affect them. Mr. Williams finds that the equatorial white spots have exhibited a great slackening of speed in recent years. This variation apparently affects the entire equatorial zone, and it will be important to determine the exact extent of it, and whether it is sustained in the present year. The changes of velocity alluded to are scarcely progressive in the same direction; we may expect to find an acceleration sooner or later to compensate for the relatively slow movement of the spots in the few past years. It is not unlikely that the various markings show oscillations of speed recurring at uniform intervals.

Students of this interesting planet will find abundance of materials to collate and discuss. There is ample evidence of the reappearance of certain features after periods of non-visibility. Some of the more durable markings apparently suffer temporary obscuration by vaporous masses suspended above them in the Jovian atmosphere. The disposition of the belts is also liable to changes, though not so rapidly as is generally supposed, for many of the alleged variations have been due to differences in telescopic definition or to the rapid rotation of the planet; circumstances which have not always been adequately allowed for.

W. F. DENNING.

SCIENTIFIC SERIALS.

American Journal of Science, August.—Some of the features of non-volcanic igneous ejections, as illustrated in the four "Rocks" of the New Haven region, West Rock, Pine Rock, Mile Rock, and East Rock, by James D. Dana. A few of the conclusions arrived at from the observations recorded in this paper are that igneous eruptions occurred in the New Haven region after the sandstone had been upturned. The liquid rock forced its way between layers of the sandstone, and lifted it up where the pressure of the rock was not too great to prevent the upheaval. This intrusive action was favoured by the fact that the fissure supplying the lava was inclined in the same direction as the layers of the uplifted sandstone. And the foliation of the underlying schists did not determine the course and dip of the supply fissures. The paper is illustrated by several excellent photographs of the formations investigated.—Note on a reconnaissance of the Ouachita mountain system in Indian territory, by Robert T. Hill.—The continuity of solid and liquid, by Carl Barus. By means of the simple arrangement described in this paper, the author is able to obtain at once the isothermals and isopiestics, and therefore the isometrics, both for the solid and liquid states of the substances experimented upon. The relation of solidification and fusion to pressure and the pressure changes of the isothermal specific volumes of solid and liquid at the solidifying and melting points can also be determined. And from such results the character of fusion and the probable position of critical and transitional points can be found. The author has as yet only investigated the behaviour of naphthalene by his method, but the whole work throws considerable light upon the relation of pressure to phenomena of fusion and solution.—Note on the asphaltum of Utah and Colorado, by George H. Stone. The author has visited all the known asphalte fields of Western Colorado and North-Eastern Utah. The observations he has made bear upon the origin of petroleum, asphalte, natural gas, and other subterranean hydrocarbons, but the facts are hardly sufficient to lead to definite conclusions.—Photographic investigation of prominences and their spectra, by George E. Hale. Account is given of the methods employed by the author for the photography of invisible solar prominences. Special attention has been directed to the photography of the bright prominence lines running through H and K, with a slit tangential to the sun's limb. Four reproductions of negatives showing prominences illustrate the paper.—A gold-bearing hot spring deposit, by Walter Harvey Weed. A microscopical and chemical examination of some specimens of ore from the Mount Morgan Gold Mine, Queensland, demonstrates that the mine is a deposit of a hot spring, the ore being a siliceous sinter impregnated with auriferous hæmatite. This is the only hot spring deposit that has been found to contain gold in commercially valuable quantities, and although the sinter deposits from the hot springs of Yellowstone Park resemble those from Mount Morgan, no trace of the precious metals has been found in them.—Restoration of Stegosaurus, by O. C. Marsh. The species restored is *Stegosaurus ungulatus*, from the Upper Jurassic of Wyoming. A plate, representing the reptile one-thirtieth its natural size, accompanies the paper.

The *American Meteorological Journal* for July contains the following articles:—Franklin's kite experiment, by A. McAdie. After giving various details respecting Franklin's experiments, the author describes similar experiments recently carried on at the Blue Hill Observatory, near Boston, U.S., the chief advance being that at every step the electrical potential of the atmosphere was measured by an electrometer. The kite was sent up on several days, and at a height of 1000 feet sparks over $\frac{1}{8}$ inch in length were obtained; while abnormal movements of the stream of water from the electrometer during electrical disturbance always foretold when a flash of lightning was about to occur.—Cloud heights and velocities at Blue Hill Observatory, by H. H. Clayton. This paper contains the results of cloud observations made at Mr. A. L. Rotch's Observatory during the last five years. The average heights of some of the principal clouds were: *nimbus* 412 metres, *cumulus* (base) 1558 m., false *cirrus* 6500 m., *cirro-stratus* 9652 m., *cirrus* 10,135 m. The cumulus is highest at Blue Hill during the middle of the day. The Upsala observations show that the base of the cumulus, as well as the cirrus, increases in height until evening, but neither of these conclusions apply to the observations at Blue Hill. The average velocity found for the cirrus (82 miles an hour) is twice as great as that found at Upsala. The extreme velocity was found to be 133 miles an hour. A comparison between wind and cloud velocity shows that below 500 metres the wind velocity is less than the cloud velocity. Above that, the excess of the cloud velocity increases up to 1000 metres, and then decreases again till about 1700 metres, after which it steadily increases. This decrease between 1000 and 1700 metres is very probably due to the fact that the clouds between 700 and 1000 metres were mostly observed during the morning, when the cumulus moves most rapidly, and that the clouds between 1000 and 1700 metres were mostly observed during the afternoon, when the cumulus moves slowest.—Meteorological kite-flying, by W. A. Eddy. This is an account of some experiments made at Bergen Point, New Jersey, to determine the vertical extension of warm air currents by means of self-recording thermometers carried by a kite string. Experiments showed that an altitude of 1800 feet could be obtained by using one kite, and that many hundred feet could be added to the altitude by lifting the weight of slack string by fastening on larger kites. It is estimated that by this means an altitude of 4000 feet was obtained. The minimum temperature at an altitude of about 1500 feet, on February 14 last, was only 2° lower than at the surface.

SOCIETIES AND ACADEMIES.

Paris.

Academy of Sciences, August 24.—M. Duchartre in the chair.—Remarks on the dynamic conditions of the development of cometary tails, by Dom Et. Siffert.—*Résumé* of solar observations made at the Observatory of the Roman College during the second quarter of 1891, by M. Tacchini.—On cyclic systems, by A. Ribaucour.—A property of involution, common to a group of five right lines and a system of nine planes, by M. P. Serret.—On the tension of water-vapour up to 200 atmospheres, by M. Ch. Antoine. From the expression $t = \dfrac{1638 - 0{\cdot}0005\mathrm{P}^2}{5{\cdot}0402 - \log \mathrm{P}} - 225$, deduced from the experimental results of MM. Cailletet and Colardeau, the author deduces formulæ for the calculation of P to a first approximation, by the aid of the general formula $\mathrm{P} = \mathrm{G}\left(\dfrac{\mathrm{T} - \lambda}{\mathrm{T}}\right)^a$, given by J. Bertrand to express the tension of vapours. The formulæ given are:—

$$\mathrm{P}^1 = [0{\cdot}0058824\,(t + 70)]^{6{\cdot}0} \text{ applicable from } 0^\circ\text{–}100^\circ;$$
$$\mathrm{P}^1 = [0{\cdot}0064516\,(t + 55)]^{5{\cdot}5} \text{ applicable from } 50^\circ\text{–}200^\circ;$$
$$\mathrm{P}^1 = [0{\cdot}0071069\,(t + 41)]^{6{\cdot}0} \text{ applicable from } 220^\circ\text{–}365^\circ.$$

The value P^1 is then used in Cailletet's formula to calculate P, of which tabulated values are given.—On the rejection, by the liver, of bile introduced into the blood, by M. E. Wertheimer. The author has examined the bile of dogs before and after the injection under varying conditions of sheep's bile. The characteristic absorption spectrum of cholohæmatine, a colouring matter not present in the bile of the dog, but always a constituent of sheep's bile, was invariably found in bile secreted by the dog's liver after injection; thus an indisputable proof is given that the liver takes out bile constituents from the blood, and passes them into the alimentary canal unaltered.

Brussels.

Academy of Sciences, July 4.—M. Plateau in the chair.—On hoar frosts, by M. Folie. Some observations of the ravages caused by frosts which occurred on June 12 and 13 indicate that, if the cultures of the Ardennes are to be preserved from such disastrous effects, the plateau must be again planted with trees. The frosts appear to have had more effect near the soil than at some metres above it.—On one of M. Servais's theorems, by M. E. Catalan.—On an extension of M. Hermite's law of reciprocity, by M. Jacques Deruyts.—On two new Lerneopodians, one of which is found at the Azores, and the other on the coast of Senegal, by M. P. J. Van Beneden. Description is given of male and female *Brachiella chavuii* found at the Azores, and of male and female *Brachiella chevreuxii* from the coast of Senegal. The description is accompanied by a plate.—On a method of generation of the cubic surface, by M. F. Deruyts.

Sydney.

Royal Society of New South Wales, July 1.—H. C. Russell, F.R.S., President, in the chair.—Eighteen new members were elected, and the following papers were read:—On Nos. 13 and 14 compressed-air flying machines, by Lawrence Hargrave.—Some folk-songs and myths from Samoa, translated by the Rev. G. Pratt, with introductions and notes by Dr. John Fraser.—On a cyclonic storm in the Gwydir district, and Preparations now being made in Sydney Observatory for the photographic chart of the heavens (illustrated by photographs), by H. C. Russell, F.R.S., Government Astronomer.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

British Cicadæ, Part 7: G. B. Buckton (Macmillan).—Bibliotheca Botanica (Wesley).—British Oligocene and Eocene Mollusca in the British Museum (Natural History): R. B. Newton (London).—Fossil Botany: H. Graf zu Solms-Laubach; translated by H. E. F. Garnsey, revised by I. B. Balfour (Oxford, Clarendon Press).—Synopsis der Hoeheren Mathematik, Erster Band: J. G. Hagen (Berlin, Dames).—Missouri Botanical Garden Second Annual Report (St. Louis, Mo.).—Blackie's Science Readers, Nos. 2, 4, and 5 (Blackie).—Free Land: Dr. T. Hertzka, translated by A. Ransom (Chatto and Windus).—A Sketch of the Vegetation of British Baluchistan: J. H. Lace and W. B. Hemsley (London).—Bulletins de la Société d'Anthropologie de Paris, January and February, March and April (Paris, Masson).—Papers and Proceedings of the Royal Society of Tasmania for 1890 (Hobart).

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THURSDAY, SEPTEMBER 10, 1891.

AN EVOLUTIONARY CASTIGATION.

Science or Romance? By the Rev. John Gerard, S.J. (London: Catholic Truth Society, 1891.)

THAT the doctrine of evolution should not be as sweet savour in the nostrils of the writer of this little book is in no way surprising, but that he should attack evolutionists and their ways with the weapons of flippancy and ridicule is an encouraging indication that the said doctrine has penetrated into quarters from which the author evidently thinks it high time to eject this modern heresy. Having seized the scourge, Father Gerard accordingly proceeds to lay out all round, delivering his blows with vigour, if not with discrimination, and occasionally throwing such force into his strokes that the lash recoils and strikes the striker. In happy unconsciousness that he hits himself quite as often as he does his adversaries, the author goes on with his flagellation through six essays occupying 136 pages of somewhat close print. Although, as we have said, the attitude taken by the author will cause no astonishment, it is very much to be regretted that he has so far put himself out of harmony with the spirit of modern biological thought as to confuse the opinions, speculations, and working hypotheses of individual exponents of evolution with the broad principles of that doctrine. For, however distasteful it may be to Father Gerard, it is an indisputable fact that the acceptance of that doctrine is well-nigh universal, and the question whether evolution is or is not a *modus operandi* in nature, has passed beyond the phase of discussion among scientific thinkers and workers. So far as the author's attacks are directed against evolution as a principle, his weapon is as a bladder of air against the hide of a hippopotamus. It is satisfactory to find, however, that amidst the whizzing of his *flagellum* the author discerns the still small voice of reason:—

> "The one fact given us, is the existence of evidence to show that various species of plants and animals have probably, or possibly, been developed one from another. This, so far as it goes, is matter for scientific treatment; and the theory of evolution, within the limits thus afforded, has a right to be called a scientific hypothesis."

We are grateful for small mercies, and it would be ungracious to inquire too closely into the origin of this concession, but to those who read between the lines it will be apparent that the thirty years' campaign carried on by evolutionists has not been without result, even in the most unpromising fields.

The antagonist whom evolutionists in general and Darwinians in particular have found in the author of the work under consideration is a foeman not altogether unworthy of their steel. He brings into the arena a certain amount of knowledge of living things which indicates that he is an observer of nature in the field. Moreover, he shows some understanding of his subject, and does not fall into the error of substituting blundering misconceptions for the statements of fact or theory which he is combating. Added to this there is a certain keenness of satire running through his essays which adds to their piquancy. The name of Father Gerard on the title-page is a sufficient indication that evolutionists will find death and no quarter in his pages, and the reader will not be disappointed if he turns to these essays with the special object of finding the weaknesses of the modern school exposed. But while the purely destructive attacks of the reverend critic may give satisfaction to those who belong to his school, the impartial reader will derive only amusement, and the man of science will soon perceive that the weapons of attack are not the legitimate implements of scientific warfare, but the tricks of disputation concealed under a somewhat alluring literary cloak, embellished here and there with a few flowers of the author's own culling.

Having arrived at this general estimate of the work, it will not be necessary to do more than take a passing glance at its contents. The first essay, entitled "A Tangled Tale," opens with an attack on natural selection; the author will have none of it; he objects to the term and he denies its efficiency:—

> "It would, in fact, be vastly more likely that we should cast aces three hundred times running, with a pair of unloaded dice, or toss 'tails' two thousand times with an honest coin, than that a development should be handed down by natural selection through ten generations, even if we start with so favourable a supposition as that one-half of the offspring tend to vary in the required direction."

This conclusion is based on a calculation in which the whole principle of selection is ignored!

The central idea of this essay is, that evolutionists have reduced the operations of Nature to "chance," "accident," and so forth. We are told, at the very outset:—

> "The cardinal point of the doctrine they proclaim is, that no purpose operates in Nature, and that the explanation of everything we see is to be found in the mechanical forces of matter."

In order that there may be no misunderstanding as to what the author means by chance, he defines it as "the coincidence of independent phenomena—that is, of phenomena not co-ordinated to an end." By what criterion, may we ask, are "chance" phenomena, as thus defined, to be distinguished from "pre-determined" phenomena? Prof. Huxley's example, quoted from Darwin's "Life and Letters," is critically dealt with, and the author tells us that this is "utterly wide of the mark. The phenomena here described [a storm at sea] end with themselves, they lead to nothing else; nothing follows from them. They are mere effects, and not, so far as we know, a means to obtain a result beyond." The insight which the author appears to have gained into the motive, or want of motive, in nature is really most enviable; the man of science who must perforce arrive at his conclusions by the circuitous roads of observation and experiment can only look with admiring wonder upon a method which is so completely foreign to his philosophy.

This same dummy, chance, is well belaboured throughout; among the slain, after this first tilt, we find not only Prof. Huxley, but Andrew Wilson, Oscar Schmidt, and, above all, Mr. Grant Allen, whose form is so terribly hacked that he appears to have been in the very centre of the fray, if not the chief object of attack.

Tilt the second is headed "Missing Links," and the onslaught begins upon Mr. Wallace, whose work on

"Darwinism" appears to have been published in the interval between the first and second essays. And here—perhaps not altogether disconnected with the appearance of Mr. Wallace's book—we find that the author has executed a series of mental evolutions with such skill that we have to rub our eyes in order to make sure that we have not deceived ourselves as to the position which he has actually taken. For natural selection, which, in the first essay, was considered to be so feeble as to be incapable of carrying on development through ten generations, even with the most favourable assumptions to start with, is now considered to be "as yet but hypothesis, and hypothesis which needs confirmation from fuller inquiry into the facts of the case, just as much as the other hypothesis of the continuity of forms between one species and another." At any rate, we seem to be justified in concluding from this that, as a scientific hypothesis, natural selection ranks with evolution, which, we were told in the first essay, had a right to be so called. The change of front has been very skilfully made, but that there has been a change is evident from the foregoing extracts.

The way in which evidence, which has been hitherto considered as fairly good from the evolutionist's point of view, can be manipulated so as to bear the quite opposite interpretation, is a study in intellectual jugglery which might be worthy of serious attention by certain classes of politicians. The second essay furnishes several examples of such feats. More especially may attention be called to the remarkable way in which the palæontological evidence is thus disposed of, and still more remarkable is the author's Podsnappian dismissal of the embryological evidence. Wallace's later treatment of natural variation is accepted:—

> "The variations of form and structure which occur among wild animals—and the same is to be said for plants—are not occasional and minute, but incessant and important. There is clearly an end of the objection . . . based on the supposed infinitesimal character of variations."

But if the reader fondly imagines that this admission brings the author any nearer to Darwinism he will be grievously mistaken. For in this larger and more widely divergent variability Father Gerard sees a "centrifugal tendency" by which "every varying climate and soil and circumstance on the face of the globe should make its own species; or rather there should be no species at all, but a fleeting and evanescent succession of individual forms, like the shapes of clouds in a windy sky." Of course, evidence has to be adduced in disproof of this astonishing result, to which the later study of variability has led us, or rather should have led us. But there is no difficulty at all about this: the house sparrow and the water-crowfoot, we are told, are widely distributed over the face of the globe, and yet retain their specific forms and characters. True; but the instances of cosmopolitan species retaining their distinctness are few and exceptional; we are not told anything about local forms and races, or about "representative species"; we hear nothing about widely distributed species which merge imperceptibly into each other to the utter confusion of those who make species their particular study. Can it be that these facts are inconvenient and "not to be endured"? or has the author discovered some absolute criterion of species? If the latter is the case, he can hardly be congratulated on his definition:—

> "It would seem to be simpler and plainer to say that a species is a *permanent group* [italics mine] of plants or animals framed in all particulars after a single type."

Enough has been said about this work to indicate its general tendency: its tone, on the whole, is antagonistic to evolution, but with respect to the special Darwinian form of this theory antagonism but feebly expresses the author's attitude. In each essay, the attack generally centres upon one or two representative writers; *e.g.* the third essay ("The Game of Speculation") dealing with Mr. Wallace, the fourth ("The Empire of Man") with Prof. Huxley, the fifth ("The New Genesis") with Messrs. Grant Allen and Edward Clodd, and the sixth ("The Voices of Babel") with a number of miscellaneous authorities, such as Mr. Herbert Spencer, Mr. Frederic Harrison, the late Prof. W. K. Clifford, and Sir James Stephen, of whom the author makes horrid examples by the very simple expedient of pitting their opinions against each other. From this general view, it will be seen that, so far as science is concerned, the effect of Father Gerard's last production will be practically *nil.* Among certain classes of general readers it may be mischievous, but we do not imagine that the mischief will spread very far. As the criticisms are for the most part destructive, it is impossible to attempt to deal with them in detail in these columns. Where it is possible to glean a vestige of a constructive idea, it will be seen that the main point towards which the author appears to be driving is that the doctrine of evolution—especially in its Darwinian form—is destructive of the notion of preconceived and determinate "plan," *e.g.*:—

> "Intrinsic forces working definitely towards one play not indeterminate forces swept hither and thither by external agencies like a cloud of dust, are suggested by the phenomena of nature."

We have become so accustomed to this style of criticism from all kinds of anti-evolutionary writers that it is almost superfluous to attempt to deal with it again. But it may really be asked whether those who are so constantly dinning this idea of a "plan" in nature will now condescend to give us some idea what that plan is. If "intrinsic forces are working definitely towards one plan," surely the author to whom has been permitted this glimpse into the inner sanctuary might enlighten the outer darkness a little by telling us something about the general scheme, or, at any rate, by giving us a notion as to the method by which he has arrived at such an important conclusion. On the other hand, if the author is satisfied that there is such a pre-arranged plan—whether he reveals that plan to the uninitiated or not—I, for one, fail to see how evolution, Darwinian or otherwise, has anything to do with the matter. If Father Gerard has managed to extract from the writings of popular authors, this notion of antagonism between ideas which are not necessarily antagonistic, with these authors must rest the responsibility. It cannot be said that the castigation which he has inflicted is altogether unmerited; there has been a great deal of crude and hasty speculation perpetrated in the name of evolution, and the blows aimed do occasionally tell in the right direction. Had Father Gerard not sacrificed his position by aiming so much at smart writing—had he favoured us with more solid thought instead of endeavouring "to split the ears of the groundlings"—his lucubrations would have received more respectful attention. But satire and cynicism, interspersed with ridicule, are not the best methods for securing consideration from men of science, and it is surprising that the author should have resorted so largely to their use. R. MELDOLA.

THE LAWS OF FORCE AND MOTION.

The Laws of Force and Motion. By John Harris (Kuklos). (London: Wertheimer, Lea, and Co., 1890.)

IN his preface the author, very rightly, sounds a warning note against the arrogance of Conventional Science, in its tendency to become ultra-conservative, intolerant, and extremely dogmatic.

But Real Science will always welcome and encourage attack and contradiction, feeling sure that Truth will ultimately prevail in the consensus of the majority who have devoted themselves dispassionately to the consideration of the facts in dispute. "Transibunt multi et augebit Scientia."

We presume the author would not ask to be judged with more leniency than he has displayed for the opponents he has singled out; so we may say at once that, after careful winnowing, we have not secured those grains of fact and truth which we were led to expect.

The experimental apparatus described seems carefully constructed and suitable for exact measurements; but does not differ essentially from that employed by Smeaton more than 100 years ago. However, the author assumes the true scientific sceptical spirit, in refusing to accept implicitly the statement of theoretical laws without putting them to the test of practical experimental verification.

Mathematicians will understand the nature of the author's attacks on Conventional Science from the specimen on p. 31:—

> "It would seem that, some time ago, a highly influential party of natural philosophers (Leibnitz, the two Bernoullis, &c.) entertained and supported the idea that the momentum of a moving body varies as the square of the velocity. This idea or conclusion was probably based on an inference, that, since a double velocity of the resistance required four times the force to produce it, four times the momentum must have been imparted to the resistance."

After this wavering as to the meaning of momentum, we are quite prepared to find (p. 60) that the author is of the school who declare that the moon does not rotate.

The author cannot decide between 16·1 or 32·2 for the value of g (p. 24); and cannot settle in consequence whether the normal acceleration in a circle is the squared velocity divided by the radius or by the diameter (p. 19).

"Tangential force" is, in the author's opinion, a more correct scientific term to use than "centrifugal force," although he allows that the latter is hallowed by long usage; but in his treatment he enunciates a theorem on p. 21, "The actual lineal ratio of the sine to the arc, when the arc is an octant, is 9 to 10," quoted from his own "Treatise on the Circle and Straight Line"; this makes $\pi = 2\sqrt{2} \div 0{\cdot}9$, a result worth recording by collectors of mathematical curiosities.

We hoped to find something combative in the articles on the Tidal Effect of Lunar Gravitation (p. 57), and on the Moon's Gravitative Influence at the Equatorial Surface of the Earth measured by Pendulum Oscillations (p. 76), considering that even the great Abel went astray in his theory at this point; but our author confines himself to vague generalities.

He would perform a valuable service to Science if he employed his experimental skill in observing the effect of Lunar Gravity on the Seconds Pendulum, as Conventional Science asserts that this effect does not amount to more than a rate of one 200th of a second in the day, although so noticeable in the Tides.

"Some Propositions in Geometry," by the same author, is advertised at the end of the book, whereof the Trisection of the Angle, the Duplication of the Cube, and the Quadrature and Rectification of the Circle, occupy the chief part; but we wonder whether the author has quite settled in his Geometry that the versed sine (or vertical height) is proportional to the chord, in a circle (p. 71). This might have been a misprint, but that the author adds immediately a numerical illustration, by saying that, if the chord is duplicated, the versed sine is also duplicated.

And this homely mode of verifying a law of comparison, by halving or doubling some quantity, and then observing the consequent change in the phenomena, is the single idea we consider worth lifting from the book, for general purposes of convincing argument and illustration of a mathematical law. A. G. G.

OUR BOOK SHELF.

An Introduction to the Mathematical Theory of Electricity and Magnetism. By W. T. A. Emtage, M.A. (Oxford: Clarendon Press, 1891.)

THE want of a text-book especially designed for the use of candidates for examinations in which a knowledge of the more elementary portions of the mathematical theory of electricity and magnetism is demanded has been felt for some time. Though the absence of such a book has caused some inconvenience, we are not at all sure that it has been detrimental to the study of electricity, for hitherto the candidate for a mathematical examination in electricity has been compelled to learn the subject from books such as those of Maxwell, or of Mascart and Joubert, in which electricity is treated as what it really is outside the examination-room—a subject in which mathematics and experiment are closely mixed and mutually helpful: it is to this that, we think, is to be ascribed a good deal of that interest which electricity, above all other subjects, seems to excite in its students. When, however, the analytical parts of the subject are divorced from the experimental, we do not believe they will be found to excite any special enthusiasm, or that the result will be much more interesting than an ordinary text-book for the Mathematical Tripos on, say, hydrostatics.

There is no doubt, however, that there is a demand for a text-book suitable for examination purposes, and this demand will, we think, be well met by the book before us. The scope of the work may be described by saying that it includes nearly all the analytical parts of Maxwell's larger treatise which do not involve analysis higher than the simpler parts of the differential and integral calculus;

it thus covers the portions of electricity and magnetism which, under the new regulations, are selected for examination in Part I. of the Mathematical Tripos, and we have no doubt it will be found useful for that examination. The book is very well arranged, and the explanations are generally clear and concise. Among some minor points which, we think, might with advantage be altered in subsequent editions are the following. When discussing the rapidly alternating currents produced by discharging a Leyden jar, the author says: "We do not know, for instance, whether we are right in supposing the currents to be the same throughout the conducting wire." This seems an unnecessary affectation of ignorance, for we do know that such a supposition is certainly wrong. The method of determining "v" by repeatedly charging and discharging a condenser placed on one arm of a Wheatstone's bridge is not given, though several other less accurate methods are described. This is the more singular as the method itself is given in another part of the book as one for determining the capacity of a condenser, but no hint is given of its most important application. The method of measuring the self-induction of a coil, which is ascribed to Lord Rayleigh, is really due to Maxwell, and, though not in the treatise on "Electricity and Magnetism," is given in the paper on the "Dynamical Theory of the Electro-magnetic Field."

Le Sommeil et le Système Nerveux: Physiologie de la Veille et du Sommeil. Par S. Serguéyeff. (Paris: Felix Alcan, 1890.)

IT is difficult to understand why a writer upon the higher branches or outlying districts of neurology should assume that his readers are totally ignorant of the rudiments of that science, and should occupy nine-tenths of his book with a description of the anatomy and physiology of the nervous system. If, indeed, for the purpose of throwing new light upon his subject, he presented his facts in a new form, or taught them from a novel point of view, or arranged them so as to bring out some new principle, then there might be an excuse for restating the facts; but even then a brief summary would be enough for the purpose, there would be no need for the rediscussion of settled theories and the requotation of trite authorities. Scarcely ever do we find a writer on neurology who is content to assume that his readers are acquainted with the alphabet of his subject, or who will refrain from inflicting upon them the wearisome account of cells and fibres, of corona and cortex, illustrated by the familiar engravings that have done duty in so many previous books. The vicious habit is common enough and bad enough, but very rarely is it carried to such an extent as in the book before us, in which only about three hundred out of the seventeen hundred pages of which it is composed are devoted to the subject of which it is said to treat; the great bulk of the book being occupied by anatomical and physiological descriptions which are not in this case even relieved by illustration. So far is this system of padding carried, that the author has even inserted, in his book on waking and sleeping, descriptions of the minute structure of the retina, of the internal ear and the organ of Corti. When we have at last waded through his pages of preliminary matter, we do not find that he presents any fresh theory of sleep that is worth considering, or that he has any new facts to bring under our notice. It is a shame that a student should be trapped by an enticing title into spending his time in reading such stuff.

Elementary Science Lessons. By W. Hewitt, B.Sc. (London: Longmans, Green, and Co., 1891.)

THE thirty-six object-lessons contained in the present volume form the third part of a scheme of lessons drawn up by the author at the request of the Liverpool School Board. They are designed for children of Standard III., and are in continuation of others given in previously published volumes suitable for Standards I. and II. The author's long experience in teaching science to children in elementary schools gives him the ability which is necessary properly to draw up such a course as the one before us. For the most part the facts and principles dealt with relate to the classification of bodies into solids, liquids, and gases, and with the changes from one of these states to another. The experiments described may be performed with the simplest of apparatus, and the inferences to be drawn from them must be manifest to all children for whom the work is intended. Whenever possible, the principles considered in the lessons are applied to explain physiographical phenomena, thus aiding the development of that intelligent observation which is the soul of science. The arrangement of the matter is generally good, and elementary school teachers will find in the work exactly what they require for their pupils.

Solutions of the Examples in Charles Smith's "Elementary Algebra." By A. G. Cracknell. (London: Macmillan and Co., 1891.)

MR. SMITH'S small "Algebra" has deservedly obtained high favour in our schools for its lucidity. The work before us aims at presenting the solutions, not always necessarily in the shortest way, but rightly so as to "follow naturally from the formulæ and theorems with which the student is acquainted at that stage." It has Mr. Smith's *imprimatur*, for he has revised the sheets; and from our own examination of it we can commend it to teachers and students.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Anatomy of Heloderma.

THE number of NATURE for July 30, which I have only just seen, contains (p. 295) a criticism of a statement of mine, to which I have to answer. It is stated in my paper on the osteology of Heloderma that there are eight or nine premaxillary teeth in *H. horridum*, and only six in *H. suspectum*, because such is the number in the specimens examined by me. As Dr. Shufeldt has, on re-examination, found eight teeth in a specimen of the latter species, I admit that the distinction, as a specific character, does not hold good. It is just because my figures are *not* diagrammatic that they represent fewer teeth than are mentioned in the text; to anyone familiar with the dentition of lizards and snakes, it is clear enough that some of the teeth have been lost, and they were therefore not represented in the figures, which are faithful representations (in outline) of the objects from which they are drawn. I am much surprised at Dr. Shufeldt's statement, that he "cannot conceive of any lizard normally having but nine teeth in its premaxillary bone; it should at least be an even number." I could refer him to no end of examples of premaxillary teeth normally in odd number among lizards with single premaxillary; perhaps the best known is afforded by the family *Amphisbænidæ*. I must again correct Dr. Shufeldt on a matter of fact: my figure of *H. horridum* shows *seven* teeth, not *six*, as he states in his letter; and that of *H. suspectum five*, not *four*. G. A. BOULENGER.

British Museum (Nat. Hist.), August 28.

A Straight Hand.

ALTHOUGH my writing master, who was an Englishman, taught me slanting letters which old habit still clings to, I approve highly what you say against it (NATURE, August 6, p. 325). Allow me to add some remarks on another side of that question. For many years past I have had in succession several amanuenses, and my first care has always been to require a straight hand without any distinction between up and down strokes. These precepts and a few minor ones have been easily followed in all cases. I inclose a few lines copied from your interesting journal by a youth who does not understand English: he would have done this work with more care had he known that I wanted merely a sample. At all events it is most easy to read.

Straight letters without hair lines give the reader a comfortable facility which is a far greater compliment to a correspondent than the "*dear* Sir" imported from England to France during the last fifty years. We suppose that slant writing has been invented on your side of the Channel, and we call it therefore *écriture anglaise.* However, experience seems to show that it is more easily deformed than a straight one, and that it degenerates often into an illegible scrawl, causing much loss of time, or even, what is worse, a tiresome amount of perplexity and worry. We are told that the schoolmaster is abroad, but I am afraid that he leads our children on a false trail far away from the main aim of writing, which is *legibility*. Is the invention of typewriters the antidote or the outcome of illegible slants? Some of your philosophers may answer this question while giving a wholesome lesson to the schoolmaster.

A. D'ABBADIE (de l'Institut).

Abbadia, Hendaye, France, August 16.

Cordylophora lacustris.

IN NATURE for June 4 (p. 106) Mr. John Bidgood recorded the presence of this Hydrozoon in vast numbers on submerged roots and stems in the Ant, Bure, and Thurne. Till then its only known Norfolk locality was that given in Allman—"an agricultural drain near Lynn Regis." This summer innumerable colonies were to be seen on weed floating on the surface on both sides of the Thurne from Ludham Bridge right up to Hickling Broad. A boatman told me he had seen "them insecs" every summer for many years past. Mr. Edward Corder, the Secretary of the Norwich Natural History Society, took some early in June, and some, which he was good enough to send me, is still living in a 4-ounce bottle. All the authorities state that Cordylophora is a "light-shunning animal," and the localities hitherto recorded certainly warranted such a conclusion. But the colonies taken from the surface of the water by Mr. Corder, and those I took some time later, were stronger and cleaner than those obtained from below the surface. I distributed some of the gathering which I brought back to London, and learn that it is all doing well in ordinary aquaria. Some that I sent to Mr. Bolton for distribution unfortunately died in transit. One large colony, some eight inches long, on the stem of a Potamogeton, was kept in the shade for a fortnight; the tubes became flaccid, and the hydranths pendent, but they revived within twenty-four hours when exposed on the ledge of a window with a western aspect. This seems to point to a change of habit. All the colonies were doubtless founded below the surface of the water, and the weeds, when cut to clear the fairway for wherries, were floated up towards Hickling Broad by the tide. But if reproduction takes place—as it certainly does—under these conditions, is it not probable that we shall have a race tolerant of direct light, if not as sensitive thereto as *Hydra vulgaris?* HENRY SCHERREN.

5 Osborne Road, Stroud Green, N., September 3.

Absolute and Gravitation Systems.

THE present condition of things is such that students of engineering need familiarity with, and ability to use, both systems o measuring force and related quantities. It seems necessary, therefore, that the transition from one system to the other should be kept clear of complications, and be presented as the simple matter which it really is. But in two text-books which have come to my notice, each offering points of excellence, and both evidently written by competent hands, a change in the unit of *mass* occurs in passing from the absolute to the gravitation system. The unit-mass is defined as the mass in which unit-acceleration is produced by unit force, which, of course, gives about 32 pounds as the mass-unit for the British gravitation system.

There is, in my opinion, much that is undesirable about this method of statement; the new mass-unit appears quite artificially in this one only of the many uses of the conception of mass, for the purpose, I suppose, of making it possible to put in generally applicable form such statements as: "Force is measured by change per second in momentum." My particular objection to it, however is that it locates the point of divergence among the fundamental units instead of among those derived from them. Does it not seem preferable to begin with units of mass, length, and time; to construct derived units, and to make common use of these as far as possible, postponing the differentiation of the two systems till the moment when it actually occurs? Surely it has been pointed out often, since the days of early exposition of these matters by Maxwell, Tait, and others, that the *force-unit* is the first cardinal point of difference, and that the absolute system simplifies here, while the gravitation system adopts another convention, which may be called arbitrary as opposed to the simpler one fixed upon by its rival.

In the hope of hastening the day of agreement in presenting the connection of ideas which underlie so much of modern physics and its applications, I have thought it permissible to state in summary, and for British units, the scheme used in my own teaching of mechanics. The claim is not advanced that the numerical work becomes different; indeed, the appended table is equally valid whichever basis be chosen; but there does seem to be a gain in logical clearness, as well as in what we may call historical accuracy.

Absolute System.—Fundamental units: foot, pound, second. Units of force, work, impulse derived in the usual way, so as to make proportional factors unity.

Gravitation System.—Fundamental units as before. Unit of force, the weight of one pound under circumstances specified to the required degree for scientific definiteness (locality, vacuum). Units of work and impulse connected with the force-unit, so as to make proportional factors unity.

The table shows the matter at a glance. g_1 is the value of g for the standard circumstances, and is to be regarded as a divisor in each case affecting the *product* of the other factors. The other symbols explain themselves.

Absolute.

$$\mathrm{P} = mp,$$

$$\text{(work)} \int \mathrm{P}ds = \text{(change in)}\,\frac{mv^2}{2},$$

$$\text{(impulse)} \int \mathrm{P}dt = \text{(change in)}\,mv.$$

Gravitation.

$$\mathrm{P} = \frac{mp}{g_1},$$

$$\text{(work)} \int \mathrm{P}ds = \text{(change in)}\,\frac{mv^2}{2g_1},$$

$$\text{(impulse)} \int \mathrm{P}dt = \text{(change in)}\,\frac{mv}{g_1}.$$

The choice of force-unit here affects what is logically subsequent to it, as it must; but leaves unaffected what is logically antecedent, as it ought.

So small a change as that of regarding g_1 as a divisor of m alone changes the basis of presentation; but there is an important difference of thought involved. FREDERICK SLATE.

University of California.

Eucalyptus as a Disinfectant.

IN a paragraph on the use of Eucalyptus branches for disinfection, as recommended by Baron von Mueller, you have unintentionally stated that to be the manner in which I have used Eucalyptus.

For the last two years I have used "Tucker's Eucalyptus Disinfectant" (a solution of antiseptics in the essential oil) in all cases of scarlet fever and diphtheria, and have not had one case of infection. In the former disease I have not used any isolation, and in most cases have not excluded the other children of the family from the sick-room. None of the cases, except two or three that were severe, were kept to their bed-room more than ten days; the isolation of six or eight weeks being unnecessary, as the cuticle is perfectly disinfected. This is accomplished by rubbing the disinfectant over the whole body twice and then once a day for ten days.

Baron von Mueller, in a letter I received from him, quite approves of my method of disinfecting by inunction. I read a paper before the Epidemiological Society last year on the subject. It is published in the Society's Transactions, and in a separate form by Mr. Lewis, of Gower Street. I also read a paper before the International Congress of Hygiene on antiseptic inunction. In this I have related the experience of other medical men in confirmation of my own. One, whose child had scarlet fever, placed his two other children in the same room, and kept them there for eight days, and they did not take the disease. This will be published in the Transactions of the Congress, and any one interested in the disinfection of infectious diseases, may obtain all the information they require from those two papers. J. Brendon Curgenven.

Teddington Hall, S.W., August 17.

Alum Solution.

One frequently reads, in accounts of experiments on the physical or chemical action of luminous rays, that a *solution of alum* has been used to absorb obscure heat radiations. An instance of this occurs in your description of the investigation by M. D'Arsonval (Nature, vol. xliv. p. 390). I should like to be informed if this practice is based upon actual evidence, or merely upon the supposition that, because alum itself cuts off a larger proportion of heat rays than any other easily available solid, its solution should be more effective than any other liquid. The only figures bearing on the question with which I am acquainted are those of Melloni, and he, as cited by Ganot, states the percentage of heat rays transmitted by alum solution as 12, and that by distilled water as 11. Why, then, not use distilled water? Harry Napier Draper.

Dublin, August 27.

A NEW KEYED MUSICAL INSTRUMENT FOR JUST INTONATION.

One of those subjects which periodically turn up for discussion, and then vanish for an interval of neglect, is the possibility of obtaining just intonation in the performance of music. Those who have studied theory, properly so-called, know very well that the series of musical sounds commonly used, as expressed on the pianoforte, do not give the true harmonic combinations on which the art is based, and many zealous attempts have been made to cure the evil. One of these, showing some novelty and much merit, is now exciting the attention of eminent musicians on the Continent; it was mentioned briefly in Nature of April 2 last (p. 521), and it may be interesting to many readers to give some further account of its general features. We may, however, preface this with a few words on the state of the question generally.

Although the equal division of the octave has now taken such a firm hold on modern musicians, it is only within a comparatively recent period that its use has become common. It was well known at an early date, but its defects checked its use until the general introduction of the class of instruments which have culminated in the pianoforte; the reason of its adoption then being that the want of sustaining power in the clavecin and the harpsichord so diminished the discordant effect as to make the faulty tuning endurable. People then began to get accustomed to it, and it was soon found that the system gave such extraordinary facilities for chromatic music, that the cultivation of this style became enormously developed. Hence the chromatic style and the equal temperament have become closely allied, and it is almost a matter of doctrine that the pianoforte division of the octave is a necessary element for the proper performance, or proper understanding, of the compositions of modern days.

For organs, the application of the equal temperament came much later. Down to about the middle of this century they were tuned on a system which gave the most usual keys fairly in tune, at the cost of an occasional harsh chord, which, for church purposes, was considered but a small price to pay for the general smooth and harmonious effect. But when highly skilled players began to increase, they required the organ to be more used for exhibition, and for this purpose the introduction of the equal temperament was deemed desirable. And so, as it thus commanded the two most powerful sources of music, it crept into use also by stringed instruments, orchestras, and voices, and so it has become general.

The consequence is that, now, practical musicians are in the habit of accepting the equal-tempered intonation as genuine and true music; and as the study of the principles of musical structure is by no means highly encouraged in this country, efforts are seldom made to undeceive them. Students are authoritatively told that questions about just intonation may be interesting to physicists and mathematicians as recondite problems in acoustical science, but that they have no bearing on "practical" music, and that, therefore, musicians need not trouble themselves about them. Some years ago, at a meeting of one of our musical educational establishments, it was said, "We do not here make music an affair of vibrations"—a sentiment which was received with loud applause.

No doubt some enthusiasts have carried the investigations on this subject to a degree of refinement which far outruns practical utility; and one can have little sympathy with those who delight in reviling and despising the duodecimal scale; seeing that it has been the means of materially advancing the art, and that the modern enharmonic system, founded upon it, has been so thoroughly incorporated into modern music that it is difficult to see how it could be now ignored.

But, on the other hand, one must, if one is to exercise reason and common-sense in musical matters, be equally at variance with the party who, arrogating to themselves the title of "practical" musicians, force on us the equal temperament to an extent which really means the extinction of true intonation altogether. We now, indeed, never hear it, and in fact only know by imagination what a true "common chord" means.

The principal objection to this state of things is that the ears of musicians become permanently vitiated, and lose the sense of accurate intonation, or the *desire to approach it*, which is tantamount to abandoning the most precious feature that modern music possesses—namely, beauty of harmony. A chord of well selected sounds, exactly in tune, is a very charming thing; but it is a thing unknown to ears of the present day. I can recollect the time when singers and violin-players strove to sing and play in good tune, and the effect of such unaccompanied part-singing, and such violin-playing, was very delightful. But now, music not being made "an affair of vibrations," one is often ashamed of the quality of what one hears; nobody seems to think purity of harmony, either with voices or violins or orchestras, to be a matter worth striving after.

It is surely a reasonable wish that this should be checked, but one must be reasonable in one's expectations. The pianoforte must certainly be let alone, and so must the organ when used for exhibitional purposes, though its cacophony under the present tuning detracts much from the pleasure of hearing such fine playing as is now common. But vocalists and violin-players ought to be encouraged, as of old, to sing and play in tune, and for this purpose what is wanted is an instrument which will keep up and circulate the tradition of what true music means. To attain this, therefore—*i.e.* to construct an instrument which shall enable a player, with moderate ease, to play polyphonic music, of moderately chromatic character, in strict tune—has been the aim of many ingenious musicians and mechanics.

I need not go into history. Everybody may see at South Kensington the wonderful enharmonic organ, built half a century ago by General Thompson, and may read of the instruments described by Helmholtz, and his voluminous commentator, the late Dr. Ellis; and the efforts in the same direction of Mr. Colin Brown, and of Mr. Bosanquet, who has devoted much attention to the matter, are worthy of all praise. But my object now is to describe the latest attempt of the kind, by a native of Japan, Dr. Shohé Tanaka. Persons who have lately had to do with that country have been well aware, not only of the natural ingenuity of the Japanese, but of the high standing which many of their youth have taken in scientific studies. Dr. Tanaka combines these two qualifications. After an industrious preliminary education in his own country, he went to Berlin, where he has been for five years studying physical and mechanical science under the best professors, and with these he has combined also a study of music. He has published, in the *Vierteljahrsschrift für Musikwissenschaft* for 1890, a long essay on the subject generally, which fully demonstrates his knowledge of it; and he appears to have made a very favourable impression in Germany. He exhibited his "Enharmonium," as it was called, to the Emperor and Empress, and he produces testimonials from many musicians of the highest rank, among whom are Joachim, Von Bülow, Reinecke, Richter, Fuchs, Moszkowski, the whole staff of the Leipzig Conservatoire of Music, and many others. These not only speak highly of the instrument, but (in strong contrast to the English authorities) earnestly support and recommend the object it is proposed to serve. Indeed, some of the testimonials are essays on the advantage of the cultivation of pure intonation. Von Bülow especially says:—

"I have requested the maker to make me such an enharmonium for my personal use at home. I am earnestly desirous to protect myself during the few remaining years of the exercise of my art against constantly possible relapses into already conquered errors. In order to make pure music it is necessary to think in pure tones. It is *de facto* the practically insuppressible conventional pianoforte-lie to which nearly all corruptions of hearing may be traced."

With these credentials the inventor has brought a sample of his instrument for examination in England, and I may proceed to give some idea of what it is like.

The great object to be aimed at is facility of performance. It is in this respect that most of the former instruments have failed; the multitude of notes has generally required a new kind of clavier, or the manner of manipulating them has been so complicated and difficult as to require a special learning attended with much trouble. The present instrument is a harmonium of five octaves, having a key-board modelled precisely on the usual pattern and size. Dr. Tanaka has greatly simplified the problem by adopting the transposing system, often adopted with pianofortes. Whatever key the music is in, it is played in the simplest of all keys, the key of C, and by means of a bodily shifting of the key-board to the right or left, it is set so as to act in the key required. It is, in fact, the principle used in the horn tribe; the horn or trumpet player reads and plays his music in the key of C, and the transposition of this to the key required is previously arranged as a part of the mechanism of the instrument; or, rather, as the author puts it, the music may be read and played on the tonic sol-fa system, and he might have adopted its symbols if he had not feared it would be too startling a change.

The points in which the new key-board differs from the ordinary one are, that the black keys are divided, some into two and some into three parts, and one additional shorter and narrower black key is introduced between the E and F white keys. This arrangement gives twenty notes, which suffice for modulating into a reasonable number of keys with sharp signatures.

To provide for modulations into keys with flat signatures, since these and the sharp modulations are not both wanted at the same time, six of the notes can be instantaneously changed for the purpose, at any time, in a manner hereafter explained.

The whole of the keys are well under the hand, and, if the performer knows which note he ought to use, he can take it in any usual chord without difficulty.

Fig. 1 represents one octave of the key-board as arranged for the key of C, with provision for modulating into keys with sharps.

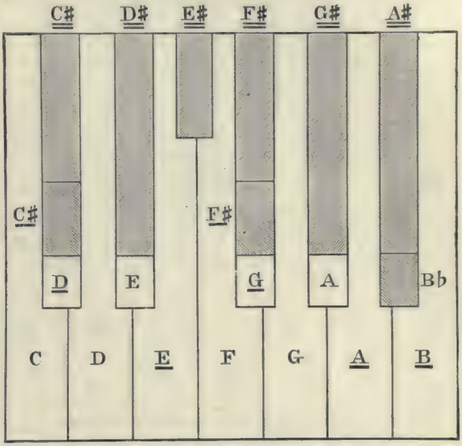
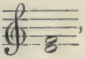
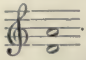


FIG. 1.—As arranged for modulation into keys with sharps.

In order to explain the exact intonation or musical position of the notes, the author adopts a notation already pretty well known—namely, when the letter indicating a note has no line above or below it, it is intended to correspond with what may be called the "Pythagorean" position of that note, which is given by a succession of fifths upwards from C as a base. If the letter has a stroke below it—thus, $\underline{\text{E}}$—it is a comma below that position; and if the stroke is above—thus, $\overline{\text{E}}\flat$—it is a comma above that position. Two strokes below—thus, $\underline{\underline{\text{C}\sharp}}$—indicate two commas below.

Now, in the first place it will be seen that the ordinary seven white keys indicate the seven ordinary notes of the major scale of C, according to the intonation usually understood, *i.e.* the major triads on the tonic, dominant, and subdominant, being perfectly in tune.

But as, for certain harmonies, variations of some of these notes are required, there are four alternative small white notes, $\underline{\text{D}}$, E, $\underline{\text{G}}$, and A, placed at the near extremity of four of the black ones. For example, the note $\underline{\text{D}}$ is the one required to make the true minor third , or the true fifth .

The position of the keys for the sharp notes, and also their intonations, will be seen in the figure. F♯ and C♯ each require alternative values, a comma distant from each other, and these are obtained by dividing the black keys in the manner formerly practised with some organs in this country.

It will be seen that there are in all twenty effective finger keys, each sounding a separate note.

When it is requisite to modulate into keys with flats, the above arrangement will not answer; and the necessary change is made by a lever placed conveniently for being worked by the knee of the player, like the swell of a harmonium.

When this is pushed over, the six hindmost black keys are altered from sharps to flats, as shown in Fig. 2. $\underline{C}\sharp$ and $\underline{F}\sharp$ still remain, and an alternative B♭ and an alternative $\overline{F}$ are added. This change gives six new notes, so that the total number of sounds used in the octave, for the key of C with its modulations, is twenty-six.

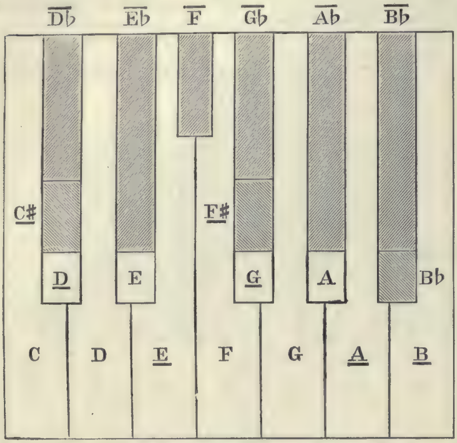


FIG. 2.—As altered for modulation into keys with flats.

As a further indication of the exact musical positions of these twenty-six notes, their ratios of vibration with the keynote C, may also be given. And the logarithms of these (here limited, for simplicity, to three places) will represent approximately the height of each note above C. In this scale, an octave is represented by 301, a mean semitone by 25, and a comma by 5.

Table of the Positions of the various Notes used for the Key of C.

	Ratio.	Logarithm.		Ratio.	Logarithm.
C =	1	... 0			
D =	$\frac{9}{8}$	... 51	$\underline{D}$ =	$\frac{10}{9}$	... 46
$\underline{E}$ =	$\frac{5}{4}$	... 97	E =	$\frac{81}{64}$	... 102
F =	$\frac{4}{3}$	... 125	$\overline{F}$ =	$\frac{27}{20}$	... 130
G =	$\frac{3}{2}$	... 176	$\underline{G}$ =	$\frac{40}{27}$	... 171
A =	$\frac{5}{3}$	... 222	A =	$\frac{27}{16}$	... 227
B =	$\frac{15}{8}$	... 273			
$\underline{F}\sharp$ =	$\frac{45}{32}$	... 148	$\underline{\underline{F}}\sharp$ =	$\frac{25}{18}$	... 143
$\underline{C}\sharp$ =	$\frac{135}{128}$	... 23	$\underline{C}\sharp$ =	$\frac{25}{24}$	... 18
$\underline{G}\sharp$ =	$\frac{25}{16}$	... 194			
$\underline{\underline{D}}\sharp$ =	$\frac{75}{64}$	... 69			
$\underline{\underline{A}}\sharp$ =	$\frac{225}{128}$	... 245			
E♯ =	$\frac{675}{512}$	... 120			
B♭ =	$\frac{9}{5}$	... 255	B♭ =	$\frac{16}{9}$	... 250
$\overline{E}\flat$ =	$\frac{6}{5}$	... 79			
$\overline{A}\flat$ =	$\frac{8}{5}$	... 204			
$\overline{D}\flat$ =	$\frac{16}{15}$	... 28			
G♭ =	$\frac{64}{45}$	... 153			

This information will enable any student of musical theory to judge of the capability of the instrument to play modern music with just intonation. The great object is, of course, to play the consonant triads, major and minor, in strict tune, and it will be found that the instrument, as above arranged, will play the following

Major Triads on—
C, D, E, F, G, A, B,
F♯, B♭, E♭, A♭, D♭, G♭,

Minor Triads on—
C, D, E, F, G, A, B,
F♯, C♯, G♯, D♯, A♯, B♭,

and some of each in duplicate with a comma variation. These would certainly seem sufficient for all ordinary music in C major or A minor.

By means of the transposing movement, the key-board can be set upon either of the eleven other keys, for which a similar modulating power is obtained, except in some very remote cases. In order, however, to effect this, ten additional notes are used, making thirty-six in all. But the adaptation of them is entirely automatic, and the mechanism for this purpose constitutes one of the chief novelties of the invention.

This is the provision for the purpose by the manufacturer. Now, let us see what the *performer* has to do.

In the first place, whatever key the original composition is in, it must be played in the key of C. In these days of strict examinations by the College of Organists, it is not uncommon to find players who can transpose at first sight from any key into any other. For players who cannot do this the piece will have to be re-copied, but this is nothing in comparison with the great gain in simplicity of the key-board.

Secondly, the performer has not only to play the music in the ordinary way, but he has another problem before him—namely, where certain notes are in duplicate, he has to decide which of the two to use. Now this, although by no means a difficult matter, requires some knowledge of the theory of music, in a sense beyond what is ordinarily taught. To explain it would lead us into more technical detail than would be proper here; but Dr. Tanaka, in compassion for those unfortunates with whom music "has not been made an affair of vibrations," has shown that the printed music can have certain very simple symbols prefixed to the notes, which will easily guide the purely "practical" player what to do.

In this way any competent organist, though he may never have heard of the system before, may, after a few minutes' explanation, and a quarter of an hour's practice, play any piece of music correctly in the true musical intonation, a result which, I believe, has never been attained by any former instrument, and which says much for the ingenuity of the whole contrivance.

It is recorded that the Emperor of Germany expressed a wish to see the experiment tried on a large organ, and the inventor is now engaged in constructing one with eight stops, and a simplified enharmonic pedal-clavier, for the Prussian Goverment.

WILLIAM POLE.

THE NEW AUSTRALIAN MARSUPIAL MOLE—NOTORYCTES TYPHLOPS.

OUR Corresponding Member, Prof. E. C. Stirling, of the University of Adelaide, has most kindly sent to us an original water-coloured drawing of the newly-discovered Australian Marsupial, prepared from a pencil sketch taken from life. The animal is represented upon the surface of one of the red sandhills in which it passes the greater part of its life, among some tussocks of *Ariodia irritans*, the "porcupine grass" of the interior of Australia, and is figured of the natural size. The drawing will be exhibited at the first scientific meeting of this Society in November next, but in the meanwhile can be inspected in our library by any naturalist who may wish to see it.

Prof. Stirling has also sent us a copy of his paper in the Transactions of the Royal Society of South Australia (read February 3 of the present year), in which this extraordinary animal is fully described. The subjoined particulars as to its habits, extracted from Dr. Stirling's article, will be interesting to the readers of NATURE:—

"It appears that the first specimen was captured by Mr. Wm. Coulthard, manager of the Frew River Station and other northern runs belonging to the Willowie Pastoral Company. Attracted by some peculiar tracks, on reaching his camp one evening on the Finke River, while traversing the Idracoura Station with cattle, he followed them up, and found the animal lying under a tussock of spinifex or porcupine grass (*Triodia irritans*). Though he is an old bush hand, with all the watchful alertness and powers of observation usually acquired by those who live lives of difficulty and danger, this was the first and only specimen of the animal he ever saw. As previously stated, this found its way to the Museum through the agency of Messrs. Benham and Molineux. The three subsequently received shortly afterwards, as well as the last lot recently secured by Mr. Bishop during our journey through the country, were also found on the Idracoura Station. This is a large cattle-run comprising several hundred square miles of country in the southern part of the Northern Territory of South Australia, which lies immediately to the west of the telegraph line between the Charlotte Waters and Alice Springs Stations. The great dry water-course of the Finke River, which runs from north-west to south-east, bounds the run for some eighty miles on the north and north-east. Its distance from Adelaide is, roughly speaking, a thousand miles. Flats and sandhills of red sand, more or less well covered with spinifex and acacias constitute a large portion of the country, and the rainfall is inconsiderable. Curiously enough, all the specimens of *Notoryctes* hitherto received by me have been found within a circumscribed area, four miles from the Idracoura Head Station, which is situated on the Finke watercourse itself, and almost invariably amongst the sandhills. I have it, however, on very fair authority, that the animal has been seen on the Undoolya Station, which lies immediately south of the McDonnell Ranges, and that one also was found drowned after heavy rain at Tempe Downs, a station situated about 120 miles west-south-west of Alice Springs. These points will sufficiently define its range, so far as is known at present. They do not appear to be very numerous. Very few of the white men in the district had ever seen it, even though constantly travelling; and not many of the natives whom I came across recognized the well-executed drawing I carried with me. It must be remembered, however, that I did not pass through the exact spot which so far appears to be its focus of distribution. Nor did a very considerable reward, which I offered, cause any specimens to be forthcoming between the first lot received, over two years ago, and that recently secured during my trans-continental trip. With a few exceptions, the animals have been captured by the aboriginals, who, with their phenomenal powers of tracking, follow up their traces until they are caught. For this reason they can only be found with certainty after rain, which sets the surface of the sand, and enables it to retain tracks that would immediately be obliterated when it is dry and loose. Nor are they found except during warm weather, so that the short period of semi-tropical summer rains appears to be the favourable period for their capture. For this suitable combination of wet and warmth, Mr. Bishop had to wait three months before he was able to get them, and in all cases they were found during the day-time. Perpetual burrowing seems to be the characteristic feature of its life. Both Mr. Bishop and Mr. Benham, who have seen the animal in its native state, report that, emerging from the sand, it travels on the surface for a few feet at a slowish pace, with a peculiar sinuous motion, the belly much flattened against the ground, while it rests on the outsides of its fore-paws, which are thus doubled in under it. It leaves behind it a peculiar sinuous triple track, the outer impressions, more or less interrupted, being caused by the feet, and the central continuous line by the tail, which seems to be pressed down in the rear. Constantly on the look-out for its tracks, I was often deceived by those of numerous lizards, which are somewhat similar in these respects.

"It enters the sand obliquely, and travels under ground either for a few feet or for many yards, not apparently reaching a depth of more than two or three inches, for whilst underground its progress can often be detected by a slight cracking or moving of the surface over its position. In penetrating the soil, free use as a borer is made of the conical snout with its horny protecting shield, and the powerful scoop-like claws (fore) are also early brought into play. As it disappears from sight, the hind-limbs, as well, are used to throw the sand backwards, which falls in again behind it as it goes, so that no permanent tunnel is left to mark its course. Again emerging, at some distance, it travels for a few feet upon the surface, and then descends as before. I could hear nothing of its making, or occupying at any time, permanent burrows. Both my informants laid great stress on the phenomenal rapidity with which it can burrow, as observed in both a state of nature and captivity."

To these notes of Prof. Stirling I may add the remark that this is certainly one of the most extraordinary discoveries in zoology made of late years. *Notoryctes typhlops*, as shown by Prof. Stirling's full and elaborate description and figures, is unquestionably a new and perfectly isolated form of Marsupial life, and must be referred to a new section of the order Marsupialia. We must all congratulate Prof. Stirling on his success in bringing before the world such an important novelty.

P. L. SCLATER.

Zoological Society of London, 3 Hanover Square, W.,
August 20.

FRANCIS BRÜNNOW, PH.D., F.R.A.S.

WE regret to have to announce the death of Francis Brünnow, whose fortune it was to earn in two continents a reputation as an ardent astronomer and an indefatigable observer and computer. He was not less distinguished as a Professor at Ann Arbor, Michigan, than when he filled the Chair of Astronomy at Dublin, and occupied the position there of Astronomer-Royal. He was fortunate in his early career. Nearly fifty years ago he was one of the band of earnest astronomers that Encke summoned round himself at Berlin, and thus he became the friend and companion of Galle, of Bremiker, and of D'Arrest. The time, too, was interesting. Adams and Leverrier had traced the existence of a Neptune, and the issue of that well-known drama was worked out under the eyes of the late Dr. Brünnow. He was present in the Berlin Observatory when Neptune was first recognized as a planet, and an early, if not the earliest, notification of its discovery, that reached this country, came from his hand.

It would be tedious to recall all the results that his untiring industry wrought in the department of cometary astronomy. His greatest and best-known work is his classical investigation of the motion of De Vico's comet of short period. The close and eager search that was made for this comet, particularly in 1855, was not successful, and its ultimate career is unknown; but this fact does not detract from the merit of Dr. Brünnow's memoir, on which a lesser reputation might rest. As a calculator of a high order, he will, however, be remembered for his work on the theory of some of the minor planets, as Flora, Victoria, and Iris—a work which to some extent was carried out during his Directorship of the Observatory of Ann Arbor, Michigan, to which he was appointed in 1854. Here, too, he published for a short time a periodical under the title of *Astronomical Notices*. This journal had but a short life, and judging from its rarity must have had but a small circulation. A very different fate attended the publication of his "Lehrbuch der sphärischen Astronomie," first issued in 1851, and which has passed through several editions, been more than once translated, and is everywhere recognized as an authoritative text-book.

In 1865, on the death of Sir W. Hamilton, Dr. Brünnow was appointed Andrews Professor of Astronomy in the University of Dublin and Director of the Dunsink Observatory. The important and original mathematical researches in which his illustrious predecessor had been engaged had not left him sufficient leisure to superintend with activity the affairs of the Observatory; and the work of organizing and of placing it on a modern footing, adequately equipped, fell to the lot of Dr. Brünnow, who proved himself admirably fitted for the task. The South object-glass, which had remained unmounted, was, under Dr. Brünnow's auspices, provided with an equatorial movement, and with it he carried out the researches in stellar parallax which marked alike his assiduity and his competence as an observer. This line of research, thus connected with the Observatory, his successor, Sir Robert Ball, has recognized and pursued with vigour and success. In 1874, Dr. Brünnow retired from the Directorship on account of failing health and eyesight, and he has since lived privately, principally abroad. He died at Heidelberg, in his sixty-seventh year, to the deep regret, not only of his numerous private friends, but of all those who have profited by his teaching, whether as members of his class or students of his valuable contributions to astronomy.

NOTES.

THE Australasian Association for the Advancement of Science will hold its fourth annual meeting at Hobart in January 1892. The first general meeting will take place on January 7, when Sir James Hector will resign the chair, and Sir Robert G. C. Hamilton, Governor of Tasmania and President of the Tasmanian Royal Society, will assume the Presidency, and deliver an address. Visits to places of interest in the immediate neighbourhood of Hobart will be made during the time when the meeting is being held, and afterwards there will be excursions to different places in Tasmania. Application has been made to the New Zealand Shipping Company, and to Shaw, Savill, and Albion Company, for passages at reduced rates to members of the British Association visiting Tasmania to attend the meeting at Hobart, and it is expected that this will be granted.

THE International Electro-Technical Congress was opened at Frankfort-on-the-Main on Tuesday. An address was delivered by Dr. Stephan, Imperial Minister of Post and Telegraphs. Some 650 members, of whom 198 were foreigners, attended the proceedings. After the usual complimentary speeches, the following gentlemen were elected Presidents of the various Sections of the Congress:—Herr Siemens, of Berlin Mr. Preece, of London; M. Hospitalier, of Paris; Signor Ferrares, of Turin; Herr Waltenhofen, of Vienna; and Herr Kohlrausch, of Hanover. It was decided that a special Section should be formed to consider the principles of legislation dealing with electro-technical matters.

THE Crystal Palace Electrical Exhibition, to be opened on January 1 next, has received the sanction of the Board of Trade, and is duly certified as an International Exhibition, under the provisions of the Patents, Designs, and Trade Marks Act, 1883. The exhibits of Her Majesty's Government will include historical telegraphic and electrical apparatus, instruments, and appliances, as well as the modern apparatus and instruments now in use in the Postal Telegraph Department. This exhibit will be arranged under the direction of Mr. W. H. Preece, F.R.S.

IT has been suggested in America that steps should be taken to secure an International Conference of Electricians at the "Columbian World's Fair." "The time and place," says the new Chicago journal, *Electrician*, "are certainly auspicious, and as there are many questions in electrical science that are now awaiting adjudication it would seem that it were only necessary that the invitation be made by the properly constituted bodies to have it meet with the hearty approbation of scientific men everywhere. Could such a Convention be assembled it would do more than any other agency to bring together at the Columbian Exposition the most complete and varied display of electrical apparatus the world ever saw."

THE International Agricultural Congress was opened on Monday at the Hague by M. Méline, the President, who briefly reviewed the labours of the Paris Congress, dwelling upon its great importance to agriculture in general, and pointing out that the results obtained by that meeting would assist the various Governments in the legislative, administrative, and financial problems requiring solution. The conclusions arrived at in Paris were, however, not final, and would be more precisely defined by the present assembly.

WE have received an intimation of the sudden death, from apoplexy, of Dr. L. Just, Professor of Botany at the Polytechnikum, Carlsruhe, and Director of the Botanic Garden belonging to the same institution. Dr. Just was best known to the botanical world through the *Botanischer Jahresbericht*, which has appeared under his name since its foundation in 1874 up to the present time, though he resigned the editorship in 1885.

MR. CHARLES JAMRACH, well known as an importer, breeder, and exporter of all kinds of animals, died last Sunday at his residence in Bow. He was of German parentage, and inherited from his father the business which he conducted with so much energy and intelligence. Many scientific collections, as well as travelling menageries, have been enriched by him with valuable specimens. He showed particular interest in the breeding of long-coated Persian greyhounds, Japanese pugs, and Madagascar cats. The collection he had last formed includes, the *Times* says, young lions, tigers, and dwarf cattle from Burmah.

THE number of visitors to the South Kensington Museum during the last month exceeded 120,000. This is the largest number in any one month since 1883, in which year the Fisheries Exhibition was held opposite to the Museum, on the ground formerly occupied by the Royal Horticultural Society.

THE Staffordshire County Council have appointed Prof. D. E. Jones, B.Sc. (of the University College of Wales, Aberystwyth), as Director of Technical Instruction for Staffordshire.

THE Oxford Delegates responsible for the University Extension work have just published their Annual Report for the year ending July 31, 1891. No fewer than 192 courses of lectures were delivered. Of these, 90 were on historical subjects, 64 on natural science, 33 on literature and art, and 5 on political economy. These figures show a small increase in the number of courses on history and literature, and evidence a marked increase in the attention that is being paid throughout the country to natural science. On the other hand, political economy does not appear to be popular with those who are responsible for the arrangement of the lectures, and this circumstance the Delegates regret. At several centres in the North of England the courses have been regularly attended by many hundreds of artisans, and the funds to defray the expenses of these lectures have been provided by working men societies. The results of the examinations have in many cases been most satisfactory. In the opinion of Mr. York Powell "The paper classed as distinguished would have been accepted in Oxford as distinctly belonging to the honour class; the 'pass' standard is that which would be adopted in the Oxford pass school." Mr. Lodge and Mr. A. H. Johnson bear similar testimony to the efficiency and capacity of the students.

THE *Times* has been printing an interesting correspondence on county museums, and we may hope that the discussion will lead to some practical results. There can be no doubt as to the need for such institutions. Properly organized, they might be of high educational value, and they would preserve for posterity many objects of archæological interest which are now in danger of being either destroyed or lost. The aim of the proposed museums ought, however, as Prof. Flower has urged, to be very clearly defined, and it would be necessary that arrangements should be made for the preparation of good catalogues and labels.

EVERYONE interested in the scientific aspects of agriculture was sorry to hear that Miss Ormerod had felt it necessary to resign her position as consulting entomologist to the Royal Agricultural Society. It is much to be regretted that misunderstandings should have led to the severance of her connection with the Society with which she has so long been honourably associated. Fortunately her work as an entomologist is not to be interrupted, and she will continue to place her knowledge at the service of agriculturists.

THE Department of Agriculture in New South Wales is not likely to complain of lack of work. During the first three months of the Department's existence—March to May 1890—1200 letters were received from farmers and others on matters of agricultural interest; during the same months of this year, 2300 were received and fully answered. During the first five months of the current year, over 1000 letters were written by the Department, giving specific advice on manures, analysis of soils, insect pests, and parasitic diseases, and were gratefully acknowledged; 18,000 Gazettes and Bulletins were distributed, and 7000 circulars sent out.

IN the official statement relating to the work of the British Museum (Natural History) during 1890, reference is made to two new cases which have been placed in the central hall. One of them illustrates external variation according to age, sex, and season, as exemplified in the well-known bird the Ruff (*Machetes pugnax*). The other case is intended to illustrate the subject of protective resemblance and mimicry. The lower part of the case is occupied by a group showing the simplest form of such resemblance, *i.e.* general conformation of colour to habitual surroundings. Various species of mammals, birds, and reptiles, from the Egyptian desert, are arranged upon a ground consisting of the actual rocks and sand among which they were living. These specimens were collected in February 1890, and presented by Mr. F. S. Worthington. In the upper part of the case specimens are exhibited, chiefly from the class of insects in which the imitation both of the form and colour of external objects is carried to various degrees of perfection and complexity. Among these is a group of Indian butterflies (*Kallima inachis*), which, when at rest with their wings closed, present a marvellous resemblance to dead leaves. Still further stages of complexity of imitation are shown in insects which closely resemble, externally, others belonging to different families or even orders, apparently for purposes of protection.

M. E. HECKEL, of Marseilles, has recently described an interesting case of mimicry which may be frequently seen in the south of France. The mimic is a spider, *Thomisus onustus*, which is often found in the flowers of *Convolvulus arvensis*, where it hides itself for the purpose of snaring two Diptera, *Nomioides minutissimus* and *Melithreptus origani*, on which it feeds. Convolvulus is abundant, and three principal colour-variations are met with: there is a white form, a pink one with deep pink spots, and a light pink form with a slight greenishness on the external wall of the corolla. Each of these forms is particularly visited by one of three varieties of *Thomisus*. The variety which visits the greenish form has a green hue, and keeps on the greener part of the corolla; that which lives in the white form is white, with a faint blue cross on the abdomen, and some blue at the end of the legs; the variety which lives in the pink form is pink itself on the prominent parts of the abdomen and legs. If the animal happens to live on *Dahlia versicolor*, the pink turns to red, and if it lives in a yellow flower—*Antirrhinum majus*, for instance—it becomes yellow. At first Prof. Heckel supposed the three varieties of *Thomisus* to be permanent, but he discovered accidentally that any one of these peculiarly coloured spiders, when transferred to a differently coloured flower, assumes the hue of the latter in the course of a few days; and when the pink, white, green, and yellow varieties are confined together in a box, they all become nearly white.

MR. THEODORE BENT, according to a telegram received from him at Cape Town, has good reason to be satisfied with the results of his investigation of the Zimbabye ruins. He is of opinion that the "finds" unmistakably indicate the form of worship, the manner of decoration, and the system of gold smelting practised by the vanished people who inhabited the buildings. He is now visiting other ruins.

THE series of "One Man" photographic exhibitions at the Camera Club is to be continued during the coming winter. According to the Journal of the Club, there will first be an exhibition of photographs by Mr. Ralph W. Robinson. This will be followed by an exhibition of the work of Mr. J. P. Gibson, of Hexham.

AT a meeting of the Meteorological Society of Mauritius on July 30, it was stated that, on June 13 and 14 last, thunderstorms occurred in that island. This, so far as was known, was the first instance of a thunder-storm having taken place since the year 1801. There was a considerable increase of sunspots at about this time, and on June 14 a remarkable magnetic disturbance took place. Photographs of the latter part of the transit of Venus, on May 10 last, were exhibited. At sunrise the planet had already traversed about one-half of its apparent path, and its appearance was perfectly round and intensely black. The time of tangential contact (at egress) was, as nearly as could be ascertained, 8h. 36m. 36s. A number of charts showing the winds and weather experienced by several vessels which encountered cyclones in December, January, and February last were submitted; the greatest of the disturbances which had been experienced of late occurred from February 3–13. At the Observatory the barometer fell from 29·962 inches, at 9h. a.m. on the 1st, to 29·409 inches, at 3h. 25m. a.m. on the 6th. Full details of these cyclones will be published. With reference to the "Atlas of Cyclone Tracks," lately published by the Meteorological Council, Dr. Meldrum stated that the preparation of an appendix was under consideration.

THE Report of the Meteorological Commission of Cape Colony for the year 1890 contains the results of observations taken at 45 principal stations, and monthly and yearly rainfall values at about 300 stations in the colony and neighbouring States. The observations are made chiefly by public officials, and by private gentlemen who lend their aid. Summaries from a selected number of rainfall stations are also published monthly in the *Government Gazette* and in the *Agricultural Journal*. The expenditure for the year was only £378, so that, considering the smallness of the funds available, the results obtained are highly satisfactory; and the cost of instruments, which become the property of the observers after 5 years' continuous observations, is not inconsiderable. The Commission express the hope that their labours may lead to the discovery of the laws which govern the weather in those parts, and ultimately result in the issuing of trustworthy storm warnings. With this view simultaneous observations from various stations are telegraphed to various ports, where they are entered on sketch maps for the information of mariners and others.

A CORRESPONDENT informs us that Dr. Sleich, of Berlin, has found that the subcutaneous injection of distilled water produces sufficient local anæsthesia at the point of insertion to allow small operations, such as opening a boil, to be made without pain.

THE following are some results of Herren Elster and Geitel's recent electric observations on the Sonnblick, described to the Vienna Academy:—The intensity of the most refrangible solar rays, measured by their discharging effect on a negatively electrified surface of amalgamated zinc is about doubled on rising 3100 m. from the lowland. The authors were unable to find other actino-electrically active substances; even pure fresh snow and dry Sonnblick rock were not perceptibly discharged by light. Waterfalls may produce in a valley a negative fall of potential, and to considerable heights (500 m.). The morning maximum in fall of potential, observed regularly between 7 and 9 a.m. in the plain and in Alpine valleys, was absent at 3100 m. Before thunderstorms in July, the positive fall of potential sank gradually, in light showers, to *nil*, at which it remained sometimes two or three hours till completion of the electrical process in the cloud. In thunder-clouds, or on low ground, during a thunderstorm, the atmospheric electricity usually changes sign after a discharge. St. Elmo's fire (negative as often as positive) always accompanied thunderstorms. The observation that negative St. Elmo's fire burns with blue flame, positive with red, was repeatedly confirmed.

IT is well known that the fox possesses an excellent "head for country." Referring to this subject in an interesting article in the current number of the *Zoologist*, Mr. Harting says a fox has been known to return seventy miles to his "earth," and this not once, but three times. He was caught in Yorkshire, and sent into Lancashire to be hunted by the hounds of the late Mr. Fitzherbert Brockholes, of Claughton Hall, Garstang, and his identity was established by his having been marked in the ear by the fox-catcher. This story Mr. Harting had from his friend Captain F. H. Salvin, who was living in Yorkshire at the time, and was well acquainted with Mr. Brockholes, who gave him all the details.

DURING the nesting season the male ostrich seems to be anything but an agreeable creature. In a paper lately read before the Royal Society of Tasmania, Mr. James Andrew says that at that period the bird is most pugnacious, and may only be approached in safety with great precaution. He resents the intrusion of any visitors on his domain, and proves a most formidable opponent. His mode of attack is by a series of kicks. The leg is thrown forwards and outwards, until the foot, armed with a most formidable nail, is high in the air; it is then brought down with terrific force, serious enough to the unhappy human being or animal struck with the flat of the foot, but much worse if the victim be caught and ripped by the toe. Instances are known of men being killed outright by a single kick, and Mr. Andrew remembers, whilst on a visit in the neighbourhood, that on a farm near Graaff Reinet a horse's back was broken by one such blow aimed at its rider. If attacked, a man should never seek safety in flight; a few yards and the bird is within striking distance, and the worst consequences may result. The alternative is to lie flat on the ground, and submit with as much resignation as possible to the inevitable and severe pummelling which it may be expected will be repeated at intervals until a means of escape presents itself, or the bird affords an opportunity of being caught by the neck, which, if tightly held and kept down, prevents much further mischief. Under such circumstances, however, Mr. Andrew has known a bird, with a badly-calculated kick, strike the back of its own head, scattering the brains—"a serious loss of valuable property to the farmer."

WE learn from the Tiflis paper *Caucasus* that during an excursion to the sources of the Jiagdon, which was made recently by several explorers, no fewer than eight glaciers were discovered, six of which are not marked on the 5 versts to the inch map of Caucasus. They have been viewed now and sketched from Styr-khokh Pass. The southern slope of the branch-ridge of the main chain, between the Kazbek and the Syrkhubarzon peak, has also been sketched from the Trussoff's Pass, and it appears that several of the glaciers of this part of the chain are not represented on the great map, while perpetual snow is shown where there is none. The glaciers visited by the party proved to have very much changed their aspect since 1882. Several sulphur and iron carbonate springs were visited in the Trussoff's valley, and several interesting Alpine flowers in bloom were collected on the passes.

A SKETCH of the vegetation of British Baluchistan, with descriptions of new species, published originally in the Linnean Society's Journal, has now been issued separately. The author is Mr. I. H. Lace, who has had the advantage of Mr. W. B. Hemsley's aid.

IN the *Bulletins de la Société d'Anthropologie de Paris* (fourth series, vol. ii. Parts 1 and 2) the subject most prominently dealt with is the slow rate at which the population of France increases. According to the report of a prolonged discussion on this question, there is much difference of opinion as to the causes to which the phenomenon must be attributed. The *Bulletins* also include interesting contributions on the Koubous, a native tribe of Sumatra, by M. Zelle; a series of spoons of various epochs, by M. Capitan; the pre-Columbian ethnography of Venezuela, by Dr. G. Marcano; justice in Ancient Egypt, by M. Ollivier-Beauregard; and religious evolution in the region of the Congo, by M. Clément Rubbens.

THE second part of the Catalogue of Mammalia in the Indian Museum, Calcutta, by Mr. W. L. Sclater, has just been issued. The first part was compiled by Dr. Anderson, the late Superintendent. The total number of species included in the Catalogue amounts to 590, of which 276 are found within the Indian Empire, and 314 are exotic.

THE Smithsonian Institution has issued a set of useful directions, by Leonhard Stejneger, for the use of collectors, who, without being herpetological experts, desire to procure for the U.S. National Museum specimens of the reptiles and batrachians which they may be able to gather in the neighbourhood of their residence or while travelling. The same Institution publishes directions for collecting recent and fossil plants, by F. H. Knowlton; and notes on the preparation ot rough skeletons, by F. A. Lucas.

STUDENTS will be glad to welcome the fourth edition of Prof. Milnes Marshall's well-known work on "The Frog: an Introduction to Anatomy, Histology, and Embryology." The author explains that the chapter on embryology has been in great part rewritten, and that some new figures have been added. The entire book has been carefully revised.

THE additions to the Zoological Society's Gardens during the past week include a Dorsal Hyrax (*Hyrax dorsalis*) from Sierra Leone, presented by Mr. Reginald Brett; a Common Polecat (*Mustela putorius*), British, presented by Mr. F. D. Lea Smith; a Ring-necked Parrakeet (*Palæornis torquatus*) from India, presented by Mrs. Bowen; an Australian Thicknee (*Œdicnemus grallarius*) from Australia, presented by Sir Ferdinand von Mueller, C.M.Z.S.; a Manx Shearwater (*Puffinus anglorum*), British, presented by Master Riviere.

OUR ASTRONOMICAL COLUMN.

SOLAR OBSERVATIONS.—In *Comptes rendus* for August 24. Prof. Tacchini gives a *résumé* of the solar observations made at the Observatory of the Roman College during the second quarter of this year. Spots and faculæ have been observed on 73 days, viz. 25 in April, 23 in May, and 25 in June, The following are the results obtained:—

1891.	Relative frequency		Relative magnitude		Number of groups per day.
	of spots.	of days without spots.	of spots.	of faculæ.	
April	9·24	0·00	24·56	55·60	2·36
May	14·35	0·00	48·14	51·82	4·09
June	16·88	0·00	47·00	89·38	3·80

The distribution and magnitude of the prominences observed are as follow:—

1891.	Number of days of observation.	Mean number.	Mean height.	Mean extension.
			″	°
April	18	7·50	42·3	1·5
May	21	4·62	37·3	1·4
June	19	5·53	39·4	1·8

It is worthy of remark that there was a secondary maximum in May in the case of spots, whilst a secondary minimum is indicated by the observations of prominences.

CONNECTION BETWEEN TERRESTRIAL MAGNETISM AND RADIANT SUNLIGHT.—Prof. Frank H. Bigelow contributes a note to the *American Journal of Science* for September, on the causes of the variations of the magnetic needle. He finds, from a discussion of magnetic observations made at thirteen stations during the month of June 1883, that "the permanent magnetic condition of the earth may be principally due to the orbital motion of the earth through the radiant field of sunlight. The rotation of the earth on its axis causes a modification of the direction of the axis of polarization, by diminishing the angle between the two axes, and as the result of the annual motion may cause it to rotate in a secular period about the axis of figure, or if the magnetization has already become set in the body of the earth, may cause a succession of secular waves to sweep over it from east to west, as is shown to be the case in the history of the isogonic lines and the long-period deflections of the needle." This interesting identification of the magnetic and light action of solar radiations is in harmony with the results of the investigations of Maxwell and Hertz. And Prof. Bigelow believes that, by the application of similar considerations to Mercury, he will be able to satisfactorily account for the outstanding motion of this planet's perihelion.

TWO NEW ASTEROIDS.—On August 28, Charlois discovered the 313th minor planet; and Palisa found the 314th two days later.

PHYSICS AT THE BRITISH ASSOCIATION.

THIS Section, as is unfortunately the custom, was housed in an ecclesiastical edifice in which no provision had been made for the exhibition of apparatus or lantern slides by the readers of papers. No doubt, it is impossible always to provide accommodation equal to that furnished two years ago at Newcastle, when the Physical Lecture Theatre of the Durham College of Science, with its appliances, was placed at the disposal of the Section. Still, it should be possible to provide lantern and screen, and provision should be made, when necessary, for partially darkening the room. If there were a guarantee that lantern slides could always be exhibited, many readers of papers would avail themselves of the opportunity to illustrate their communications much more adequately than is possible at present, when the only appliances are a piece of chalk and a diminutive blackboard; *e.g.* on Monday morning the beautiful photographs of Mr. Clayden and Dr. Copeland had to be passed round from hand to hand instead of being exhibited in a manner which would have done justice to their merits. The contents of many of the papers, too, would be much more easily and pleasantly grasped if such a course were adopted.

Unfortunately, some of the leading physicists, notably Sir William Thomson, Lord Rayleigh, and Prof. Fitzgerald, were unable to be present. Prof. Lodge, however, admirably filled the chair, and spared no exertion in the endeavour to clear up points of obscurity or difficulty that arose during the discussion.

In all, some fifty papers and reports were read. In the limited space at our disposal, we regret that it is only possible to refer to communications of general rather than of special scientific interest.

After the President's address on Thursday morning, Prof. Newton communicated a most interesting account of the action of Jupiter on small bodies passing near the planet, in which he showed that if a comet pass in front of Jupiter, owing to the gravitational attraction between the two bodies the kinetic energy of Jupiter will be increased, while that of the comet will be diminished, and may be diminished to such an extent as to cause it to form (though possibly only temporarily) a member of the solar system. On the other hand, if a comet, already a member of the solar system, pass behind Jupiter, the kinetic energy of the planet will be diminished and that of the comet will be increased, and may conceivably be increased under favourable circumstances to such an extent that the comet may no longer remain as a member of the system. Prof. Newton had calculated that of 1,000,000,000 comets from space crossing, in all directions, a sphere equal in diameter to that of Jupiter's orbit, about 1,200 would come near enough to Jupiter to have their period so much diminished as to be less than that of the planet.

Mr. W. E. Wilson read a paper on the absorption of heat in the solar atmosphere, and exhibited some of the apparatus he had used in the investigation. The method of observation employed consisted in allowing the sun's image to transit across the thermo-electric junction of a Boys radio-micrometer. He finds that the solar radiation from the extreme peripheral portion of the disk is distinctly less than that from the central portions. In this respect the sun's radiation differs entirely from that of the moon, in which there is little or no such difference in the illumination of different parts of the surface. This difference is attributable to the absorption of heat in the solar atmosphere, which will necessarily be much more marked for the peripheral than for the central portions of the disk.

Mr. G. H. Bryan presented an elaborate report on researches relative to the second law of thermodynamics, in which is described an exceedingly simple mechanical representation of Carnot's reversible cycle.

Friday was devoted to papers on electrical subjects. Prof. Andrew Gray read a paper on the electro-magnetic theory of the rotation of the plane of polarized light. Sir William Thomson's explanation of the phenomenon rests on the supposition that the ether has embedded in it a large number of small gyrostats. Prof. Gray showed that the ordinary Maxwellian equations for the phenomenon were obtainable on the supposition of the existence of a closed chain of small magnets embedded in the undisturbed medium, which set themselves with their axes in the direction of propagation of the ray as soon as the medium was magnetized in that direction.

This paper was followed by a most interesting communication from the President, in which he gave an account of preliminary experiments to ascertain if the ether is disturbed in the neighbourhood of a rapidly moving body—in other words, to ascertain whether the ether behaves as a viscous fluid. Allusion was first of all made to the experiments of Arago, in which he endeavoured to determine whether or not the ether was stagnant with respect to the earth by measuring the refractive index of a glass prism at different times of the day, when the ether stream (if it exist) will flow in one direction or the opposite through the prism. Arago found no such shift, indicating that the ether was stagnant with reference to the earth. Fresnel, Fizeau, and Michelson had also studied theoretically or experimentally the ratio of so-called "bound" ether to "free" ether. The problem which Prof. Lodge set himself to determine was whether a disk moving with great rapidity would or would not drag after it the ether in its immediate neighbourhood. Two parallel co-axial disks of steel were arranged to spin at an enormous rate. Rays of light from a single source were allowed to fall on a glass plate feebly silvered so that about half the light was transmitted and half reflected. By means of additional reflectors the two beams passed in opposite directions several times round in the space between the two disks, and were then observed in a common telescope and made to give interference bands. In this way, assuming viscosity of the ether, the one beam would have its velocity increased, the other would have its velocity retarded, with the result that a shift of the interference bands would be produced. So far, however, no such shift has been observed.

Prof. D. E. Jones gave an account of some experiments made by him at Bonn on electric waves in wires. Measurements of the electrical disturbance at different points of a wire, in which stationary waves are set up, were made quantitatively by putting a thermo-electric junction in the circuit at different points, and noting the deflection of the galvanometer in its circuit. Several curious results were recorded for which no explanations were forthcoming.

A communication was read from Lord Rayleigh, relating to the reflection of polarized light from liquid surfaces. He finds that the light reflected at the polarizing angle, from clean liquid surfaces, is only very slightly elliptically polarized ; if, however, the surface be ever so slightly contaminated, the amount of elliptically polarized light in the reflected beam is enormously increased.

Saturday was devoted principally to the consideration of papers on electrolysis. Mr. Shaw's report on the present state of our knowledge in electrolysis and electro-chemistry included a tabular compilation by Mr. Fitzpatrick of the electrical properties of soluble salts at different temperatures, and for different concentrations.

Mr. J. Brown read a paper on Clausius's theory of electrolytic conduction, and on some recent evidence for the dissociation theory of electrolysis, in which he gave an account of experiments with so-called semi-permeable membranes. The explanation of their filtering qualities simply depends on the membrane acting as a conductor.

Mr. Chattock gave an account of some important quantitative experiments which he had made on the discharge of electricity from points from which he finds that it is the air round the point rather than the metal surface itself which offers resistance to the discharge.

On Monday the meteorological and allied subjects were taken. The Reports of various Committees appointed to deal with meteorological subjects were read.

Dr. Johnstone Stoney read an interesting paper on the cause of double lines in the spectra of gases. He assumes that the molecules are vibrating in more or less complex harmonic curves, and he illustrated the simple case of sodium vapour by means of a pendulum oscillating to and fro, but with an apsidal motion. He stated that the application of astronomical methods of calculation to molecular motions of sodium vapour gives rise to a double D line instead of to a broadening of the line as might at first sight be imagined. In the discussion which followed, Mr. Webster stated that Prof. Michelson, who was endeavouring to determine the metre in terms of the wave-length of light emitted by a vibrating atom, had found by the interference method that all the mercury lines are double.

Dr. Copeland exhibited a model to explain the probable nature of the bright streaks on the moon. He attributes the appearance of the streaks to the existence of transparent spheres on the moon's surface, which reflect the light from the posterior surface so as to be only visible in the line of light.

During the morning the President interpolated some observations dealing with the effect of light in modifying the effect of the gravitational attraction of the sun on small particles. When sunlight falls upon a body, a very small repulsive effect is produced, amounting to about 67 dynes per square metre. Thus, for example, during an eclipse of the moon about 1000 tons are suddenly applied, but this small force is incapable of producing any observable effect on the motion of our satellite. The smaller the body, the larger, of course, the surface exposed relatively to the mass, and therefore the greater should be the effect produced. For a certain size of particle (about that of a grain of dust) the gravitational attraction and light repulsion should balance one another. The effect is clearly independent of distance.

On Tuesday, after the Report of the Committee on Electrical Standards, read by Prof. Carey-Foster, and an account of an elaborate research by Mr. Swinburne on the causes of variation of Clark cells, there was arranged a joint discussion with Section G, on "Units and their Nomenclature," which was opened by the President, who suggested that the discussion should, as far as possible, be confined to electrical units, and that the mechanical units should be left to a later period. He discussed at some length the relative advantages and disadvantages of the various names for the unit of self-induction, secohm, quadrant, henry, &c., and expressed himself as of opinion that the quadrant, which was really an angular measure, but which was frequently used as a linear measure, was very objectionable in that it indicated that the unit of self-induction was a length, when it was perfectly well known not to be a length. He was, therefore, of opinion that some name with a less obvious meaning, such as that of a person, was very desirable. He thought also that the secohm was too large for practical purposes, and that some sub-multiple such as $\frac{1}{1000}$ would be preferable.

The President was followed by Mr. Preece, who referred to the work of the British Association Committee on Electrical Standards, which had lasted now for thirty years, and expressed the opinion that it would be undesirable to interfere in any way with the old standards now about to be legalized by the Board of Trade.

Prof. Stroud read a paper on some revolutionary suggestions on the nomenclature of electrical and mechanical units, in which he advocated selecting 10^9 cm. as the unit of length, 10^{-9} gm. as the unit of mass, and 1 sec. as the unit of time to form the basis of a new practical system of units. He also explained the details of a system of automatic nomenclature for C.G.S. and other units, which he thought should be taken into consideration before any fresh names were authorized. The special feature of the system is that every label is self-explanatory.

Dr. Johnstone Stoney thought the old system should remain intact, and that the proper way to deal with the subject of nomenclature was to indicate sub-multiples by numerical prefixes ; *e.g.* he would call a microfarad a sixth farad, and the capacity of a Leyden jar would be about a tenth farad. He suggested that the name for the unit of magnetism should be a Gilbert, and that of the unit magnetic field a Gauss.

Prof. Carey Foster thought that if the volt and ampere were made ten times as great, fresh names, such *e.g.* as "gal," from Galvani, should be introduced.

Prof. Rücker laid stress on the importance of recognizing the fact that we possessed at present no definite knowledge as to the absolute dimensions of any electrical or magnetic unit, and therefore it was undesirable to introduce names (such *e.g.* as quadrant) implying the possession of such knowledge.

Prof. S. P. Thompson drew attention to the desirability of distinguishing between scalar and vector quantities in our dimensions.

Prof. Gray disapproved of the term electromotive force, but thought it was a term which could scarcely be eradicated now.

Each speaker, in fact, discussed the subject from his own point of view, with the result, as the President remarked, that the time allotted had only served to open the discussion, but he hoped that it would be continued in the technical journals during the year, so that some definite conclusions might be arrived at in 1892.

Wednesday morning was devoted to clearing off arrears.

Prof. S. P. Thompson read two optical papers, one on the measurement of lenses, and a second on a new polarizer. In this instrument the polarization is effected by reflection from black glass, but to avoid the angling of the beam a reflecting prism is used in addition. This arrangement has the disadvantage that the axis of the beam undergoes a translational shift, so that rotation of the polarizer is out of the question. To get over this difficulty two more reflectors are introduced, or two quarter-wave plates may be used, one of which converts the plane polarized light into circularly polarized light, while the other reconverts it into light plane polarized in any azimuth.

Dr. Webster then gave an account of some experiments on a new method for determining v. The method is similar in some respects to Ayrton and Perry's, and gave as a result in the preliminary experiments $2\cdot987 \times 10^{10}$.

Prof. Rücker then gave an account of some experiments made by Prof. Ayrton and himself, on the magnetic field near the South London Electrical Railway. The experiments were made in a house in Kennington Park Road with ordinary galvanometers, and showed conclusively that the magnetic disturbances on delicately suspended needles would be perceptible at considerable distances.

Prof. J. V. Jones, in describing some experiments on the periodic time of tuning-forks, maintained in vibration electrically, stated that dry platinum-platinum contacts do not work satisfactorily, whereas the results obtained with mercury contacts are much better, at all events when changes of temperature are carefully guarded against.

Mr. F. T. Trouton described some interesting experiments to determine the rate of propagation of magnetization in iron. A large coil of iron wire, from 8 to 12 feet in diameter, was supplied with one fixed coil wound on it, and through which the alternating current passed. A second exploring coil was connected up with a telephone, and one experiment consisted in endeavouring to find out the positions of nodes and internodes in the magnetized material from which it might have been possible to have determined the length of the wave of magnetization for a definite period of alternation. Nodes were observed in the half of the ring remote from the magnetizing coil, but these were easily ascertained not to be the ones sought for, because their position was not found to depend on the period of alternation.

The President attributed the effects to mechanical vibrations excited by magnetization.

CHEMISTRY AT THE BRITISH ASSOCIATION.

THE proceedings of Section B at Cardiff were not felt to be as interesting as on some previous occasions. Several well-known chemists were not present, and no set discussions on subjects of general chemical interest, which have been special features at other times, took place. Still, in the course of the meeting several papers of very considerable importance were read, and provoked valuable comments. The President's Address was listened to by an enthusiastic audience, and his remarks, together with several of the papers contributed during the meeting, should give a fresh impetus to the study of the metals.

Prof. Dunstan read the Report of the Committee on the Formation of Haloid Salts. It has been found by Mr. Shenstone that chlorine, prepared by the action of hydrogen chloride on manganese dioxide, attacks mercury readily, even when both substances are pure and dry, while that obtained by heating platinous chloride only attacks mercury extremely slowly. Incidentally it has been discovered that pure platinous chloride is a very difficult substance to prepare, an oxychloride being formed at the same time. The results so far obtained are to be regarded as preliminary.

Prof. Vivian B. Lewes read a paper on the spontaneous ignition of coal. His experiments lead him to reject the explanation of Berzelius, which attributes spontaneous ignition to the oxidation of pyrites contained in the coal. The heat given off by the combustion of the pyrites present in the most dangerous kind of coal, even if localized, would not be sufficient to raise the temperature of the adjacent coal to the ignition point. The cause of spontaneous ignition of coal is to be found, rather, in its power, especially when finely divided, of absorbing oxygen, which causes the slow combustion of some of the hydrocarbon constituents even at the ordinary temperature. The action may increase under favourable conditions until ignition of the coal results. The risk is greatest with large masses of coal, and with the ordinary air supply on board ships. The oxidation increases rapidly with the initial temperature of the coal, so that coal fires are found to occur most often on ships frequenting tropical climates. It may be roughly estimated that the absorbing power of a coal for oxygen is proportional to its power of taking up moisture.

In the discussion which followed, Prof. Bedson mentioned his experiments on the heating of coal-dust at various temperatures up to 140° C. He had noticed that in some cases combustible gases were given off by the coal.

A feature of special interest was the exhibition by Ludwig Mond of specimens of nickel-carbon-oxide and metallic nickel obtained therefrom. In the paper read in conjunction with this exhibit an account was given of the discovery and properties of the above compound. The physical properties have been described in the *Journal für physikalische Chemie.* Chemically, nickel carbonyl is most inactive, numerous experiments made to introduce the carbonyl group into organic substances by its means having been uniformly unsuccessful. Experiments were described having for their object the direct extraction of nickel from its ores by means of carbon monoxide. It was found that, as long as the nickel is combined with arsenic or sulphur, the process is entirely successful on a laboratory scale. Such ore, or matte, or speiss, is calcined, reduced by water gas at 450°, cooled down to a suitable temperature, and treated with carbon monoxide in a suitable apparatus. On exposing a heated surface to the gas containing nickel-carbon-oxide, it is possible to produce, direct from such gas, articles of solid nickel, or goods plated with nickel, resembling in every way those obtained by galvanic deposition of metals, and reproducing with the same exactitude and fineness any design upon such articles. This result can also be obtained by immersing heated articles in a solution of nickel-carbon-oxide in such solvents as benzole, petroleum, tar oils, &c., or by applying such solution to the heated articles with a brush or otherwise.

A specimen of iron-carbon-oxide was exhibited, which Messrs. Mond and Langer have obtained as an amber-coloured liquid, which, on standing, deposits tabular crystals of a darker colour, and solidifies entirely below − 21° C. to a mass of needle-shaped crystals. It boils at 102° C., but leaves a small quantity of green-coloured oil behind. Several analyses and vapour-density determinations have been made, but it is not yet certain whether a pure substance has been obtained or a mixture of several iron carbonyls. The authors hope shortly to publish a full account of this interesting substance, which differs considerably in its chemical behaviour from nickel-carbon-oxide.

Mr. Crookes described his experiments on the electrical evaporation of metals and alloys. If a brush of gold is placed in a vacuum tube and connected with the negative pole of an induction coil at ordinary temperature, and if a piece of glass be placed underneath the gold in the tube, on passing the current a metallic mirror appears on the glass, increasing in thickness to a leaf, which can be peeled off, and which is perfectly homogeneous. Films of silver and platinum can also be obtained. It is found that different metals thus treated evaporate at different rates, one or two, such as aluminium and magnesium, being apparently non-volatile. It is thus possible, in the case of the aluminium-gold alloy discovered by Prof. Roberts-Austen, to separate a large portion of the gold from the aluminium by electrical evaporation.

T. Turner gave an account of experiments which he had made to discover the cause of the red blotches which often appear on the surface of brass sheets on rolling, and which are a great source of annoyance to Birmingham manufacturers. They are due to the erosion of the zinc by the chlorides present in the solution in which the brass has been pickled, and in the water in which it is afterwards washed, care not being always taken to prevent such chlorides from drying on before rolling.

A. P. Laurie described the experiments he has made to determine the electromotive forces of various alloys with a view to establishing the existence of definite compounds among them. His earlier experiments will be found in the Journ. Chem. Soc., 1888, p. 104. His recent work leads him to conclude that a compound of gold and tin of the formula AuSn exists, a sudden rise of electromotive force being observed when the proportion of tin in the alloy exceeds that required by the above formula. Compounds do not appear to exist among the alloys of zinc, cadmium, lead, and tin.

Prof. Roberts-Austen exhibited and described his self-recording pyrometer. In this instrument, thermal junctions of platinum and platinum containing 10 per cent. of rhodium are connected with a galvanometer. The spot of light from the mirror of this is caused to fall on a slit before which a photographic plate passes at a given rate, by which means a curve is traced, corresponding to the variations in temperature of the heated thermal junction. The other junction is kept at a constant temperature by immersion in water. Temperatures up to the melting-point of platinum can be determined with an accuracy of 10°. The curves of cooling of several alloys have been determined. The alloy of gold and aluminium differs from others, such as that of platinum and lead, in that there is no break in the curve at the point of solidification of the alloy.

A paper by A. Vernon-Harcourt and F. W. Humphery was entitled "The Relation between the Composition of a Double-Salt and the Composition and Temperature of the Liquid in which it is formed." The authors have obtained a large number of double chlorides of ammonium and iron by crystallizing from solutions containing varying amounts of ferrous and ammonium chlorides, and maintained at different temperatures. The composition of the salts varied, according to conditions, from two to twenty-one molecules of ammonium chloride combined with one of ferrous chloride. The salts could be obtained well crystallized, and varied considerably from each other in their crystalline habit. The authors suggest that similar complex compounds may exist in other cases.

Prof. Dunstan, in the discussion which followed, described a series of double cyanides of zinc and mercury, of complex composition, which he had obtained by precipitation.

In a preliminary account of some experiments he is making on the action of oxide of cobalt in causing the evolution of oxygen from hypochlorites, Prof. M'Leod showed that, on boiling an alkaline solution of a hypochlorite alone, some oxygen is evolved and chlorate formed, so that the action is probably somewhat complex in presence of oxide of cobalt.

In the absence of Prof. Armstrong, Dr. Morley read the Report on the Isomeric Naphthalene Derivatives. The study of the dichloronaphthalenes has been completed. Of the twelve reported to exist, only ten could be obtained. This number is that required by theory. Of the fourteen theoretically possible trichloronaphthalenes, thirteen have been obtained. The compound containing the chlorine atoms in the positions 1 : 2 : 1′ is missing. These results put it beyond question that naphthalene has a symmetrical structure. Its exact inner configuration has yet to be dealt with. Experiments have been made with a view to determine the manner in which substitution takes place. It appears probable that an addition product is always first formed.

Prof. Rücker gave an account of the experiments made by Prof. Roberts-Austen and himself to determine the specific heat of basalt. The experiments were performed with the aid of the self-recording pyrometer above-mentioned. The results obtained when the substance was heated in a platinum crucible in a gas furnace agreed well together. The specific heat increases regularly up to the melting-point, which is not very definite. About this point there is considerable absorption of latent heat. The mean specific heat between 20° and 470° was found to be ·199; between 470° and 750°, ·244; between 750° and 880°, ·626; and between 880° and 1190°, ·323.

Prof. F. Clowes described an apparatus for testing safety-lamps which permitted economy in the marsh-gas used. It consisted essentially of a large wooden box, rendered gas-tight by paraffin, in which the mixture of fire-damp and air could be made, the safety-lamp being afterwards introduced. A lamp was exhibited which would indicate in this apparatus ·25 per cent. of fire-damp.

Prof. C. M. Thompson described the results he has obtained on repeating the experiments of Krüss and his colleagues on the rare earths, which caused them to announce the probable existence of about twenty new elements. Although he has worked on material from the same locality and of the same appearance as that used by the above-named workers, he has entirely failed to confirm their results, at any rate with regard to the didymium fraction. He considers that the absence of certain lines noticed by them in the didymium spectrum may be due simply to dilution, and do not indicate a splitting up of that element. On making his solutions sufficiently strong, he was able in all cases to obtain the lines.

Prof. Ramsay drew attention to the remarkable properties which are exhibited by the liquids obtained by passing excess of hydrogen sulphide into solutions of certain metals, and afterwards expelling the excess of hydrogen sulphide by hydrogen. Mercuric sulphide treated in this way dissolves to a dark-brown solution. Antimony and arsenic sulphides also dissolve. On examining the mercury solution under the microscope, brown particles are seen in a state of rapid motion. With antimony solution, particles are not visible, but a sort of granular movement is to be seen. With arsenic solution, nothing is visible. On dialysis of the solution, none of the metal diffuses if the solution is pure; in the case of the antimony, diffusion takes place if tartaric acid is present. These solutions are readily precipitated by the addition of certain salts, but, although the antimony solution becomes nearly solid on precipitation, no accompanying rise of temperature can be noticed. Also, no depression of the freezing-point is observed with such a solution. The specific gravity of the solution, however, is higher than that of water. The experiments show the power of the solvent to bring about extremely fine mechanical division of a substance, and suggest the possibility of further atomic or ionic separation. The particles of quasi-dissolved substance are believed to be in a state of rapid but circumscribed motion.

One of the few papers on organic chemistry was read by J. J. Sudborough, on the action of nitrosyl chloride on unsaturated carbon compounds. He has examined the action of nitrosyl chloride on ethylene, propylene, amylene, and cinnamene, crotonic, oleic, erucic, and cinnamic acids. Of these, ethylene is chlorinated, and forms the dichloride $C_2H_4Cl_2$; propylene is practically unacted upon; amylene forms a nitrosochloride, $C_5H_{10}NOCl$, melting at 152°; and cinnamene a similar compound, C_8H_8NOCl, melting at 97°. Crotonic acid is unacted upon, even when heated to 90°, while oleic and erucic acids readily form definite nitrosochlorides, the former melting at 86° and the latter at 92°. Cinnamic acid is unacted upon when cooled, but forms the dichloride $C_9H_8O_2Cl_2$ when heated to 100°. Up to the present the author can find no laws regulating the action of nitrosyl chloride on various carbon compounds.

A paper was read by C. G. Moor, on a new method for the disposal of sewage. This consists in the application of a method invented by Mr. Rees Reece for obtaining tar, ammonia, &c., from peat, to the recovery of similar products from sludge cake. A kind of lime-kiln is employed, with a forced draught, connected to a series of condensers. The operation is conducted in such a manner that the material in the lower part of the furnace is kept in active combustion; its heat distils the material directly above, and this in its turn gradually descends to serve as fuel for the succeeding charge. Eighty per cent. of the theoretical yield of ammonia has been obtained. In order for the process to be commercially successful, it seems that the use of lime in pressing the sludge should be avoided at all costs, as, if much lime is present, the ash obtained in the furnace has a very low value, and clinker is apt to be produced. The author suggests the use of carbonized sludge in powder, mixed with salts of alumina and iron, in place of lime.

A. H. Allen described a curious reaction he had noticed on treating glycerides with alcoholic potash. If the quantity of potash or soda present is insufficient to completely saponify the glyceride, an ethyl salt of the acid is obtained. Thus in the case of butyrin large quantities of ethyl butyrate pass over on distillation. In the case of acetin it was found that no action took place on boiling sodium acetate, acetin, and alcohol together; but, on the addition of a trace of potash, 80 per cent. of the theoretical yield of ethyl acetate was obtained.

SOME DIFFICULTIES IN THE LIFE OF AQUATIC INSECTS.[1]

WE understand insects to be animals of small size, furnished with a hard skin and six legs, breathing by branched air-tubes, and commonly provided in the adult condition with wings. The animals thus organized are pre-eminently a dominant group, as is shown by the vast number of the species and individuals, their universal distribution, and their various habitat.

The insect type, like some fruitful inventions of man—paper or lithography, for instance—has proved so successful that it has been found profitable to adapt it to countless distinct purposes. I propose to consider one only of its infinitely varied adaptations, viz. its adaptation to aquatic life.

There are insects which run upon the earth, insects which fly in the air, and insects which swim in the water. The same might be said of three other classes of animals—the three highest—viz. mammals, birds, and reptiles. But insects surpass all other classes of animals in the variety of their modes of existence. Owing to their small size and hard skin, they can burrow into the earth, into the wood of trees, or into the bodies of other animals. There are some insects which can live in the water, not as the mammal, bird, or reptile does, coming up from time to time to breathe, but constantly immersed, like a fish. This is the more remarkable because insects are, as a class, air-breathers. Air-tubes or tracheæ, branching tubes, whose walls are stiffened by spiral threads, supply all the tissues of the body with air. That such an animal should be hatched in water, and live almost the whole of its life immersed, a thing which actually happens to many insects, is a matter for surprise, and implies many modifications of structure, affecting all parts of the body.

The adaptation of insects to aquatic conditions seems to have been brought about at different times, and for a variety of distinct purposes. Many Dipterous larvæ burrow in the earth. Some of these frequent the damp earth in the neighbourhood of streams; others are found in earth so soaked with water that it might almost be called mud, though they breathe by occasionally taking in atmospheric air. In yet more specialized members of the same order we find that the larva inhabits the mud at the bottom of the stream, and depends for its respiration entirely upon oxygen dissolved in the water. The motive is usually that the larva may get access to the decaying vegetable matter found in slow streams, but some of these larvæ have carnivorous propensities.

Other insects merely dive into the water, coming up from time to time to breathe, or skate upon the surface.

Nearly every order of insects contains aquatic forms, and the total number of such forms is very large. I believe that all are modifications of terrestrial types, and it is probable that members of different families have often betaken themselves to the water independently of one another.

The difficulties which aquatic insects have to encounter begin with the egg. It is in most cases convenient that the egg should be laid in water, though this is not indispensable, and the winged, air-breathing fly is, as a rule, ill-fitted for entering water. Some insect-eggs hatch if they are merely scattered, like grains of sand, over the bottom of a stream, but others must be laid at the surface of the water, where they can gain a sufficient supply of oxygen. If the water is stagnant, it will suffice if the eggs are buoyant, like those which compose the egg-raft of the gnat, but this plan would hardly answer in running streams, which would carry light, floating eggs to great distances, or even sweep them out to sea. Moreover, floating eggs are exposed to the attacks of hungry creatures of various kinds, such as birds or predatory insect larvæ. These difficulties have been met in the cases of a number of insects by laying the eggs in chains or strings, and mooring them at the surface of the water. The eggs are invested by a gelatinous envelope, which swells out, the moment it reaches the water, into an abundant, transparent mucilage. This mucilage answers more than one purpose. In the first place it makes the eggs so slippery that birds or insects cannot grasp them. It also spaces the eggs, and enables each to get its fair share of air and sunlight. The gelatinous substance appears to possess some antiseptic property, which prevents water-moulds from attacking the eggs; for, long after the eggs have hatched out, the transparent envelope remains unchanged. The eggs of the frog, which are laid in the stagnant water of ditches or ponds, float free at the surface, and do not require to be moored. The eggs of many snails are laid in the form of an adhesive band, which holds firmly to the stem or leaf of an aquatic plant. Some insects, too, lay their eggs in the form of an adhesive band. In other cases the egg-chain is moored to the bank by a slender cord.

The common two-winged fly, Chironomus, lays its eggs in transparent cylindrical ropes, which float on the surface of the water. During the summer months these egg-ropes, which are nearly an inch in length, may readily be found on the edges of a stone fountain in a garden, or in a water-trough by the side of the road. The eggs are arranged upon the outside of the rope in loops, which bend to right and left alternately, forming sinuous lines upon the surface. Each egg-rope is moored to the bank by a thread, which passes through the middle of the rope in a series of loops, and then returns in as many reversed and overlapping loops, so as to give the appearance of a lock-stitch. The thread is so tough that it can be drawn out straight with a needle without breaking. If the egg-rope is dipped into boiling water, the threads become apparent, but in the natural state they are invisible, owing to their transparency. The mucilage is held together by the threads interwoven with the mucilage. The loops can be straightened without injury until the length of the rope is almost doubled. If stretched beyond this point the threads become strained, and do not recover their original shape when released. By means of these threads, firmly interwoven with the mucilage of the egg-rope, the whole mass of many hundreds of eggs is firmly moored, yet so moored that it floats without strain, and rises or falls with the stream. The eggs get all the sun and air which they require, and neither predatory insects, nor birds, nor water-moulds, nor rushing currents of water, can injure them.

The eggs of the caddis-fly are laid in larger ropes, which, in some species, are very beautiful objects, owing to the grass-green colour of the eggs. The egg-raft of the gnat, which has often been described, is well suited to flotation in stagnant water, and is freely exposed to the air, a point of unusual importance in the case of an insect which in all stages of growth seems to need the most efficient means of respiration, and whose eggs are usually laid in water of very doubtful purity. The lower or submerged end of each egg opens by a lid, and through this opening the larva at length escapes.

The eggs of water-haunting insects are in many ways particularly well suited for the study of development. The eggs of Chironomus, for instance, can always be procured during the summer months. They are so transparent as to admit of examination under high powers of the microscope as living objects, and as they require no sort of preparation, they may be replaced in the water after each examination to continue their development. This saves all trouble in determining the succession of the different stages—a point which usually presents difficulties to the embryologist. The whole development of the egg of Chironomus is completed in a few days (three to six, according to temperature), and it is therefore an easy matter to follow the process throughout with the help of three or four chains of eggs.

When the larvæ are hatched, and escape into the water, new difficulties arise. Some have to seek their food at the surface of the water, and must yet be always immersed, others live upon food which is only to be found in rapid streams, and these run serious risk of being swept away by the rush of water. All need at least a moderate supply of oxygen, which has either to be drawn from the air at the surface, or extracted from the water by special organs. The difficulty of breathing is, of course, greatly increased when the larva seeks its food at the bottom of foul streams, as is the case with certain Diptera. The larva of Chironomus, for example, feeds upon vegetable matter, often in a state of decay, which is obtained from the mud at the bottom of slow streams, and in this mud the larva makes burrows for itself, cementing together all sorts of materials by the secretion of its salivary glands, drawn out into fine silken threads. The burrows in which the larva lives furnish an important defence against fishes and other enemies, but they still further increase the difficulty of procuring a supply of air. Hence, the larva frequently quits its burrow, especially by night, and swims towards the surface. At these times it loops its body to and fro with a kind of lashing movement, and is thus enabled to advance and rise in the water. From the well-aërated water at the surface of the stream it procures a free supply of oxygen,

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[1] Evening Discourse, delivered before the British Association, Cardiff, 1891, by L. C. Miall, Professor of Biology in the Yorkshire College.

which becomes dissolved in the abundant blood of the larva. Four delicate tubes filled with blood, which are carried upon the last segment of the body, are believed to be especially intended for the taking up of dissolved oxygen. The tracheal system is rudimentary and completely closed, and hence gaseous air cannot be taken into the body. The dissolved oxygen, procured with much exertion and some risk, must be stored up within the body of the larva, and used with the greatest economy. It is apparently for this reason that the larva of Chironomus contains a blood-red pigment, which is identical with the hæmoglobin of vertebrate animals. The hæmoglobin acts in the Chironomus larva as it does in our own bodies, as an oxygen-carrier, readily taking up dissolved oxygen, and parting with it gradually to the tissues of the body.

It is instructive to notice that only such Chironomus larvæ as live at the bottom and burrow in the mud possess the red hæmoglobin. Those which live at or near the surface have colourless blood, and a more complete, though still closed, tracheal system. The larva of the carnivorous Tanypus, which is found in the same streams, but does not burrow, has a much more complete tracheal system, and only enough hæmoglobin to give a pale red tint to the body. The larva of the gnat again, which has a large and open tracheal system, and in all stages of growth inhales gaseous air, has no hæmoglobin at all. A list of the many animals of all kinds which contain hæmoglobin, shows that for some reason or another each of them requires to use oxygen economically. Either the skin is thick, and the respiratory surface limited, or they are inclosed in a shell, or they burrow in earth or mud. We might expect to find that hæmoglobin would always be developed in the blood of animals whose respiration is rendered difficult in any of these ways, but any such expectation would prove to be unfounded, and there are many animals whose mode of life renders it necessary that oxygen should be stored and economically used, which contain no hæmoglobin in their blood. Hence, while we have a tolerably satisfactory reason for the occurrence of hæmoglobin in a number of animals whose respiratory surface is limited, and whose surroundings make it a matter of difficulty to procure a sufficient supply of oxygen, we have to admit that many similar animals under the same conditions manage perfectly well without hæmoglobin. Such admission is not a logical refutation of the explanation. I might fairly put forward the baldness of mankind as at least the principal reason for wearing wigs, and this explanation would not be impaired by any number of cases of bald men who do not wear wigs. The fact is that the respiratory needs, even of closely allied animals, vary greatly, and further, there are more ways than one of acquiring and storing up oxygen in their bodies.

Either the storage-capacity for oxygen of the Chironomus larva is considerable, or it must be used very carefully, for the animal can subsist long without a fresh supply. I took a flask of distilled water, boiled it for three-quarters of an hour, closed it tight with an india-rubber bung, and left it to cool. Then six larvæ were introduced, the small space above the water being at the same time filled up with carbonic acid. The bung was replaced, and the larvæ were watched from day to day. Four of the larvæ survived for forty-eight hours, and one till the fifth day. Two of them changed to pupæ. Nevertheless, the water was from the first exhausted of oxygen, or nearly so.

The Chironomus larva is provided with implements suited to its mode of life. The head, which is extremely small and hard, carries a pair of stout jaws, besides a most complicated array of hooks, some fixed, some movable. The use of these minute appendages cannot always be assigned, but some of them are apparently employed to guide the silky threads which issue from the salivary glands. The first segment behind the head carries a pair of stumpy legs, which are set with many hooks. These are mainly used in progression, and help the larva to hitch itself to and fro in its burrow. A similar, but longer pair of hooked feet is found at the end of the body. This hinder pair serves to attach the animal to its burrow when it stretches forth in search of food.

Creeping aquatic larvæ, such as Ephydra, possess several pairs of legs in front of the last pair, but the burrowing species, such as caddis-worms, agree with Chironomus, not only in their mode of life, but also in the reduction of the abdominal legs to a single pair, which are conspicuously hooked.

The larval head in this, as in many other aquatic insects, is far smaller and simpler than that of the fly. The larval head is little more than an implement for biting and spinning, by no means such a seat of intelligence as it is in higher animals. In Chironomus it contains no brain; the eyes are mere specks of pigment, and the antennæ are insignificant. But the head of the fly incloses the brain, and bears elaborate organs of special sense—many-facetted eyes, and in the male beautiful plumed antennæ. This difference in size and complexity probably explains the fact that the head of the fly is not developed within the larval head, but in the thorax. It is only at the time of pupation that it becomes everted, and its appendages assume the position which they are ultimately intended to occupy.

At length the Chironomus wriggles out of the larval skin, and is transformed into a pupa. It no longer requires to feed, and the mouth is completely closed. It is equally unable to burrow, and usually lies on the surface of the mud. Two tufts of silvery respiratory filaments project from the fore-end of the body just behind the future head, and these wave to and fro in the water, as the animal alternately flexes and extends its body. At the tail-end are two flaps, fringed with stout bristles, which form a kind of fan. The pupa virtually consists of the body of the fly, inclosed within a transparent skin. The organs of the fly are already complete externally, and even in microscopic detail they very closely resemble those of the perfect animal. These parts are, however, as yet very imperfectly displayed. The wings and legs are folded up along the sides of the body, and are incapable of independent movement. For two or three days there is no outward change, except that the pupa, which originally had the blood-red colour of the larva, gradually assumes a darker tint. The tracheal system, which was quite rudimentary in the larva, but is now greatly enlarged, becomes filled with air, secreted from the water by the help of the respiratory tufts, and the pupa floats at the surface. At last the skin of the back splits, the fly extricates its limbs and other appendages, pauses for a moment upon the floating pupa-case, as if to dry its wings, and then flies away.

This fly is a common object on our window panes, and would be called a gnat by most people. It can be easily distinguished from a true gnat by its habit of raising the fore-legs from the ground when at rest. It is entirely harmless, and the mouth-parts can neither pierce nor suck. Like many other Diptera, the flies of Chironomus associate in swarms, which are believed in this case to consist entirely of males. The male fly has plumed antennæ with dilated basal joints. In the female fly the antennæ are smaller and simpler, as well as more widely separated.

In brisk and lively streams another Dipterous larva may often be found in great numbers. This is the larva of Simulium, known in the winged state as the sand-fly. The Simulium larva is much smaller than that of Chironomus, and its blood is not tinged with red. The head is provided with a pair of ciliary organs, fan-like in shape, consisting of many longish filaments, and borne upon a sort of stem. The fringed filaments are used to sweep the food into the mouth. The larva of Simulium subsists entirely upon microscopic plants and animals. Among these are great numbers of Diatoms, and the stomach is usually found half full of the flinty valves of these microscopic plants. The Simulium larva seeks its food in rapid currents of water, and a brisk flow of well-aërated water has apparently become a necessity to it. If the larvæ are taken out of a stream and placed in a vessel of clear water, they soon become sluggish, and in warm weather do not survive very long. It matters little, however, to the larvæ whether the water in which they live is pure or impure; and streams which are contaminated with sewage often contain them in great abundance. There are no externally visible organs of respiration, but the skin is supplied by an abundant network of fine tracheal branches, which, no doubt, take up oxygen from the well-aërated water in which the animal lives. From this network at the surface, branches pass to supply all the internal organs. The Simulium larva is found upon aquatic weeds, and the pair of hind-feet, which in Chironomus were shaped so as to enable the larva to hold on to its burrow, here become altered, so as to furnish a new means of attachment. The two feet are completely united into one. The two clusters of hooks found in the Chironomus larva form now a circular coronet, and the centre of the inclosed space becomes capable of being retracted by means of muscles which are inserted into it from within. The larva is thus enabled to adhere to the smooth surface of a leaf, holding on by its sucker, which is, no doubt, aided by the circle of sharp hooks. Efficient as this adhesive organ undoubtedly is, it must be liable to derangement by occasional accidents, as, for instance, if there should be a sudden rush of water of unusual violence, or if the larva should be obliged to quit its hold in order to avoid some dangerous enemy. In the case of such an accident it is not easy to see how it will ever recover its footing. Swept along in a rapid current, we might suppose that there would be but a slender probability of its ever finding itself favourably placed for the application of its sucker and hooks. But such emergencies have been carefully provided for. The salivary glands, or silk-organs, which the Chironomus larva uses in weaving the wall of its burrow, furnish to the Simulium larva long mooring-threads, by means of which it is anchored to the leaf upon which it lives. Even if the larva is dislodged, it is not swept far by the stream, and can haul itself in along the mooring-thread in the same way that a spider or a Geometer larva climbs up the thread by which, when alarmed, it descended to the ground.

When the time for pupation comes, special provision has to be made for the peculiar circumstances in which the whole of the aquatic life of the Simulium is passed. An inactive and exposed pupa, like that of Chironomus, may fare well enough on the soft muddy bottom of a slow stream, but such a pupa would be swept away in a moment by the currents in which Simulium is most at home. When the time of pupation draws near, the insect constructs for itself a kind of nest, not unlike in shape the nest of some swallows. This nest is glued fast to the surface of a water-weed. The salivary glands, which furnished the mooring-threads, supply the material of which the nest is composed. Sheltered within this smooth and tapering case, whose pointed tip is directed up-stream, while the open mouth is turned down-stream, the pupa rests securely during the time of its transformation.

When the pupa-case is first formed, it is completely closed and egg-shaped, but, when the insect has cast the larval skin, one end of the case is knocked off, and the pupa now thrusts the fore-part of its body into the current of water. The respiratory filaments, which project immediately behind the future head, just as in Chironomus, draw a sufficient supply of air from the continually changed water around. The rings of the abdomen are furnished with a number of projecting hooks, which are able to grasp such objects as fine threads. The interior of the cocoon is felted by a number of silken threads, and by means of these the pupa gets an additional grip of its case. If it is forcibly dislodged, a number of the silken threads are drawn out from the felted lining of the case. The fly emerges into the running water, and I do not know how it manages to do so without being entangled in the current of water, and swept down the stream. The pupa-skin splits open just as it does in Chironomus, but remains attached to the cocoon.

The larva of the gnat is perhaps more familiar to naturalists of all kinds than any other aquatic Dipterous insect. The interesting description, and, above all, the admirable engravings, of Swammerdam, now more than two hundred years old, are familiar to every student of Nature.

The larva, when at rest, floats at the surface of stagnant water. Its head, which is provided with vibratile organs suitable for sweeping minute particles into the mouth, is directed downwards, and, when examined by a lens in a good light, appears to be bordered below by a gleaming band. There are no thoracic limbs. The hind-limbs, which were long and hooked in the burrowing Chironomus larva, and reduced to a hook-bearing sucker in Simulium, now disappear altogether. A new and peculiar organ is developed from the eighth segment of the abdomen. This is a cylindrical respiratory siphon, traversed by two large air-tubes, which are continued along the entire length of the body, and supply every part with air. The larva ordinarily rests in such a position that the tip of the respiratory siphon is flush with the surface of the water, and, thus suspended, it feeds incessantly, breathing uninterruptedly at the same time. When disturbed, it leaves the surface by the sculling action of its broad tail. Once below the surface, it sinks slowly to the bottom by gravity alone, which shows that the body is denser than the water. We have, therefore, to explain how it is enabled to float at the surface when at rest. The larva does not willingly remain below for any length of time. It rises by a jerking movement, striking rapid blows with its tail, and advancing tail foremost. When it reaches the top, it hangs as before, head downwards, and resumes its feeding operations.

In order to explain how the larva hangs from the surface against gravity, I must trouble you with some account of the properties of the surface-film of water. You will readily believe that I have nothing new to communicate on this subject, and I venture to show you a few very simple experiments, merely because they are essential to the comprehension of what takes place in the gnat.[1]

In any vessel of pure water, the particles at the surface, though not differing in composition from those beneath, are nevertheless in a peculiar state. I will not travel so far from the region of natural history as to offer any theoretical explanation of this state, but will merely show you experimentally that there is a surface-film which resists the passage of a solid body from beneath. [Mensbrugghe's float shown.] You see (1) that the float is sufficiently buoyant to rise well out of the water; (2) that, when forcibly submerged, it rises with ease through the water as far as the surface-film; (3) that it is detained by the surface-film, and cannot penetrate it. The wire pulls at the surface-film and distorts it, but is unable to free itself. In the same way the surface-film resists the passage of a solid body which attempts to penetrate it from above. This will be readily seen if we throw a loop of aluminium wire upon the surface of water. [Experiment shown.] The loop of wire floats about like a stick of wood. Aluminium is, of course, much lighter than iron, but the floating of this little bar does not mean that it has a lower density than that of water. If the bar is once wetted, it sinks to the bottom and remains there. Even a needle may, with a little care, be made to float upon the surface of perfectly pure water. Still more readily can a piece of metallic gauze be made to float on water. [Experiment shown.] Air can pass through the meshes with perfect ease; water also can pass through the meshes with no visible obstruction. But the surface-film, bounding the air and water, is entirely unable to traverse even meshes of appreciable size. These simple experimental results will enable us to appreciate certain facts of structure, which would otherwise be hard to understand, and which have been wrongly explained by naturalists of the greatest eminence, to whom the physical discoveries of this century were unknown.

We may now try to answer three questions about the larva of the gnat, viz.:—

(1) How is it able to break the surface-film when it swims upwards?

(2) How is it able to remain at the surface without muscular effort, though denser than water?

(3) How is it able to leave the surface quickly and easily when alarmed?

The tip of the respiratory siphon is provided with three flaps, two large and similar to one another, the third smaller and differently shaped. These flaps can be opened or closed by attached muscles. When open, they form a minute basin, which, though not completely closed, does not allow the surface-film of water to enter. When closed, the air within the siphon is unable to escape. At the time when the larva rises to the surface, the pointed tips of the flaps first meet the surface-film, and adhere to it. The attached muscles then separate the flaps, and in a moment the basin is expanded and filled with air. The surface-film is now pulling at the edges of the basin, and the pull is more than sufficient to counterbalance the greater density of the body of the larva, which accordingly hangs from the surface without effort. When the larva is alarmed, and wishes to descend, the valves close, their tips are brought to a point, and the resisting pull of the surface-film is reduced to an unimportant amount. [Living larvæ shown by the lantern.]

Swammerdam found it necessary, in explaining the flotation of the larva of the gnat to suppose that the extremity of its siphon was supplied with an oily secretion which repelled the water. No oil-gland can be discovered here or elsewhere in the body of the larva, and indeed no oil-gland is necessary. The peculiar properties of the surface-film explain all the phenomena. The surface-film is unable to penetrate the fine spaces between the flaps for precisely the same reason that it is unable to pass through the meshes in a piece of gauze.

After three or four moults the larva is ready for pupation. By this time the organs of the future fly are almost completely formed, and the pupa assumes a strange shape, very unlike that of the larva.

At the head-end is a great rounded mass, which incloses the wings and legs of the fly, beside the compound eyes, the mouth-parts, and other organs of the head. At the tail-end is a pair of flaps, which form an efficient swimming-fan. The body of the pupa, like that of the larva, is abundantly supplied with air-tubes, and a communication with the outer air is still maintained, though in an entirely different way. The air-tubes no longer open towards the tail, as in the larva, but towards the head. Just behind the head of the future fly is a pair of trumpets, so placed that in a position of rest the margins of the trumpets come flush with the surface of the water. Floating in this position, the pupa remains still, so long as it is undisturbed, but if attacked by any of the predatory animals which abound in fresh waters, it is able to descend by the powerful swimming movements of its tail fin.

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[1] A number of other experiments, illustrating the properties of the surface-film of water, are described by Prof. Boys in his delightful book on "Soap-Bubbles."

Not that the descent is without its difficulties. The pupa is not like the larva, denser than water, but buoyant. There are two respiratory tubes in the pupa, whereas there is only one in the larva, and to these two tubes the surface-film clings with a tenacity of which only experiment can give an adequate idea. Will you allow me to give you a little more borrowed physics?

If we take a solid body, capable of being wetted by water, and place it in water, the surface-film will adhere to the solid. If the solid is less dense than the water, it will float with part of its surface out of the water. Under such circumstances the surface-film will be drawn upwards around the solid, and will therefore pull the solid downwards. But if the solid is denser than the water, the surface-film around the solid will be pulled downwards, and will pull the solid upwards. Suppose that a solid of the same density as water floats with part of its surface in contact with air, and that weights are gradually added to it. The result will be that the surface of the water around the upper edge of the solid will become more and more depressed. The sides of the depression will take a more vertical position, until at last the upward pull of the film becomes unable to withstand further increase of weight. If this point is passed, the solid will sink. Before this point is attained, we shall have the solid, though denser than water, kept at the surface by the pull of the surface-film.

This state of things may be illustrated by a model. [Float with glass tube attached to its upper surface.] You will readily see that the float has to be weighted appreciably in order to break the connection of the tube with the surface-film. Now the pupa of the gnat has a pair of tubes which are in like manner attached to the surface of the water. When it requires to descend, the pull of the surface-film would undoubtedly be considerable. Adding weight to the body is, of course, impossible, and a great exertion of muscular force would be wasteful of energy, even if it could be put forth. The gnat deals with its difficulty in a neater way than this, and saves its muscular power for other occasions. Let me show you a method of freeing the float from the surface, which was suggested by observation of what was seen in the pupa of the gnat. A thread wetted with water is drawn over the mouth of each tube. It cuts the connection with the surface, and the float, loaded so as to be denser than water, goes down at once. Meinert has described a pencil of hairs which appear to perform the same office for the pupa of the gnat. The hairs draw a film of water over the open mouth of each respiratory tube, and muscular contraction, used moderately and economically, does the rest. When the pupa again comes to the surface the tubes are overspread by a glistening film of water. This is partially withdrawn by a movement of the hairs, so that a chink appears by which air can be slowly renewed. When the insect is completely tranquil, the hairs appear to withdraw more completely, and the tube suddenly becomes free of all film. The act of opening or closing the film is so rapid—like the wink of an eye—that I cannot pretend to have observed more than the closed tube, the slightly open tube, and then the sudden change to a completely open condition. [Living pupæ shown by the lantern.]

Another Dipterous larva described and admirably figured by Swammerdam is the larva of Stratiomys, a larva which, as the structure of the fly shows, belongs to an altogether different group from Chironomus, Simulium, or the gnat. Though only remotely connected with the gnat in the systems of zoologists, the Stratiomys larva has learned the same lesson, and is equally well fitted to take advantage of the peculiar properties of the surface-film. The tail-end of the Stratiomys larva is provided with a beautiful coronet of branched filaments. When this coronet is extended, it forms a basin open to the air and impervious to water, by reason of the fineness of the meshes between the component filaments. Were the larva provided with a basin of the same proportions formed out of continuous membrane, it might float and breathe perfectly well, but the old difficulty would come back, viz. that of freeing itself neatly and quickly when some sudden emergency required the animal to leave the surface. As it is, the plumed filaments collapse and their points approach; the side-branches are folded in, and the basin is in a moment reduced to a pear-shaped body, filled with a globule of air, and reaching the surface of the water only by its pointed extremity. Down goes the Stratiomys larva at the first hint of danger, swimming through the water with swaying and looping movements, somewhat like those of Chironomus. When the danger is past, it ceases to struggle, and floats again to the surface. The pointed tip of its tail-fringe pierces the surface-film, the filaments separate once more, and the floating basin is restored.

The larva of Stratiomys is extremely elongate. The length of its body has evidently some relation to the mode of life of the larva, but none at all to that of the fly which is formed within it. The pupa is so much smaller than the larva as to occupy only the fore-part of the space within the larval skin.[1] The interval becomes filled with air, and during the pupal stage the animal floats at the surface within the empty larval skin.

Stratiomys, both in its larval and pupal states, floats at the surface of the water. The larva can descend into the water when attacked, but the pupa is too buoyant, and too much encumbered by its outer case, to execute any such manœuvre. Provision has accordingly to be made for the protection of the helpless pupa against its many enemies. It is probable that hungry insects and birds mistake the shapeless larval skin, floating passively at the surface, for a dead object. The considerable space between the outer envelope, or larval skin, and the body of the pupa may keep off others, for the first bite of a Dytiscus or dragon-fly larva would be disappointing. Still further security is gained by the texture of the larval skin itself. The cuticle consists of two layers. The inner is comparatively soft and laminated, while the outer layer is impregnated with calcareous salts, and extremely hard. The needful flexibility is obtained by the subdivision of the hard outer layer. Seen from the surface, it is broken up into a multitude of hexagonal fields, each of which forms the base of a conical projection, reaching far into the softer layer beneath. The conical shape of these calcareous nails allows a certain amount of bending of the cuticle, while the whole exposed surface is protected by an armour, in which even the pointed mandibles of a Dytiscus larva can find no effective chink.

The larva and pupa of the Dipterous fly, *Ptychoptera paludosa*, exhibit some interesting adaptations of the tracheal system to unusual conditions. The larva is found in muddy ditches, where it buries itself in the black ooze to a depth of an inch or two. Here, of course, it can procure no oxygen, either gaseous or dissolved. When it requires a fresh supply, it must reach the surface with part of its body, and to enable it to do so with the least possible exertion, the tail-end of the body is made telescopic, like that of another and still more familiar Dipterous larva, Eristalis. The last segments are drawn out very fine, and are capable of a very great amount of retraction or expansion. No visible opening for the admission of air has been discovered, nor do the hairs form a floating basin, as in the Stratiomys larva. The larva may be often seen lying just beneath the surface, which is broken by the tip of the tail. Whether air can be admitted here by some very minute orifice, or whether it is renewed by the exchange of gases through a thin membrane, I cannot as yet venture to say. In shallow water the larva may be occasionally found lying on or in the mud, and stretching out its long tail to the surface. In deeper water, it often floats at the surface.

Two tracheal trunks run along the whole length of the body, including the slender tail, where they are extremely convoluted and unbranched. Towards the middle of the body the tracheæ become greatly enlarged in the centre of each segment, the intervening portions, from which many branches are given off, being comparatively narrow. Each tube, therefore, resembles a row of bladders connected by small necks. A cross-section shows that the tubes are not cylindrical, but flattened, and that, while the lower surface is stiffened by the usual parallel thickenings, the upper surface is thrown into two deep, longitudinal furrows, so that it is readily inflated, becoming circular in section, and readily collapses again when the air is expelled. It seems likely that the buoyancy of the larva can thus be regulated, and a larger or smaller quantity of air taken in as desired.

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[1] So singular is the disproportion between the larva and the pupa that some naturalists have actually described the latter as a parasite (Westwood's "Mod. Classification of Insects," vol. ii. p. 532).

The pupa has a pair of respiratory tubes, which are carried, not on the tail, but on the thorax, close behind the head. One of these tubes is very long, the other very short. The long tube is twice as long as the body, and tapers very gradually to its free tip. Here we find a curious radiate structure, rather like the teeth of a moss-capsule, which seems adapted for opening and closing. There is, however, no orifice which the most careful scrutiny has succeeeded in discovering. A delicate membrane extends between the teeth, and prevents any passage inwards or outwards of air in mass. The tube incloses a large trachea, the continuation of one of the main tracheal trunks. This is stiffened by a spiral coil, but at intervals we find the coil deficient, while the wall of the tube swells out into a thin bladder. However the tube is turned, a number of these bladders come to the surface. As the pupa lies on the surface of the mud, the filament floats on the top of the water, and the air is renewed without effort through the thin-walled bladders.

Why should the position of the respiratory organs be changed from the tail-end in the larva to the head-end in the pupa? Chironomus, the gnat, Corethra, and many other aquatic insects exhibit the same phenomenon. Evidently there must be some reason why it is more convenient for the larva to take in air by the tail, and for the pupa to take in air by the head. Let us consider the case of the larva first. Where it floats from the surface, or pushes some part of its body to the surface, it is plain that the tail must come to the top and bear the respiratory outlet, for the head bears the mouth and mouth-organs, and must sweep to and fro in all directions, or even bury itself in the mud in quest of food. To divide the work of breathing and feeding between the opposite ends of the body is of obvious advantage, for the breathing can be done best at the top of the water, and the feeding at the bottom, or at least beneath the surface. Such considerations seem to have fixed the respiratory organs at the tail of the larva. Why, then, need this arrangement be reversed when the insect enters the pupal stage? There is now no feeding to be done, and it surely does not signify how the head is carried. Why should not the pupa continue to breathe like the larva, by its tail, instead of developing a new apparatus at the opposite end of its body, as if for change's sake? Well, it does not appear that, so far as the pupa itself is concerned, any good reason can be given why the larval arrangement should not continue. But a time comes when the fly has to escape from the pupa-case. The skin splits along the back of the thorax, and here the fly emerges, extricating its legs, wings, head, and abdomen from their close-fitting envelopes. The mouth-parts must be drawn backwards out of their larval sheaths, the legs upwards, and the abdomen forwards, so that there is only one possible place of escape, viz. by the back of the thorax, where all these lines of movement converge. If, then, the fly must escape by the back of the thorax, the back of the thorax must float uppermost during at least the latter part of the pupal stage. Otherwise the fly would emerge into the water instead of into the air. Granting that the back of the thorax must float uppermost in the pupal condition, it is clear that here the respiratory tubes must be set.

I need hardly speak of the many insects which run and skate on the surface of the water in consequence of the peculiar properties of the surface-film. They are able to do so, first, by reason of their small size; secondly, because of the great spread of their legs; and thirdly, on account of the fine hairs with which their legs are provided. The adhesion of the surface-film is measured by the length of the line of contact, and accordingly the multiplication of points of contact may indefinitely increase the support afforded by the surface of the water.

In the case of very small insects, it becomes possible, not only to run on the surface of the water, but even to leap upon it, as upon a table. This is particularly well seen in one of the smallest and simplest of all insects—the little black Podura, which abounds in sheets of still water. The minute and hairy body of the Podura is incapable of being wetted, and the insect frisks about on the silvery surface of a pond, just as a house-fly might do on the surface of quicksilver. This is all very well so long as the Podura is anxious only to amuse itself, or move from place to place, but it has to seek its food in the water, and, indeed, the attractiveness of a sheet of water to the Podura lies mainly in the decaying vegetation far below the surface. But if the insect is thus incapable of sinking below the surface, how does it ever get access to its submerged food? I have endeavoured to arrive at the explanation of this difficulty by observation of Poduras in captivity. If you place a number of Poduras in a beaker half full of water, they are wholly unable to sink. They run about and leap upon the surface, as if trying to escape from their prison, but sink they cannot. I have chased them about with a small rod until they became excited and much alarmed, but they were wholly unable to descend. Even when large quantities of alcohol were added to the water, the dead bodies of the Podura are seen floating at the top, almost as dry as before. It is only when they are placed upon the surface of strong alcohol that the dead bodies become wetted, and after a considerable time are seen to sink. How, then, does the Podura ever descend to the depths where its food is found?

I found it an easy matter to make a ladder, by which the Poduræ could leave the upper air. A few plants of duck-weed introduced into the beaker enabled them at pleasure to pull themselves forcibly through the surface-film, and climb down the long root hanging into the water like a rope. Once below the surface, the Podura, though buoyant, is enabled, by muscular exertion, to swim downwards to any depth.

Other aquatic insects, not quite so minute as the Podura, experience something of the same difficulty. A Gyrinus, or a small Hydrophilus, finds it no easy matter to quit the surface of the water, and is glad of a stem or root to descend by.

To leave our aquatic insects for a moment, we may notice the habit of creeping on the under-side of the surface-film, which is so often practised by leeches, snails, cyclas, &c. I find this is often described as creeping on the *air*, and some naturalists of the greatest eminence, speak of fresh-water snails as creeping "on the stratum of air in contact with the surface of the water."[1] The body of the animal is, nevertheless, wholly immersed during this exercise, as may be shown by a simple experiment. If Lycopodium powder is sprinkled over the water, the light particles are not displaced by the animal as it travels beneath. The possibility of creeping in this manner depends, not upon any "repulsion between the water and the dry surface of the body," to quote an explanation which is often given, but upon the tenacity of the surface-film, which serves as a kind of ceiling to the water-chamber below. The body of the leech is distinctly of higher specific gravity than the water, and falls quickly to the bottom, if the animal loses its hold of the surface-film. The pond-snails, however, actually float at the surface, and if disturbed, or made to retract their foot, they merely turn over in the water.

What is the result of all the expedients which have enabled air-breathing insects to overcome the difficulties of living in water? They have been successful, we might almost say too successful, in gaining access to a new and ample store of food. Aquatic plants, minute animals, and dead organic matter of all kinds abound in our fresh waters. Accordingly the species of aquatic insects have multiplied exceedingly, and the number of individuals in a species is sometimes surprisingly high. The supply of food thus opened out is not only ample, but in many cases very easy to appropriate. Accordingly the head of the larva degenerates, becomes small and of simple structure, and may be in extreme cases reduced to a mere shell, not inclosing the brain, and devoid of eyes, antennæ, and jaws. The organs of locomotion also commonly afford some indications of degeneration. Where the insect has to find a mate, and discover suitable sites for egg-laying, the fly at least must possess some degree of intelligence, keen sense-organs, and means of rapid locomotion. But some few aquatic insects, as well as some non-aquatic species which have found out an unlimited store of food, manage to produce offspring from unfertilized eggs, and to have these eggs laid by wingless pupæ or hatched within the bodies of wingless larvæ. The development of the winged fly, the whole business of mating, and even the development of the embryo within the egg, have thus, in particular insects, been abbreviated to the point of suppression. This is what I mean by saying that the pursuit of a new supply of food has in the case of certain aquatic insects proved even too successful. Abundant food, needing no exertion to discover or appropriate it, has led in a few instances to the almost complete atrophy of those higher organs and functions which alone make life interesting.

The degeneration of aquatic insects, however, very rarely reaches this extreme. In nearly all cases the pupa is succeeded by a fly, whose activity is in striking contrast to the sluggishness of the larva. They differ, to the eye at least, almost as much as air differs from water.

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[1] Semper's "Animal Life," Eng. trans., p. 205, and note 97.

Of the friends to whom I am indebted for help, I must specially name my fellow-worker, Mr. Arthur Hammond, who has communicated to me many results of his own observations, and has drawn most of the illustrations shown this evening. My colleague, Dr. Stroud, has very kindly arranged, and in some cases devised, the physical experiments which have been so helpful to us.

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The Clarendon Press promises "Geography of Africa South of the Zambesi," by W. Parr Greswell; "Mathematical Papers of the late Henry J. S. Smith, Savilian Professor of Geometry in the University of Oxford," with portrait and memoir, 2 vols.; "Plane Trigonometry, without Imaginaries," by R. C. J. Nixon; "A Treatise on Electricity and Magnetism," by J. Clerk Maxwell, new edition; "A Manual of Crystallography," by M. H. N. Story-Maskelyne; "Elementary Mechanics," by A. L. Selby; "Weismann's Lectures on Heredity," Vol. II., edited by E. B. Poulton, F.R.S.

During the coming winter Mr. Edward Arnold proposes to issue a series of popular papers on Animals, by Prof. C. Lloyd Morgan, the well-known author of "Animal Life and Intelligence"; "A Treatise on the Standard Course of Elementary Chemistry," by E. J. Cox, Head Master of the Technical School, Birmingham; and a series of scientific works, by Doctor Wormell (the series will embrace text-books of Mechanics, Sound, Light, Heat, Magnetism and Electricity).

Messrs. Longmans, Green, and Co. announce a new volume of "Fragments of Science: being Detached Essays, Addresses, and Reviews," by John Tyndall, F.R.S. "About Ceylon and Borneo: being an Account of Two Visits to Ceylon, One Visit to Borneo, and how I Came Home and was Rocked to Sleep on the Bosom of—well, 'The Suez Canal,'" by Walter J. Clutterbuck, author of "The Skipper in the Arctic Seas," and joint author of "Three in Norway," and "B.C. 1887," with illustrations; "Anthropological Religion," the Gifford Lectures delivered before the University of Glasgow in 1891, by F. Max Müller; "An Introduction to Human Physiology," being the substance of lectures delivered at the St. Mary's Hospital Medical School from 1885 to 1890, by Augustus D. Waller; "Elements of Materia Medica and Therapeutics," with numerous illustrations, by C. E. Armand Semple, M.R.C.P. Lond., Member of the Court of Examiners, and late Senior Examiner in Arts at Apothecaries' Hall, &c.; "Outlines of Theoretical Chemistry," by Lothar Meyer, Professor of Chemistry in the University of Tübingen, translated by Profs. P. Phillips Bedson and W. Carleton Williams (this book, of about 200 pages, gives a concise account of the theories of modern chemistry, which, it is expected, will not only be of use to advanced students, but will also enable those who take a general interest in science, but are unfamiliar with the details of chemical investigation, to gain a general idea of the development of theoretical chemistry); "The Dynamics of Rotation," by A. M. Worthington, Professor of Physics, and Head Master of the Dockyard School, Portsmouth; "The Principles of Chemistry," by D. Mendeléeff, Professor of Chemistry in the University of St. Petersburg, translated by George Kamensky, A.R.S.M. of the Imperial Mint, St. Petersburg, and edited by A. J. Greenaway, Sub-Editor of the Journal of the Chemical Society, 2 vols.; "A Manual of the Science of Religion," by Prof. Chantepie de la Saussaye, translated by Mrs. Colyer Fergusson (*née* Max Müller), revised by the author; "Solutions: being an English Translation (by M. M. Pattison Muir) of Book IV. Vol. I. of the Second Edition of Prof. Ostwald's 'Lehrbuch der allgemeinen Chemie.'"

Messrs. Smith, Elder, and Co. have in preparation "Vertebrate Embryology," by A. Milnes Marshall, F.R.S., Professor in the Victoria University, Beyer Professor of Zoology in Owens College, late Fellow of St. John's College, Cambridge; new, revised, and cheaper edition of Finlayson's "Clinical Manual"; new edition of Farquharson's "Guide to Therapeutics"; new edition of Part I. of MacCormac's "Surgical Operations."

Messrs. Sampson Low, Marston, and Co. announce: "Theory and Analysis of Ornament," applied to the work of elementary and technical schools, by François Louis Schauermann, for eight years Head Master of the Wood and Carving Department, Royal Polytechnic, Regent Street, with 263 illustrations; "Answers to the Questions on Elementary Chemistry," theoretical and practical (ordinary course), set at the examinations of the Science and Art Department, South Kensington, 1887–91, by John Mills, formerly of the Royal College of Science, London, author of "Alternative Elementary Chemistry," fully illustrated; "Chemistry for Students," consisting of a series of lessons based on the Syllabus of the Science and Art Department, and specially designed to facilitate the experimental teaching of elementary chemistry in schools and evening classes, by John Mills, author of "Alternative Elementary Chemistry," &c., numerous illustrations; "A Complete Treatise on the Electro-Deposition of Metals," comprising electro-plating and galvanoplastic operations, the deposition of metals by the contact and immersion processes, the colouring of metals, the methods of grinding and polishing, &c., translated from the German of Dr. George Langbein, with additions by William T. Brannt, editor of "The Techno-Chemical Receipt Book," &c., illustrated by 125 engravings; "Handwriting in Relation to Hygiene," being a paper read at the Seventh International Congress of Hygiene and Demography, London, 1891, by John Jackson, and the Report of the Commission of Specialists appointed by the Imperial and Royal Supreme Council of Health, Vienna, 1891.

The next volume of the Contemporary Science Series, published by Mr. Walter Scott, will be "The Man of Genius," by Prof. Lombroso; this volume, which will be issued on September 25, will be copiously illustrated.

Messrs. Blackie and Son have in the press a "Text-book of Agriculture," under the editorship of Prof. R. P. Wright, of the Glasgow and West of Scotland Technical College; they have also in preparation a series of "Guides to the Science Examinations" (the first number, which is nearly ready, is by Mr. Jerome Harrison, of Birmingham, and deals with the examinations in physiography); Pinkerton's "Mechanics," in their series of Science Text-books, is about to enter a second edition, and the opportunity is being taken to adapt it to the revised requirements of the 1891 Syllabus of the Science and Art Department.

Messrs. A. and C. Black have in preparation: "Manual of Chemistry," by Dr. Alexr. Scott, Durham; "Manual of Botany," by Dr. Scott, Bickley; "Dictionary of Birds," by Prof. Alfred Newton and Dr. Gadow.

Messrs. Whittaker and Co. announce the following books:—In Whittaker's Library of Popular Science—"Light," by Sir H. Trueman Wood, Secretary of the Society of Arts, 86 illustrations, containing chapters on the Nature of Light, Reflection, Refraction, Colour and the Spectrum, Lenses, Optical Instruments, &c.; "The Plant World: its Past, Present, and Future," by George Massee, with numerous illustrations. In Whittaker's Specialist's Series—Prof. Oliver Lodge's work upon "Lightning Conductors and Lightning Guards"; "The Alkali Maker's Hand-book," by Prof. Dr. George Lunge and Dr. F. Hurter, a new, revised, and enlarged edition; "Electric Light Cables and the Distribution of Electricity," by Stuart A. Russell; "The Artificial Production of Cold," by H. G. Harris; "The Dynamo," by C. C. Hawkins and J. Wallis; "The Drainage of Habitable Buildings," by W. Lee Beardmore, Member of the Council and Hon. Sec. of the Civil and Mechanical Engineers' Society; a fourth revised and enlarged edition of "The Working and Management of an English Railway," by G. Findlay, General Manager of the London and North-Western Railway; "The Working and Management of an Atlantic Liner; with a Retrospect of the Trade," by A. J. Maginnis, recently Assistant Superintendent of the White Star Line. In Whittaker's Library of Arts, Sciences, Manufactures, and Industries—"A First Book of Electricity and Magnetism," by W. Perren Maycock; "The Practical Telephone Hand-book and Guide to Telephonic Exchange," by J. Poole, Whitworth Scholar, 1875, late Chief Electrician to the Lancashire and Cheshire Telephone Exchange Co., with 227 illustrations; "The Optics of Photography and Photographic Lenses," by J. Traill Taylor, editor of the *British Journal of Photography*; "The Art and Craft of Cabinet-making," by D. Denning, with upwards of 200 illustrations.

Messrs. Cassell and Co. announce:—"Geometrical Drawing for Army Candidates," by H. T. Lilley, new and enlarged edition; "A First Book of Mechanics for Young Beginners," with numerous easy examples and answers, by the Rev. J. G. Easton, late Scholar of St. John's College, Cambridge, formerly Head Master of the Grammar School, Great Yarmouth; "Work," yearly volume, an illustrated magazine of practice and theory for all workmen, professional and amateur; "The Principles of Perspective as Applied to Model-Drawing and Sketching from Nature," with 32 plates and other illustrations, by George Trobridge, Head Master Government School of Art, Belfast, second edition, revised and enlarged.

SCIENTIFIC SERIALS.

American Journal of Science, September.—On the capture of comets by planets, especially their capture by Jupiter, by H. A. Newton. The full paper is not now given. The completed results will be noted in Our Astronomical Column as soon as they are published.—Pleistocene fluvial planes of Western Pennsylvania, by Frank Leverett. Some facts are stated which clash with certain conclusions drawn by Mr. P. Max Foshay in a paper entitled "Pre-Glacial Drainage and Recent Geological History of Western Pennsylvania," which appeared in the November number of the *Journal*. From these it appears that the obstacles to a northward discharge of the Shenango, Mahoning, and Beaver are, on the whole, greater than those in the way of a southward discharge. In the Monongahela, Lower Alleghany, and the Ohio valleys, the available evidence all indicates southward discharge along the present course of the Ohio from the inter-Glacial period to the present time.—A method for the determination of antimony and its condition of oxidation, by F. A. Gooch and H. W. Gruener.—A method for the estimation of chlorates, by F. A. Gooch and C. G. Smith.—Dampening of electrical oscillations on iron wires, by John Trowbridge. The experiments lead to the conclusions that (1) The magnetic permeability of iron wires exercises an important influence upon the decay of electrical oscillations of high frequency. This influence is so great that the oscillations may be reduced to a half-oscillation on a circuit of suitable self-induction and capacity for producing them. (2) It is probable that the time of oscillation on iron wires may be changed. Only a half-oscillation has been obtained on iron wires, so this law cannot be stated definitely. (3) Currents of high frequency, such as are produced in Leyden jar discharges, therefore magnetize the iron.—Genesis of iron ores by isomorphous and pseudomorphous replacement of limestone, &c., by James P. Kimball. The author adduces a considerable amount of evidence showing that such products of epigenesis as siderite and ferro-calcite are, as a rule, products of direct pseudomorphous replacement of isomorphous calcic carbonate, like limestone, calcite, calc-sinter, calcareous sediments, &c. And the general proposition is therefore advanced that deposits of concentrated iron ores occur far more extensively as pseudomorphous replacements than is usually supposed.—On the constitution of certain micas, vermiculites, and chlorites, by F. W. Clarke and E. A. Schneider. Chemical analyses of several specimens are given.—A further note on the age of the Orange Sands, by R. D. Salisbury. Some new facts are stated in support of the view that the Orange Sand series of sands and gravels are of the pre-Pleistocene age.—Note on the causes of the variations of the magnetic needle, by Prof. Frank H. Bigelow. (See Our Astronomical Column.)—Notice of new vertebrate fossils, by O. C. Marsh.

THE *American Meteorological Journal* for August contains the following articles:—Mountain meteorology, by A. L. Rotch. The author points out the advantages of mountain stations at which regular and continuous observations can be made as compared with fragmentary observations in balloons. The chief characteristic of the pressure at high altitudes in temperate and northern regions is a higher pressure in summer and a lower pressure in winter; thus the barometer varies inversely at high and low levels. With elevation above the sea, the absorption of aqueous vapour diminishes, or inversely, solar radiation increases. In the Himalayas a black bulb thermometer *in vacuo* has registered 25° above the boiling point of water, while the shade temperature was only 75°. In general, the annual range of temperature diminishes with height, so that at an elevation of about 39,000 feet, which is the height of the cirrus clouds, probably the temperature is constant throughout the year. The hygrometric conditions at high altitudes are subject to rapid changes, from complete saturation to extreme dryness, and are accompanied by analogous thermal changes. In all mountainous regions, where there is no prevailing wind there is a wind blowing into the valleys during the day, and out from the valleys during the night. On calm, clear, winter nights the air in the valleys is often colder than on the mountain slopes. The author considers that much of the progress made in recent years in meteorological science is due to the establishment of mountain-stations, and that in comparing the work done by various nations to advance mountain meteorology, France stands unrivalled. The German and Austrian stations are frequently badly placed, being located in inns below the summits. Among the best stations (in addition to the French) he mentions the Sonnblick, Hoch Obir, Säntis, Ben Nevis, and Mount Washington.—On the various kinds of gradients, by L. Teisserenc de Bort. This is a translation from the memoirs of the Meteorological Congress held at Paris in 1889, in connection with the International Exhibition. The air being put in motion by differences of pressure, there ought evidently to be a relation between the gradient and the wind velocity, but although the wind increases with the gradient, there is no exact ratio, nor a constant relation from day to day. The author reviews the subject in connection with changes produced by temperature and dynamic effects upon the rectilinear movements of the atmosphere, and the movements caused by the earth's rotation, and he draws attention to the "dragging" of the air by the friction of the superincumbent layers, the effect of which ought to be revealed by observation.—The climatic history of Lake Bonneville, by R. de C. Ward. This is an abstract of a monograph by J. R. Gilbert, published by the United States Geological Survey. The paper is chiefly geological, but has an important bearing upon the secular changes in climate. Lake Bonneville was the ancestor of the great Salt Lake of Utah, which has frequently altered its level, even in recent years. At the time of the glacial epoch its level was about 300 metres higher, and it occupied about ten times its present area. The cause of the drying up of a large part of the former area is found in the prevailing winds which, on their way from the Pacific and in their passage over the Sierra Nevada, have precipitated much of their moisture, and pass over this region as drying winds.—The other articles are: observations at a distance (by means of electricity), by T. P. Hall; ocean fog (the causes which produce it), by E. P. Garriott; and water-spouts (observed on a voyage), by Prof. C. Abbe.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 31.—M. Duchartre in the chair.—Comparative amatomy of plants, by M. A. Chatin. In presenting this recently published work to the Academy, the author summarizes the results of his researches on Phanerogamic plants contained in it and former volumes.—Studies relative to the comparison of the international metre with the prototype of the *Archives*, by M. Bosscha. It has been experimentally found that, after existence for a century, the metre of the *Archives* may still be used in the production of a unit of length, with all the precision requisite in the measures of a prototype, and that the international metre and national standards defined by the equations sanctioned by the General Conference of Weights and Measures represent a unit of length sensibly different from the *Archives* metre. They are shorter by about $2\cdot6\ \mu$.—On a property of involution common to a plane group having five right angles and a system of nine planes, by M. Paul Serret.—On the laws of hardening and permanent deformations, by M. G. Faurie.—Observation of Wolf's comet, by M. J. Léotard. The comet was observed on August 27 as a feeble nebulosity about 3′ in diameter.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Lessons in Art: Hume Nisbet (Chatto).—The Electro-magnet and Electro-magnetic Mechanism: S. P. Thompson (Spon).—Hand-book of Jamaica, 1891–92 (Stanford).—The South Italian Volcanoes, edited by Dr. Johnston-Lavis (Naples).—The Frog, 4th edition.—A. M. Marshall (Manchester, Cornish).—Publications of West Hendon House Observatory, Sunderland; No. 1, Structure of the Sidereal Universe: T. W. Backhouse (Sunderland, Hills).—Telegraphic Determinations of Longitudes on the West Coast of Africa: Pullen and Finlay (Admiralty).—Electricity in Mining: S. P. Thompson (Spon).—Prize Essay on the Distribution of the Moon's Heat and its Variation with the Phase: F. W. Very (The Hague, Nijhoff).—Return, British Museum (Eyre and Spottiswoode).—Ueber den Beweis des Prinzips von der Erhaltung der Energie: T. Gross (Berlin, Mayer and Müller).—Geological Magazine, September (K. Paul).—Zeitschrift für Wissenschaftliche Zoologie, 52 Band, 3 Heft (Leipzig, Engelmann).—Morphologisches Jahrbuch, 17 Band, 3 Heft (Leipzig, Engelmann).—Encyklopædie der Naturwissenschaften, Dritte Abthg., 10 Lief (Breslau, Trewendt).—Notes from the Leyden Museum, vol. xiii. No. 3 (Leyden, Brill).—Ergänzungsheft zum 68 Jahrest. der Schlesischen Gesellschaft für Vaterländische Cultur (Breslau, Aderholz).—Journal of the Chemical Society, September (Gurney and Jackson).—The Asclepiad, No. 31, vol. 8: Dr. B. W. Richardson (Longmans).

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THURSDAY, SEPTEMBER 17, 1891.

ANIMAL CHLOROPHYLL.

Die Organisation der Turbellaria Acœla. Von Dr. Ludwig von Graff. (Engelmann, 1891.)

EIGHT years ago Dr. von Graff published his great monograph of the Rhabdocœl Turbellarians. The improved methods of histological research have enabled him to add some essential facts since that date to our knowledge of one of the most curious groups of the Rhabdocœla—namely, those known as Acœla. In 1885 he passed his Easter holidays at the Franciscan convent on the Dalmatic island of Lesina, and on the sea-shore of the garden of the convent found *Convoluta Schultzii* and *cinerea* in abundance.

Prof. Delage in 1886 published his valuable researches on *Convoluta Roscoffensis*, the green species of Roscoff, in which he made use of a method of gold-impregnation for demonstrating the nervous system. Dr. von Graff visited Roscoff in the same year, and in 1889 studied the Acœla at the Naples Station by means of Delage's and other methods of gold-impregnation. The present volume deals with *Proporus venenosus*, O. Schm.; *Monoporus rubripunctatus*, O. Schm.; *Aphanostoma diversicolor*, Oerst.; and several species of *Convoluta;* it being shown amongst other facts that the Roscoff species studied by Geddes and Delage is distinct from the Mediterranean *C. Schultzii*, and that *C. cinerea*, Graff, must be placed in a new genus, *Amphichœrus*.

The work is illustrated by ten quarto plates, coloured. A variety of important anatomical and histological details are given, and a systematic discussion of genera and species. Dr. von Graff discusses the relationship of *Trichoplax* to the Acœla, having received living specimens of this curious form from the aquarium of the Zoological Institute of Vienna, but he does not allude to the *Haplodiscus piger* of Weldon (*Quarterly Journal of Microscopical Science*, vol. xxix.), a floating form, taken off the Bahamas, which seems to be certainly a member of the group.

The chief matter of interest in Dr. von Graff's volume, which we propose to notice at greater length, is the chapter by Dr. G. Haberlandt, on "the structure and significance of the chlorophyll-cells of *Convoluta Roscoffensis*." Dr. Haberlandt states that the description by Geddes of the chlorophyll of this form, as diffused in the general plasma-body of certain cells, is erroneous. The green-coloured cells lie well below the cuticle, embedded amongst the cells of the superficial parenchyma. According to Haberlandt they are highly compressible and elastic, and devoid of anything like a cellulose envelope or even a membranous envelope. They are not uniformly green, but there is as a rule a single large chloroplast which forms a more or less complete shell to the protoplasm of the cell-body. In some of the cells Haberlandt could detect several peripheral plate-like chloroplasts. The crust-like chloroplast contains as a rule a single centrally placed pyrenoid of spherical form. As an exception two or even three pyrenoids are present. The pyrenoid is colourless; it is stained by hæmatoxylin or by borax carmine, but by no means so strongly as is the nucleus of the cell in which the chloroplast occurs. Starch granules in the form of small curved rods are grouped around the pyrenoid (sometimes within it), and are detected by a violet-brown reaction on addition of iodine solution. The colourless protoplasm of the cell is small in amount as compared with the enveloping chloroplast: its nucleus is only rendered visible by staining. The colourless protoplasm sometimes contains a group of granules of doubtful nature, erroneously taken by Geddes for starch granules.

The resemblance of these cells, especially in respect of the structure of their chloroplasts and pyrenoids, to certain cells which constitute the unicellular bodies of Volvocineæ, Tetrasporeæ, and Pleurococcaceæ, is insisted upon by Haberlandt. He raises the question as to whether they are to be regarded as parasitic Algæ in the sense of the theory of Entz and Brandt; and suggests another hypothesis—namely, that, whilst phylogenetically they must be regarded as Algæ (that is to say, have descended from Algæ), yet at the present time they have by profound adaptation to life in and with the *Convoluta*, altogether lost their character as independent algal organisms, and have become an integral histological element of the worm, and in fact constitute its assimilation tissue.

To test this hypothesis he asks: (1) How do the green cells get into the body of the worm? and (2) What becomes of them when the worm dies? Can they live in an isolated condition? To the first question he is unable to give an answer, but suggests that they *may* be handed on from generation to generation of the Convoluta, entering the egg-cell as a colourless minute cell which later develops its chloroplast just as the "leucoplasts" of higher plants are found in the egg-cell, and later become chloroplasts. As to the second question, Haberlandt has no doubt. The green cells die when they are removed from the worm's body or when the worm dies. He notes in this connection their membraneless character, and regards the loss of a cellulose envelope as one of the modifications which the ancestral parasitic Alga has undergone, rendering it incapable of living an independent life away from the tissues of its host. Lastly, Haberlandt justly remarks that *similarity* to an Alga is no proof that the green cells are really Algæ in nature. Haberlandt is inclined to place his theory as to the green cells of Convoluta alongside the suggestion of Schimper as to the origin of the chlorophyll corpuscles of higher plants—namely, that these are due to the union in the remote past of a green-coloured with a colourless organism. In this case and in that of Convoluta the highest phase of symbiotic association is attained, for the green organism can no longer be separated and cultivated apart, as in the case of the Lichens, but has, in fact, become an *organ* of the colourless organism, multiplying with it and forming an integral as well as a necessary part of its mechanism, and so greatly modified by ages of association as to be now barely recognizable as derived from an independent source. We can well suppose it possible that the green cells of Convoluta might proceed further in their modification, so as to lose the colourless protoplasm and the cell-nucleus; they would then become simple chlorophyll corpuscles like those of higher green plants.

The suggestion thus put forward by Haberlandt is in complete accord with the view which I have several times expressed in regard to the chlorophyll corpuscles of *Hydra viridis* and of *Spongilla viridis* (see *Quart. Journ. Micr. Sci.*, vol. xxii. p. 229), viz. that there is no more reason for regarding them as symbiotic Algæ than there is for so regarding the chlorophyll-corpuscles of a buttercup. Whether there is sufficient reason for so regarding the chlorophyll-corpuscles of a buttercup is another question, and one which certainly is not yet decided in the affirmative, though there are considerations which render such a hypothesis one not lightly to be dismissed.

A difficulty in the matter seems to be this—viz. that if the chloroplasts of the cells of multicellular organisms are to be regarded as parasitic, why should not those of unicellular Algæ also be regarded as parasitic? and if "Zoochlorella," or whatever the hypothetic Alga may be called in the case of Convoluta, can form chloroplasts, why should not the tissue-cells of Convoluta themselves, or of Hydra, or of Spongilla form chloroplasts?

It is obviously necessary to distinguish for the present (though *possibly*, as Haberlandt suggests, the one may be derived from the other) the strongly-marked unicellular parasites of Radiolaria and Anthozoa (the "yellow cells") from the green cells of Convoluta, and the chloroplasts of *Hydra viridis*, of *Spongilla fluviatilis*, and of many Ciliata. The statement which is current as to the existence of a nucleus in the chloroplasts of Hydra is simply erroneous, and that as to the independent multiplication of the chloroplasts of Ciliate Infusoria when removed from the cell in which they occur is possibly a misinterpretation of a graft-phenomenon. It is to be hoped that Dr. Haberlandt will spare the time to study for himself—as he has the green cells of Convoluta—the more readily obtainable chloroplasts of Hydra, Spongilla, and Stentor.

Some extremely interesting and suggestive remarks on the physiological and biological phenomena connected with the green cells of Convoluta conclude Dr. Haberlandt's chapter.

E. Ray Lankester.

STREATFEILD'S PRACTICAL ORGANIC CHEMISTRY.

Practical Work in Organic Chemistry. By Fredk. Wm. Streatfeild, F.I.C., &c., Demonstrator of Chemistry at the City and Guilds of London Institute's Technical College, Finsbury. With a Prefatory Notice by Prof. R. Meldola, F.R.S. "Finsbury Technical Manuals." (London: E. and F. N. Spon, 1891.)

THE numerous manuals of practical organic chemistry which have been published of late years testify to a re-awakened interest in an important subject. Some of these deal with the preparation of various typical organic compounds; others restrict themselves to describing methods of analysis. The present work combines both methods of teaching, and, as a special feature, divides the subject into "programmes of instruction" designed to meet the varied wants of the students attending the evening classes of chemical technology at the Finsbury College, taking into account the special nature of their daily avocations and the purpose to which they wish to apply their chemistry. Thus, after working through the introductory courses of "operations" and "analysis," and thus familiarizing himself with the general methods of the subject, the student would begin to specialize. The brewer would select the programme "ethyl alcohol and its reactions," which includes fermentation and the purification and estimation of alcohol, and touches on allied subjects, such as the preparation of aldehyde, acetic acid, and chloroform. The soap-maker would devote himself to the programme, "a study of the preparation and decomposition of ethyl acetate, and of the composition and reactions of some of the natural fats and oils"; thus passing from the simplest case of saponification (hydrolysis) of an ethereal salt in ethyl acetate to the more complex cases in the fats. This latter programme also includes the isolation and estimation of glycerol, and its properties; palmitic, stearic, oleic, and elaïdic acids; drying and non-drying oils; bromine and iodine absorption of oils; and other matters of interest in this connection. The tar-distiller would carry out the experiments given under "coal-tar and coal-tar products"—a very full and satisfactory chapter.

This restriction of the field of study is amply justified by the necessities of the case, and only an irreclaimable scientific purist would object to it. Even the ordinary day-student of chemistry, who can devote his whole time and energies to the subject, must work under some similar limitation when he comes to deal with the inexhaustible material of organic chemistry.

The experiments given under the various programmes are well selected, and the accompanying descriptions are evidently the outcome of a thorough practical knowledge of the subject. We may make an exception, however, in the case of the preparation of anhydrous formic acid (p. 66) by passing sulphuretted hydrogen over dry copper formate. The method is quite obsolete: Lorin's improved process of preparing the pure acid from anhydrous glycerol and anhydrous oxalic acid, drying the 95–98 per cent. acid thus obtained with boric anhydride, is now employed. Worst of all, the author recommends in this experiment that the sulphuretted hydrogen should be dried by passing it through concentrated sulphuric acid —a blunder which would go far to justify the prevailing impression that organic chemists are not always sufficiently conversant with the facts of inorganic chemistry.

In spite of this and one or two other trifling inaccuracies, we cordially recommend the book as a valuable aid to both teacher and student. What it deals with really is practical organic chemistry, and not the spurious substitute which, in the shape of "the detection of not more than one organic acid and one organic base," usurps the name in this country—thanks to the authority of examining boards, the industry of the writers of cram-books, and the credulity of the public.

Prof. Meldola, in his prefatory notice, referring to the evening classes in chemistry at the Finsbury College, says that they "cater for no examination"; and it is perhaps owing to this important circumstance that Mr. Streatfeild, on whom a considerable share of the laboratory teaching of these classes devolves, has been in a position to write a real manual of practical organic chemistry, and not a mere cram-book of tests—written up to syllabus.

TELESCOPIC WORK.

Telescopic Work for Starlight Evenings. By W. F. Denning, F.R.A.S. (London: Taylor and Francis, 1891.)

As might be expected from such an experienced and enthusiastic observer as Mr. Denning, this book is thoroughly practical. He is not contented with describing the beauties of the skies, but gives invaluable information as to how to see them best. The opening chapters give a very complete history of the invention and development of the powers of the telescope and its accessories. These are followed by chapters on the sun, moon, planets, stars, nebulæ, and clusters; the sun being introduced for the sake of completeness, although not comprehended in the title. The question of the relative advantages of large and small telescopes is discussed at considerable length, and one almost gets the impression that large telescopes, except under very favourable conditions, are not desirable possessions. It is very gratifying to note the encouragement given to observers of limited means. To them the book will be of the greatest assistance, both in the selection and use of their instruments.

The author's style is such as to make the book very entertaining as well as instructive. Some of his remarks are well worth quoting, as, for example, his opinion of controversy in scientific matters.

> "Competition and rivalry in good spirit increase enthusiasm, but there is little occasion for the bitterness and spleen sometimes exhibited in scientific journals. There are some men whose reputations do not rest upon good or original work performed by themselves, but rather upon the alacrity with which they discover grievances, and upon the care they will bestow in exposing trifling errors in the writings of their not-infallible contemporaries. Such critics would earn a more honourable title to regard were they to devote their time to some better method of serving the cause of science" (p. 56).

Mr. Denning is very emphatic in his opinion that an observer's time is too valuable to be spent in acting the showman to his friends and acquaintances. If all observers were so disposed, there might be reasonable hope for the establishment in this country of some such institution as the Gesellschaft Urania in Berlin, for the special gratification of persons desiring passing glimpses of celestial wonders.

It is scarcely necessary to say that the chapter on meteoric observations is as good as can be. More observers are undoubtedly needed in this branch of astronomy, and volunteers will find very full instructions in the pages of this book. In addition to the notes on variable stars given by the author, we would suggest the tracing of the light-curves of a small number of stars by each observer. Anyone at present attempting to determine the laws governing variability will find such information lamentably deficient.

The book is full of important practical details, and an appendix gives the chief new facts up to March 5, 1891.

The book does not attempt to deal with spectroscopic matters, but occasional references are made, and it is here, if anywhere, that fault may be found. Thus, referring to the nebula of Orion, it is stated (p. 334) that

> "the spectroscopic researches of Huggins have shown this nebula to be composed of incandescent gases, so that the stars telescopically observed in it are probably in the foreground and entirely disconnected from the nebulous mass."

In 1888, however, it was shown by the spectroscope that the stars of the trapezium, at all events, are simply condensations of the matter composing the nebula.

Everyone who uses a telescope, or who intends to use one, of whatever dimensions, should read Mr. Denning's book.

OUR BOOK SHELF.

Abbildungen zur Deutschen Flora H. Karsten's, nebst den ausländischen medicinischen Pflanzen und Ergänzungen für das Studium der Morphologie und Systemkunde. With Woodcuts of 709 Species. (Berlin: Friedländer und Sohn, 1891.)

This is a wonderfully cheap book, for the price of it is only three marks, and it contains figures with dissections of upwards of 700 plants, illustrating all the natural orders both of Cryptogamic and Phanerogamic plants which make up the European flora or are used medicinally. The text is confined to the preliminary table of the orders and families, an explanation of the details, and a final index.

The Thallophytæ are divided into 17 families, classed under 3 orders, Lichenes being maintained as on a par with Algæ and Fungi. In Cormophytæ there are 16 families under 6 orders, the orders of Sporiferæ being Filices, Selagines, Rhizocarpeæ, and Calamariæ. In Northocarpæ (Gymnosperms) there are 7 families under 5 orders, Balanophoraceæ and Lorantheæ being placed here. Under Teleocarpæ (Angiosperms) there are 159 families classed under 48 orders, Dicotyledons being divided into Monochlamydeæ and Dichlamydeæ, and the latter into Petalantheæ (Polypetalæ) and Corollanthæ (Gamopetalæ). The "families" correspond substantially to Bentham and Hooker's orders. To have such a good and cheap book in English (the text in the original, of course, is German) would be a great boon to students.

Elementary Text-book of Botany for the Use of Schools. By Edith Aitkin. 248 pp. (London: Longmans, Green, and Co., 1891.)

This volume has been written to serve as an adjunct to the teaching of Botany in girls' schools, and is the outcome of the author's own experience as a teacher. Miss Aitkin arranges the subject-matter in three parts. In the first are given the general characters of a number of selected types of Flowering plants treated in a manner suitable for young girls beginning the study. In the second part the details of Cryptogamic plants are given, commencing with *Protococcus* and Yeast, and so on, up to the Fern. In the third part we return to Flowering plants again from a more comprehensive point of view. This last section concludes with a number of chapters on the leading physiological processes of plants. We think the book will be found of service by those for whom it is intended, especially from the fact that Part I. is written, generally speaking, on the lines of the Lower Schedule laid down by the Oxford and Cambridge Schools Examination Board. The only criticism we have to make on this section is that perhaps the style is a little wanting in vitality and interest. Part II. is treated along sufficiently familiar lines, but in Part III., by the introduction of physiological work, with careful instructions as to simple experiments which can easily be performed to illustrate class teaching, we think that the author will have opened up fresh fields of interest in botanical study. The volume is profusely illustrated, many of the figures being new.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

A New Mammal from Sumatra.

A FEW years ago a new and interesting mammal, which is exceedingly rare even in its native haunts, was brought to the then President of Palembang—Mr. A. Pruys van der Hoeven. This gentleman, who is not only an eager sportsman, but also well versed in natural history, recognized it to be new to science, and to be more closely allied to certain representatives of the Edentata than to any other order of mammals.

The type-specimen was preserved in captivity for several weeks, was fed on ants, and afterwards on cooked rice, and was sent alive to Europe in order to be examined, described, and ultimately preserved in the Royal Museum at Leyden. It unfortunately died on board the vessel on its way to Holland, and, by an unaccountable blunder on the part of one of those in charge, its remains were not preserved, but thrown overboard.

During my own stay in Sumatra, from February till May 1891, I took particular trouble to obtain further information concerning this animal, and have found the fact of its existence—though, at the same time, of its exceeding rarity—confirmed in a way which does not allow me to doubt that, ere long, further specimens will be available for a thorough examination, also with respect to anatomical detail. My own attempts to secure a second specimen have not as yet been successful, but as they have drawn the attention of many persons to this animal, I feel bound, in deference to the claims to priority of its original discoverer, who has put his preliminary description as well as sketches of the animal at my disposal, to introduce this peculiar mammal into science, notwithstanding the fact that the type-specimen has been lost. The generic name has been selected, not with a view of indicating any closer anatomical relations with the genus *Manis*, but only to indicate that a hairy anteater is meant.

Trichomanis Hoevenii, gen. et sp. nov.—"Animal of the size of a very large cat. Fur grey, with a black longitudinal band along the middle of the back. Snout elongated and more or less conical, with a small mouth at the extremity. A long cylindrical tongue, which is thrust out, serves the animal in the collection of ants, which are its natural food. A more or less bushy tail. Ears not conspicuous. Legs higher than those of *Manis*, strong claws to the feet."

I have no doubt that this description—however superficial—is more than sufficient for the recognition of the animal as soon as it will have been reobtained. The type-specimen was caught in the mountainous districts that separate the Residencies of Palembang and Bencoolen in Sumatra.

A. A. W. HUBRECHT.

Utrecht, September 7.

An Oviparous Species of Peripatus.

Peripatus leuckartii has proved to be by no means uncommon in Victoria, being now recorded from a good many distinct localities, and forming a very characteristic constituent of our cryptozoic fauna. Hitherto, however, little has been known of its habits, and nothing of its mode of reproduction. The only observer, so far as I am aware, who has recorded anything concerning its life-history, is Mr. Fletcher, who has described (Proc. Linn. Soc. N.S.W., October 31, 1888) four very young individuals, the progeny of a female kept by him in captivity in damp moss and leaves for four months (July to October inclusive). Mr. Fletcher did not observe the birth of the young, but found them in company with the mother when apparently only a few days old. He assumes, naturally enough, that they were born alive, as in all other species of *Peripatus* whose life-history is known; this viviparous habit being, indeed, one of the most remarkable characters of the genus.

In May last I secured a few good specimens of *Peripatus leuckartii*, which I have since kept alive in a small vivarium specially arranged for the purpose. The vivarium consists of a large glass jar, with a flat glass cover supported above the rim of the jar on two thin slips of glass, so as to admit of free ventilation. I keep a small open jar full of water inside the large one, and the floor of the vivarium is covered with a thick layer of very rotten wood, kept moist by the evaporation of the water.

Under these conditions *Peripatus* flourishes well, and the specimens may be inspected when desired, by turning over the bits of rotten wood. On making such an inspection to-day, I found that some twelve or fifteen eggs had been deposited beneath some of the pieces of rotten wood, and in crevices of the same. Careful examination showed that these eggs were undoubtedly those of *Peripatus leuckartii*. I collected all I could find, and removed them, with some of the rotten wood, to a separate receptacle, and then carefully turned out the vivarium and examined its contents. I found that there were present four specimens of *Peripatus*, one male and three females, all apparently in good health, and that there was nothing else which could have laid the eggs; a very small ant being about the largest living thing present except the *Peripatus*. It is now some ten weeks since the vivarium was stocked, and as I have carefully examined it several times during that period, I am sure that the eggs must have been recently deposited.

The view that *Peripatus leuckartii* is really oviparous receives strong confirmation from anatomical examination of adult females. In these I have nearly always found eggs in the uterus, but, although I have dissected specimens taken in December, May, and July, I have never found any embryos. The single July specimen which I have yet dissected was captured at the end of the month and given to me by Prof. Spencer; it contained neither eggs nor embryos; as it appeared to be adult, it is not unlikely that the eggs had been laid. Moreover, the structure of the eggs *in utero* is very characteristic, and argues strongly against the probability of intra-uterine development. They are very large, oval in shape, and consist of a very tough, thick, elastic membrane inclosing a quantity of thick milky fluid full of yolk granules.

I have examined microscopically only one egg after laying, as I wish, if possible, to observe the development; but this one agrees so closely with those found *in utero* that there can be no doubt of its identity. It was of just about the same shape and size ($\frac{3}{40}$ inch long by $\frac{3}{50}$ inch broad), of a very pale yellow colour, with a very tough, elastic membrane, and a milky fluid contents containing very many yolk granules. The only difference concerns the almost chitinous-looking membrane, which, instead of having a smooth surface, or nearly so, as when *in utero*, is exquisitely sculptured or embossed in a beautiful and regular design. The design consists of curious little papillæ, somewhat resembling worm casts, arranged at fairly regular intervals over the surface of the egg, with much finer, close-set, meandering ridges occupying the spaces between them. Such sculpturing is, as is well known, characteristic of many insect eggs, and it renders those of *Peripatus* especially interesting in view of the relationships of that animal. As it is not present in intra-uterine eggs, it must be formed as the egg passes through the vagina, which is large and thick-walled.

It thus appears that *Peripatus leuckartii* lays eggs in July, and it appears also, from Mr. Fletcher's observations, that the young are hatched at the end of October. As, however, I have also found large eggs in the uterus of a specimen captured in December, I think it not improbable that the animal may be double-brooded. (I have used the term "uterus" in accordance with the customary nomenclature; it would be better, perhaps, to speak only of "oviducts" in *P. leuckartii.*)

The mode of reproduction of *Peripatus leuckartii* seems thus to differ widely from that known in all other species, and to conform rather to the insect type; and, considering the immense quantity of food-yolk present, it is probable that the development also differs in a similar way. This I hope to be able to work out, but the presence of so much fluid and granular yolk will, I fear, render the task very difficult.

ARTHUR DENDY.

University, Melbourne, July 31.

The Sun's Radiation of Heat.

A FEW months ago I sent to the *National Review* a paper, which the editors kindly inserted, on the sun's radiation of heat. So far as I am aware, my theory has been completely ignored by those best competent to form an opinion upon the subject. My contention seems so plausible that I venture to appeal to you to allow me to give the following brief exposition of my view, in the hope that I may be able to elicit some authoritative reply.

The amount of solar radiation is at present, for all intents and purposes, expressed in terms of melting ice. In other words, the sun is supposed to be giving forth as much heat as he would do were he surrounded, close to the photosphere, by a constantly renewed shell of ice, or never-failing ocean of water. My conception is, that, judging from what we know of hot bodies cooling upon the earth, it is impossible to believe that the sun could be pouring forth so much heat under existing conditions, as he would do were he continually to radiate to ice or water close to all parts of his surface.

The velocity, and the rapidity of vibration of the waves of light and heat can be accurately measured. This is the sum of motion—known as radiant heat—which the sun imparts to his surrounding medium. Absorbed heat is a very different thing (Balfour Stewart), and could not exist without the particles of matter. Now I fail to perceive what grounds the authorities have for calculating, as they do, that the sun's radiation amounts to something over a million calories per minute for each square metre of his surface. This means a million times the quantity of heat which will raise the temperature of a kilogramme of water 1° C. No doubt if the sun were surrounded by water the above would represent a correct estimate of the outflow of heat. But the men of science ignore, it appears to me, the marvellous virtue of the "if" in this case. The communication of heat consists in forcing the molecules and atoms of matter asunder against the attractions of cohesion and affinity, and causing the particles to vibrate; and there is no proof, but the evidence is all the other way, that the sum of motion imparted by the sun to the ether of space would represent anything like the expenditure of energy as would do the raising of water to an enormous temperature. If a red-hot globe of iron or copper were caused close to the surface to radiate to ice, the metal would cool much more quickly than if it were merely exposed to a very dry atmosphere—that is to say, the metal's radiant heat would constitute a less expenditure of energy than its emission of absorbed heat. I do not see, therefore, why we should not conclude that exactly the same result, only of course on a very vast time-scale, would happen in the case of the sun.

The enormously long periods demanded for the sun's past life-time by the geologist and biologist furnish strong antecedent support in favour of my contention. W. GOFF.

New University Club, S.W., August 15.

Morley Memorial College.

YOUR readers may be interested in hearing of a successful attempt to add another round to the ladder, described by Prof. Huxley, extending "from the gutter to the University." Some supporters of the Morley Memorial College for Working Men and Women, in the Waterloo Road, last year read an account in your pages of the arrangements made by the University Extension Society for some of its students to spend a month at Cambridge during the vacation. They resolved to offer scholarships to those who took the best places in the Christmas and Easter examinations in connection with Mr. McClure's astronomy class, whereby they might avail themselves of these arrangements. This, thanks to Dr. Roberts's kind co-operation, was successfully accomplished. Three students went to Cambridge, the most successful in a class all of whom did well. A plumber and a printer's reader went to Selwyn College, an elementary schoolmistress to Newnham. Two were able to take advantage of the whole month; the third (being a family man) could only spare a fortnight from his work, but all speak warmly of the pleasure and profit they have derived. The following are some extracts from their letters.

One says:—"I took chemistry and geology on alternate days, besides attending the majority of the single lectures. The work being mostly of a practical kind, has been intensely interesting." Another, after an enthusiastic description of the place, the architecture, and the College gardens, goes on:—"Everybody was most kind, cordial, and sociable, without the slightest suspicion of stiffness or formality, of condescension or patronage. More than this, everybody we met seemed to be studying our interests especially, and doing all in their power to make our stay as enjoyable as possible. . . . In science, geology was the only subject I was permitted to take up. In literature and art I attended courses on Browning and Tennyson, and on Greek art, Greek history, and Herodotus, also single lectures on 'Leopold Ranke,' . . . and 'College Life Past and Present.' I hope to continue these studies as far as possible in my home reading. . . Beyond the actual instruction received in the lectures, there has been given an impetus to further study, from which a continuous benefit must be reaped, and I have obtained a clear idea of what a student's life in a University town is like."

Cambridge opens its doors in this way only to members of University Extension classes, but at Oxford anyone may attend the classes who pays the fee. The authorities of our College accordingly offered scholarships to those of their students who passed highest in the Science and Art examinations for electricity, chemistry, and mechanical drawing. The results of these were not known early enough for the first half of the vacation classes, but the second fortnight in August was so much enjoyed that those who made the arrangements considered themselves well repaid for their trouble, though this was not small, for working men do not find it easy to get leave of absence for even a fortnight at a certain specified time. "One of the most enjoyable holidays I ever spent," writes one; "I have quite a collection of geological specimens collected on the excursion."

No wonder they enjoyed it! They come from surroundings generally dreary, sometimes squalid. They have scrambled for their education, and gained it under difficulties. They find themselves in a picturesque town, full of interesting associations, and meet with kindness without patronage from cultured men and women. Will not the school teacher's work have an added interest and dignity now she has seen (if only by a passing glimpse) what education is in its higher branches? Will not all of them feel that life contains something besides manual drudgery for weekly wages, and that those whose lot is exempt from drudgery of this kind are willing and anxious to share with them the results of culture and leisure? We live in times of a difficult transition from the old feudal loyalty to self-respecting friendship between free men, who can be mutually helpful to each other just because their circumstances and advantages are different. Feudalism was good in its day, but it has outlasted the conditions which made it so, and the "ladder from the gutter to the University" is an important instrument in effecting the transition safely to something better.

May I add that, unless the College and the scholarships receive wider support from the public than they have done, it will be difficult if not impossible to carry them on efficiently? Our fees are necessarily so low that the institution can never be self-supporting. We charge 1*s*. entrance fee, and 1*s*. 6*d*. a term for the first class; 6*d*. for each additional class. Larger fees would exclude some of our best students (one who had a perfect passion for knowledge was a rag-sorter till a better situation was found for him by one of our Council). The public imagine that we have already received a sufficient endowment from the City Parochial Charities fund. We hope shortly to have a grant from that fund, but we have lived on this hope for the last two years, and find it a sadly insufficient resource to provide intellectual food for 800 students. At this beginning of a fresh session we should gratefully welcome either personal help, or a subscription to general expenses or to the Scholarship Fund. A month at Cambridge costs about £7, and I have no doubt that (if the money were forthcoming) we should be able to arrange for scholarships to Cambridge from the University Extension Class on Physiography which Mr. A. W. Clayden is about to conduct. A fortnight at Oxford costs £5, and we hope this winter to have ten classes in connection with the Science and Art Department, to which we should like to offer this advantage.

September 9. EMMA CONS (Hon. Sec.).

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: WASHINGTON MEETING.

THE month of August 1891 was distinguished by the most notable array at Washington of scientific meetings ever held in America. The series began with the meeting of the American Society of Microscopists on August 11, and afterwards, consecutively or simultaneously, came those of the Association of American Agricultural Colleges and Experiment Stations; the Association of

Official Agricultural Chemists; the Society for the Promotion of Agricultural Science; the American Chemical Society; the Conference of American Chemists; the Association of Economic Entomologists; the American Association for the Advancement of Science; the Geological Society of America; and the International Geological Congress.

The fortieth annual meeting of the American Association for the Advancement of Science was held from August 19–25, President Albert B. Prescott, Professor of Chemistry at the University of Michigan, in the chair. The attendance of members was large; about one-third of all attending were residents of Washington, most of them employed in the various scientific Bureaus of the Government. 227 papers were read before the Sections, and these together with the addresses of the President and Vice-Presidents, Reports of Committees, and other documents, brought up the entire number to 291.

Prof. George L. Goodale, of Harvard University, delivered the annual address as retiring President: subject—"Some of the Possibilities of Economic Botany."

After giving an account of the meeting of the Australasian Association for the Advancement of Science, held at Christchurch, New Zealand, in January last, which he attended as delegate from the American Association, he proceeded to consider the subject above mentioned. An abstract of the address follows.

There is an enormous disproportion between the number of species of plants known to botanical science and the number of those which are used by man. The species of flowering plants already described and named number about 107,000, but the number of species used on a fairly large scale by civilized man does not exceed 1 per cent. The useful plants which are cultivated by man do not exceed one-third of this. Can the short list of useful plants be increased to advantage? After calling attention to the influence which synthetic chemistry exerts by the production of artificial vegetable products which can replace the natural products, he took up the cereal grains as illustrations of the history and improvement of cultivated plants. If all the cereals, like wheat, maize, rye, barley, oats, and rice, were now to be swept out of existence, we should not know positively where to turn for new species of grasses with which to begin again. He drew a picture of the condition of civilized man if all the known varieties of the cereal grasses should become extinct, and then pointed out the probable manner in which our experiment stations would have to choose and improve the grains of certain grasses which are not used to-day. He expressed the belief that our well-equipped stations would give us satisfactory substitutes for our cereals within a period not exceeding that of two generations of our race. But why do not experimenters attempt to improve our present neglected resources of this character? Because we all prefer to move in lines of least resistance, letting well enough alone. Plants which have been long cultivated are more susceptible to the influence of changes in surroundings, and hence of improvement, than those which are just removed from the field to the garden. Tracing the recent history of our cereals, he expressed his conviction that there is no probability that any new cereals will be added to our present list, but improvements will continue to be made in those which we have.

He included under the term vegetables all plants employed for table use, such as salads and relishes. The potato and sweet potato, the pumpkin and squash, the red or capsicum peppers, and the tomato, are of American origin. All the others are, most probably, natives of the Old World. Only one plant coming in this class has been derived from Australasia—New Zealand spinach (*Tetragonia*).

Among the vegetables and salad plants longest in cultivation are turnip, onion, cabbage, purslane, the large bean (*Faba*), chick-pea, lentil, and garden pea—which have an antiquity of at least 4000 years. Next in age are radish, carrot, beet, garlic, garden-cress and celery, lettuce, asparagus and the leek, three or four legumes, and the black peppers. The most prominent recent ones are parsnip, parsley, oyster-plant, artichoke, endive, and spinach. A few tropical plants, such as yams, are omitted from the list.

There is an astonishing number of varieties, which represent an enormous amount of horticultural work, each race (that is, a variety which comes true to seed) having been envolved by patient care and waiting.

For future development he recommends (1) *Arracacha esculenta*, of the parsley family, which is now cultivated in South America, near the Isthmus; (2) *Ullucus* or *Ollucus*, of the beet or spinach family; (3) the so-called Chinese artichoke from Japan.

He recommends a more thorough examination of Japanese vegetables, owing to the similarity of Japanese and Eastern North American flora.

Attention was called to the extraordinary changes produced in the commercial relations of fruits by canning and swift transportation, and the opinion was expressed that before long it would be possible to place many more of the delicious fruits of the tropics in northern markets; and even, with increasing knowledge of microbes, to preserve fruit for almost any reasonable time. Such discoveries would diminish zeal in the search for new fruits.

The improvement of fruits within historic times has been such that fruits which would once have been highly esteemed would to-day be passed by as unworthy of notice.

The list of seedless fruits may probably be materially lengthened. The common seedless fruits are banana and pineapple. Darwin mentions also bread-fruit, pomegranate, azarole, and date-palms; and says that their size and development are usually regarded as the cause of their sterility, whereas he regards sterility as rather the result than the cause of increased development.

Prof. Goodale expressed the conviction that there is no reason why we should not have seedless strawberries, blackberries, raspberries, and grapes, coreless apples and pears, and stoneless plums, cherries, and peaches, propagated by bud-division.

Promising timbers and cabinet woods, fibres, tanning materials, rubbers, and similar products were discussed in turn; the last class to be considered being fragrant flowers and plants for the florist. The necessity for caution in the introduction of new plants, lest they should prove pests by their wide dispersion through arable lands, as sweetbriar has in some parts of New Zealand, was fully illustrated. The agencies for examining useful plants were botanic gardens, museums of economic botany, and experiment stations.

Section A—*Mathematics and Astronomy.*

The address by Prof. E. W. Hyde, of Cincinatti, the President of this Section, was on the evolution of algebra, in which he traced the historical development of this branch of mathematics, beginning with the almost prehistoric Egyptian Ahmes; then giving a very full account of the Greek Diophantus, and explaining his use of syncopated methods. He had only one character to represent the unknown quantity; still he achieved great results. The Hindoos, Arya Bhatta about 600 B.C., and Brahma Gupta, 700 A.D., were discussed, and were presented as the source of Arabian algebra, and thus of the knowledge of that science in modern Europe.

Papers read before this Section include one on the latitude of the Sayre Observatory, by C. L. Doolittle, and on the secular variation of terrestrial latitudes, by George C. Comstock. The results of the investigations appear to be proof of a secular variation of the North Pole amounting to about 4½ seconds in a century.

Frank H. Bigelow exhibited and described a new aurora-inclinometer which will be sent to Alaska this autumn, and valuable results are expected in the study of the aurora.

One entire session of this and the Physical Section jointly was devoted to an elaborate monograph by A. Macfarlane, on principles of the algebra of physics.

SECTION B—*Physics*.

Prof. F. E. Nipher, President of Section B, opened the proceedings with an address on the functions and nature of the ether of space. Many reasons formerly given for the existence of such an ether, he said, no longer exist. For twenty-five years it was taught that light is an elastic pulsation in an incompressible jelly-like medium. In 1865, Maxwell proposed the theory that light is an electric displacement in a plane at right angles to the line of propagation. In 1888, Thomson showed that the compression wave required by the elastic theory, but absent in fact, might be dispensed with in the theory by making its velocity zero; and that this does not involve an unstable condition of the medium, and is therefore admissible. The showing up of light in space occupied by matter shows that the ether within must either be more dense (as Fresnel believed) or less elastic than that existing in free space. It is certainly very difficult to understand what there can be in the molecules of matter which can increase the density of an incompressible medium. The beautiful experiment of Michelson and Morley shows apparently that the ether at the surface of the earth moves with it. It is dragged along as if it were a vivid liquid. The field of a steel magnet is, however, a rotational phenomenon. It is a spin which is maintained permanently without the expenditure of energy. It seems, therefore, that the resistance to shear which shows itself in the adhesion of the ether to the moving earth must be a rigidity due in some way to motion. Other experiments of Michelson and Morley on the motion of light in moving columns of water have been taken as proof that the ether in water is condensed to nine-sixteenths of its volume in air. The ether in water certainly behaves as if it were more dense, but it is another matter to say that it is so. It seems improbable. It is still a mathematical fiction which covers a gap in our knowledge of the ether. The speaker thought that the experiment should be repeated with water at rest within a tube which should be mounted on elastic supports in a moving railway car. The water tube and observer's seat should be rigidly connected and swung on dampened spring supports from the top and sides of the car. The question to be settled is whether the ether or any part of it is at rest in space, and does it sweep through the interior of bodies which move through it as wind sweeps through the leaves and branches of a tree. This form of the experiment is the one contemplated by Eisenlohr's analysis leading to Fresnel's formula, and it is capable of great variations in the conditions of experiment. It is, however, more difficult and more expensive than the one so well executed by Michelson and Morley. Whatever its results may be, it promises to add greatly to our knowledge of the physics of the ether.

Prof. E. W. Morley, who has for several years been conducting researches under the auspices of, and with funds supplied by, the Association, read papers describing his method of determining the coefficient of expansion by means of interference fringes. He is able to determine the expansion of bars of any length as accurately as Fizeau did that of half-inch bars.

C. B. Thwing read a paper on colour photography by Lippmann's process, and exhibited samples which show a tinge of colour when looked at in the right light.

H. A. Hazen, of the U.S. Signal Service, discussed the question "Do tornadoes whirl?", and gave results of elaborate and careful study of tornadoes and of the *débris* left by them, from which he concludes that the common notion of a whirl in tornadoes is unfounded.

SECTION C—*Chemistry*.

Prof. R. C. Kedzie, of the Agricultural College, Michigan, chose the subject of alchemy for his annual address.

Thirty-three papers were read before this Section, and the meeting was characterized by the Secretary of the Section as the most valuable ever held.

Mr. Morley contributed valuable material to this Section also, in regard to the synthesis of weighed quantities of water from weighed quantities of oxygen and hydrogen. His determination of the ratio of atomic weights is: oxygen 15·888, hydrogen 1.

The Committee on Spelling and Pronunciation of Chemical Terms, which has been engaged in this work for several years, made their final Report, which will be printed and widely distributed, in order to secure uniformity if possible.

"Biological Functions of the Lecithines" was the title of a paper by Walter Maxwell In a paper presented by Mr. Maxwell at the 1890 meeting of the Association, he showed that a vegetable organism, during the initial stages of growth and under the action of the ferments operating in germination, possesses the power of taking the phosphorus present in seeds or in soils, as mineral phosphates, separating the phosphorus from the inorganic combination, and causing it to reappear in the young plantlet in an organic form as a lecithine. In brief, it was shown that the lecithine bodies are a medium through which the phosphorus of the mineral kingdom passes over into the vegetable kingdom. In the second part of Mr. Maxwell's paper he went on to show that the lecithine bodies, on the other hand, present in the animal kingdom revert to the mineral form under the action of the ferments present in the animal organism. The investigations were conducted with the egg of a hen. The phosphorus contained in the egg, in the respective forms of mineral phosphates and organic phosphorus compounds as lecithines, was determined. In the next place, the eggs were incubated, and the products of incubation were studied. It was found that the phosphorus contained in the natural egg as a lecithine reappeared in the incubation product as calcium phosphate, and forming the bone of the chicken.

In a paper by Dr. Gustav Hinrichs, facts were adduced to show that the logarithms of the molecular weights of the hydrocarbons have a direct relation to the fusing and boiling points of these substances. This is believed to be the instance discovered where logarithms exist between changes in physical or chemical condition.

SECTION D—*Mechanical Science and Engineering*.

The President of this Section, and *ex-officio* one of the Vice-Presidents of the Association, is Prof. Thomas Gray, of Terre Haute, Ind., the inventor of a great variety of ingenious apparatus, including the seismoscope and seismograph shown to the Association on their excursion to Terre Haute last year. His address was a carefully prepared discourse on problems in mathematical science. It was technical in character, and dealt with the teachings of mathematics and physics in their application to engineering.

Among the papers before this Section was one on Government timber tests, by B. E. Fermor, Chief of the Bureau of Forestry. He said there had been inaugurated in the forestry division of the Department of Agriculture a comprehensive series of tests and examinations of American timbers, the ultimate object of which is the solution of a biological problem—namely, to establish the relation of technical and physical qualities to each other and to conditions of growth. In the pursuit of this investigation, naturally, many questions of immediate practical value in the use of wood for engineering purposes will be solved. The novelty in this enterprise lies mainly in its comprehensiveness and scope. A very large number of tests alone on material of known origin and condition, and an exhaustive examination of the same will permit generalization and the recognition of laws of inter-relation. The work requires the organization of four distinct departments. First, the selection of test material from as many essentially different climatic and soil conditions as the species may occupy, five fully-matured and two young trees being carefully selected on each site and cut up for test material; secondly, the examination of the structure and physical condition of the test material, requiring the minutest detail; thirdly, the usual testing with special care; and, lastly, the compilation and comparative discussion of the results of the tests in connection with the physical examination and the known conditions of growth. Besides more trustworthy data than hitherto attainable of the qualities of our principal timbers, there is to be gained from this investigation a knowledge of conditions under which desirable qualities can be produced by the forest grower.

Prof. J. B. Johnson read a paper on the United States tests of strength of American woods, made at the Washington University Testing Laboratory, St. Louis.

SECTION E—*Geology and Geography*.

Prof. J. J. Stevenson, of New York, presided. His address was on the relations of the Chemung and Catskill on the eastern side of the Appalachian Basin. He traced the groups along the eastern outcrop from Tennessee into New York, across Southern and Western Pennsylvania, and eastward through Northern Pennsylvania again into New York, using the work of Prof. White and Messrs. Carill and Ashburne in Pennsylvania, and of Prof. Stevenson in Virginia and Pennsylvania, incidentally referring to the work of Profs. Hall and Williams in New York. In this way the continuity of the section was shown, and the insignificance of the variations was insisted upon strongly. An area in South-eastern New York and North-eastern Pennsylvania, in which the Chemung group is almost without trace of animal or vegetable life through the greater part of the thickness was described. The absence of life was thought to be due, not to fresh water, but to turbidity of the water in a shallow basin near the land. The facts that the horizons of fish-remains are much lower in the column than had been supposed, and that the plant-remains come in like manner from the home group, were thought to be of especial interest and importance. The conclusions to which the speaker was led were :—(1) That the series from the beginning of the Portage to the end of the Catskill form but one period, the Chemung, which should be divided into three epochs—the Portage, the Chemung, and the Catskill. (2) That the disappearance of animal and vegetable life on so great a part of this area toward the close of the period was due simply to gradual extension of conditions existing, perhaps, as early as the Hamilton period in South-eastern New York. (3) That the deposits were not made in a closed sea, but that the influx of great rivers with their load of *débris* made conditions in the shallow basin such that animal life could not exist. (4) That in the present state of our knowledge we are not justified in including the Chemung period in the Carboniferous age.

Notwithstanding the impending meetings of national and international Geological Societies, this Section was fully occupied with papers and discussion, mainly on the Glacial epoch, drift, &c. Mr. William Hallock read a paper entitled "A Preliminary Report of Observations at the Deep Well, Wheeling, W. Va." The question as to the conditions which exist in the interior of the earth, said Mr. Hallock, has always attracted much attention. The most important factor in the solution of this riddle is the determination or estimation of the temperatures there existing. The British Association has for years seized every opportunity to obtain data as to the rate at which the temperature increases as the earth crust is penetrated. The most recent and trustworthy contributions on this subject are by Mr. E. Dunker, of Halle, Germany, and were obtained from a 4170-foot well at Sperenberg, not far from Berlin, and a 5740-foot well at Schladabach, near Leipzig. These wells are both full of water, the circulation of which vitiates results or renders elaborate apparatus indispensable, and the thermometers must be protected from the pressure. The Wheeling deep well, sunk by the Wheeling Development Company, and by them generously dedicated to science, is 4500 feet deep, $4\frac{7}{8}$ inches diameter, and dry; cased only to 1570 feet. The strata there are nearly *in situ*, undistorted and dipping only 50 feet to the mile. More satisfactory geological conditions can scarcely be imagined. Being dry, ordinary United States Signal Service maximum thermometers were used, and no especial precaution needed to be taken to prevent circulation of the air. The thermometers were lowered and raised, and depths measured by a steel wire. Results :—

TABLE I.

Depth. Feet.	Temperature, Fahrenheit. Degrees.	Depth. Feet.	Temperature, Fahrenheit. Degrees.
1350	68·75	3125	88·40
1591	70·15	3232	89·75
1592	70·25	3375	92·10
1745	71·70	3482	93·60
1835	72·80	3625	96·10
2125	76·25	3730	97·55
2236	77·40	3875	100·05
2375	79·20	3980	101·75
2486	80·50	4125	104·10
2625	82·20	4200	105·55
2740	83·65	4375	108·40
2875	85·45	4462	110·15
2990	86·60		
		100	51·30

These observations, when plotted, show a slow increase for the upper half of the uncased portion, about 1° F. for 80 to 90 feet, whereas the lower part shows a more rapid increase—about 1° F. for 60 feet; the whole series giving a well-defined and regular curve, with a deflection at 2900 to 3000 feet, where an oil sand occurs. Practically all the rest of the uncased well is in shale. The increase in the rate at which the temperature rises as the bottom is approached can only be temporary, or we should have an inconceivable or improbable state of temperature at comparatively slight depths. The two distinct series of observations combined in Table I. nowhere disagree more than 0°·3 F., and hence are very trustworthy and accurate. Table II. gives a comparison of the results at the three great wells :—

TABLE II.

Name of well and location.	Feet for 1° F. Feet.	Total Depth. Feet.	Temperature at top. Degrees.	Temperature at bottom. Degrees.
Sperenberg, near Berlin	59·2	4170	47·8	118·6
Schladabach, near Leipzig	65·0	5740	51·9	135·5
Wheeling Development Company	—	4500	51·3	110·3
Top and greatest depths	74·3	4500	—	—
Mean of lower 3000 feet	75·4	4500	—	—
Mean of above two	74·9	4500	—	—

Inasmuch as the bottom of the well is some 3700 feet below sea-level, it seemed worth while to attempt barometer readings in it. The instruments used proved ill adapted to the work, and the results were unsatisfactory. Samples of air were taken at the bottom, but could not be analyzed in time for use. A series of observations in a coal mine near the well gave as a very probable value of the temperature of the top invariable stratum 51°·3 F. From the mean annual temperature of Marietta and Steubenville it might be taken at 52°·2 F. Drilling is temporarily stopped, but it is hoped that a depth of 5500 or 6000 feet may be reached. Mr. Anton Reyman, of the Development Company, has generously guaranteed half the expenses, and what is wanted is that some one shall furnish the other 3000 dollars, and enable the Wheeling well to be lifted from the second to the first place among the deep wells of the world.

SECTION F—*Biology*.

Prof. John M. Coulter, President of Indiana State University, gave the annual address, as President of Section F, on the future of systematic botany. He contended that for the systematists of to-day and of the future there must be three distinct lines of work, related to each other in natural sequence in the order presented, and each turning over its completed product to the next. (1) *The Collection and Description of Plants.*—He expressed great gratitude to the noble army of self-denying pioneer collectors, but claimed that the time had now come when the same amount of labour could be expended to better advantage, and that a race of field workers must be trained who shall follow their profession as distinctly and scientifically as the race of topographers. In reference to the work of description he read an unpublished note of Prof. Asa Gray, in which that distinguished botanist lamented the work of those who were incompetent. The speaker also expressed the opinion that the exclusive use of gross organs in the description of higher plants would be given up, and that the more stable minute characters would prove valuable aids in steadying diagnosis. A danger in the use of these minute characters was pointed out, viz. the tendency to use a single set of minute characters too far, and to make the fabric of a whole group conform to it. The character of a species is an extremely composite affair, and it must stand or fall by the sum total of its peculiarities, and not by a single one. There is nothing that involves a broader grasp of facts—the use of an inspiration rather than a rule—than the proper discrimination of species. (2) *The Study of Life-histories.*—The work of searching for the affinities of great groups is the crying need of systematic botany to-day. The speaker called attention to the danger of magnifying the importance of certain periods or organs in indicating affinities, and summed up what was said under this general head as follows :—"I have thus spoken of the study of life-histories to indicate that its chief function lies in the field of systematic botany; to suggest that it take into account development at every period and of every organ, and so obtain a mass of cumulative evidence for safe generalization; and to urge upon those not thoroughly equipped great caution in publication." (3) *The Construction of a Natural System*.–The speaker spoke of the necessity of constructing a natural system with easy advance in the knowledge of affinities, as a convenient summary of information, a sort of mile-post, to tell of progress and to direct future effort. The concluding summary was as follows :—" The points presented in this consideration of the third phase of systematic botany are that the last and highest expression of systematic work is the construction of a natural system, based upon the accumulations of those who collect and describe, and those who study life-histories ; that this work involves the completest command of literature and the highest powers of generalization ; that it is essential to progress for a natural system to be attempted with every advance in knowledge ; and that all the known facts of affinity, thus brought within reach, should be expressed in all systematic literature."

This Section, as usual, was the most crowded of all, forty-seven papers having been read before the Section itself, and many more before its two offshoots, the Botanical and the Entomological Club. This was another of the Sections which its Secretary considered to have had the most successful meeting on record. A feature now at every annual session is the report of members appointed the year before to study certain assigned questions. This year four such reports were presented :—Transpiration, or the loss of water in plants, was treated by Chas. E. Bessey and Albert F. Woods. " Movements of fluids in plants " was read by Prof. Wm. J. Beal, of Michigan. Dr. J. C. Arthur, of Purdue University, Lafayette, Ind., read a paper entitled " Gases in Plants." A paper was read by Prof. L. H. Pammel, of Ames, Iowa, on the absorption of fluids by plants.

SECTION H—*Anthropology*.

The youngest Vice-President at this session, if not the youngest man who ever held a Vice-Presidential office in the American Association, is Prof. Joseph Jastrow, whose age is 28 years. His address was entitled " The Natural History of Analogy."

Major J. W. Powell, Chief of the U.S. Geological Survey, exhibited and explained his linguistic map of North America, on which he showed the classification of languages of the aborigines.

Mr. Cushing read a paper on the Zuñi Indians, and danced the Messiah dance, which a few months ago was so much talked about, and almost involved a war with the Government.

SECTION I—*Economic Science and Statistics*..

Of all the Vice-Presidential addresses, that of Prof. Edmund J. James, of Philadelphia, before this Section, aroused the most widespread popular interest and attention, on account of the vital practical importance of the theme presented, which was " The American Farmer : his present economic condition and future prospects."

The silver question was carefully considered, and all who took part in the discussion agreed in opposing the free coinage schemes which are now so vehemently urged upon Congress.

The general business of the Association included a change in the constitution, so as to admit fifty foreign honorary members, and many recommendations to Congress as to forestry, water supply and management, and other topics. Preliminary arrangements were made to participate in the Columbian World's Fair in 1893. A Committee was appointed to solicit donations for the endowment of the Association with a fund of at least $100,000. Three hundred and seventy-one new members were elected, bringing the total membership up to about 2300, which is high-water mark in the history of the Association.

Prof. Joseph Le Conte, of California, was elected President ; and the Association adjourned, to meet at Rochester, N.Y., on the third Wednesday of August 1892.

RAIN-MAKING IN TEXAS.

THE announcement in the *Standard* about a fortnight since, that rain had been artificially produced in Texas by exploding oxyhydrogen balloons and dynamite, was probably received by most scientific men with a suspension of judgment. The somewhat sensational form of the report, the emphasis with which it dwelt on the unfavourable antecedent conditions, and the omission of all details that might enable us to form some rough estimate of the forces employed and of the resulting effects, seemed calculated to appeal to the barren emotions of astonishment and love of the marvellous rather than to the sober judgment of well-balanced minds ; and but for the fact that the experiments were stated to have been made by the officers of the U.S. Signal Service, which, on the hypothesis of a hoax, would have been a needless challenge to speedy denial, one might have been disposed to regard the story as only an additional instance of a kind of produce for which the Western States are somewhat notorious. The further accounts that have now reached us prove, however, that this is not one of Jonathan's amusing attempts to play off on the credulity of his simple-minded cousins across the Atlantic. Not only have experiments of the kind described been actually made, but they have been apparently successful, and they seem to have been repeated sufficiently often to render it at least improbable that this success has been entirely fortuitous. The improbable features of the *Standard's* report are, indeed, somewhat toned down ; the dryness of the local atmosphere was by no means so great as was to be inferred from the vague language of the *Standard's* informant ; but, as far as can be judged from the notices now before us, it seems unlikely that the rain which followed General Dyrenfurth's experiments would have occurred in the undisturbed course of natural events.

The experiments were made at a place known as Ranch C. One writer states that an intermittent series of experiments had been carried out for three weeks, and that " not in a single instance has rain failed to fall within ten or twelve hours after the explosion." But the number of trials is not stated—an omission the more to be regretted, because the improbability that the results are fortuitous increases in a certain geometric ratio of the number of successful repetitions. We have definite accounts of those made on August 18, 26, and apparently the morning of the 27th, and it is by no means clear that the evidence is not limited to these, although the expression quoted above would seem to imply otherwise. The first, that of August 18, was made about 3 p.m. There were at the time a few scattered clouds, but no indication of rain. The reading of the barometer is not reported, but the relative humidity of the air immediately before the experiments (presumably at the earth's surface) was not more than 60 per cent. of saturation. An oxyhydrogen balloon, the capacity of which is not stated, was exploded by electricity at an altitude of a mile and a quarter. Several kites, with packets of dynamite attached, were sent up immediately after the balloon, and the charges exploded by similar means, and " rendrock powder was distributed for a distance of two and three-quarter miles from head-quarters, and fired by igniting dynamos." These explosions " sent up great volumes of white smoke, which rose only a short distance, and was then beaten down by heavy rain, which at once began falling and continued for four hours and twenty minutes." Prof. Curtis, the meteorologist of the expedition, estimates that the rain covered an area of not less than 1000 miles.

On August 26 it is stated that " balloons containing several thousand feet of oxyhydrogen gas were sent up and exploded at heights varying from 1000 to 10,000 feet, and at sundown batteries on the ground began their work, and until 10.30 p.m. a constant cannonade was carried on under a sky of perfect clearness, lit by countless stars of a brilliancy seldom seen in the north. The barometer promised fair, and the hygrometer stood between dry and very dry," whatever these expressions may mean. The account continues :—" At 11 p.m. General Dyrenfurth withdrew his forces, and all retired for the night. Sleep, however, was soon interrupted, for at 3 a.m. the first return fire flashed from the heavens, when the rain-makers were roused by a crashing peal of thunder, and the rain was soon beating on the roof. At sunrise a magnificent double bow arched the heavens, and the downfall of rain did not cease till 8 o'clock a m. A number of heavy charges of dynamite were then made, and after every one the drops again poured down, till at last the clouds were entirely expended."

In these quotations is given all that is essential in the newspaper reports now before us. Although deficient in many details that it would be desirable to know, they are written by one who witnessed what he described, and there seems no reason whatever to doubt their genuineness and good faith; we may therefore, discuss the information they afford, without misgivings of its substantial trustworthiness.

It is not antecedently improbable that, in certain states of the atmosphere, the liberation of a large amount of heated gas consisting wholly or in great part of water vapour, at an elevation where aërial movements are but little retarded by terrestrial friction, may suffice to generate an ascending current; and elementary physical considerations teach us that a mass of air that would be called relatively dry at the lower level, will in ascending speedily become saturated and condense its surplus vapour, first as cloud, and eventually as rain, not indeed by acquiring more vapour, but in virtue of dynamic cooling as it progressively expands under the diminished pressure of greater altitudes. But unless the atmospheric strata thus immediately affected be already in a condition of unstable equilibrium, unless the vertical decrease of temperature in these strata is already somewhat greater than the adiabatic rate of decrement, so that the ascending movement once started can be maintained by the store of energy already present in the form of sensible temperature and the latent heat of the included vapour, the effect must of necessity be temporary and local—more of the nature of a small thunder-storm, or cloud-burst, than of the widely extended or sporadic rainfall that accompanies a barometric depression.

In fact, the possibility of rainfall production depends on the possibility of producing and maintaining an upward movement in the atmosphere. There is always some vapour present in the air, generally sufficient to form clouds when dynamically cooled by an ascent through two or three thousand feet; although such air, while resting on the ground and warmed by its contact, may be very dry as judged by our feelings and by the evidence of the hygrometer. The amount of energy yielded by any moderate provision of oxyhydrogen balloons and dynamite is but infinitesimal in comparison with that already locked up in the atmosphere and its vapour, and which, under the circumstances above specified, viz. a vertical decrease of temperature exceeding a certain fixed rate, is available for maintaining a movement once set up; and the part played by the heated gases of such experiments as those now described can be little more than that of a trigger that releases a detent.

It seems highly probable that on August 18 the atmosphere was in this unstable condition. Even in the warm stratum resting on the ground, the humidity was 60 per cent. of saturation, clouds (indicating saturation) existed at some height, and rain began to fall almost immediately on the conclusion of the explosions. It may be noticed, too, that the time of day was that at which the barometer is lowest and the humidity highest in the cloud-forming stratum, although, in fine weather, lowest at the ground surface. In the absence, then, of any observations of the temperature and humidity of the strata primarily stirred up by the exploding balloons and dynamite, it seems likely that they were in a condition to maintain ascending currents once started, and even to communicate the disturbance to regions around.

On the 26th, the atmosphere was evidently in a much more inert condition, and four hours elapsed before rain fell, the disturbance being then apparently more local, and of the nature of a thunder-storm. However, with the meagre data as yet before us, it would be premature to attempt any critical discussion of the processes in operation.

It is needless to say that popular theorizing, on this as on most other physical phenomena, concerns itself chiefly with the things that are most obvious to the senses, but often have little or nothing to do with the process. Thus we find that attention has been fixed on the explosion; and we are told that the idea of breaking clouds by producing a motion in the air, and so destroying the equilibrium of the suspended globules of moisture, which in coalescence form rain, is not a new one; that it was the custom to keep a cannon in French villages, with which to fire at passing clouds and thus hasten the downpour; that at the battles of Dresden and Waterloo the concussion of the air by the cannonade led to the descent of torrential showers; and we are reminded that "in the same way" rain follows a peal of thunder caused by the passage of a lightning-flash through a moisture-laden atmosphere, &c. Now, all this noise and disturbance have no more to do with the production of rainfall than has the thrashing which the village rain-maker of Central India receives from his fellow villagers to stimulate him to fresh exertions when he is thought to have neglected the performance of his official duties, or the London street-boy's whistle, with which Sir Samuel Baker startled a rain-making king in the Southern Soudan, and which was followed by such a deluge that even the rain-making potentate implored him to arrest the working of the spell.[1] The effect of a concussion, as such, is to produce an instantaneous compression of the air, and a momentary heating in a wave which travels away at the rate of about 1000 feet per second, and is incapable of generating any translational movement of the atmosphere, and certainly of promoting condensation. Nor do we know of any recorded observations in support of the idea that it can cause the coalescence of cloud corpuscles into raindrops. Neither does the concussion of the air by a thunder-clap stand to the downpour that follows it in the physical relation of cause to effect. In this case Sir John Herschel adopts the opinion originally put forward by Eeles, that the order of succession is the reverse of that here assumed, that the formation of the rain-drop is the antecedent phenomenon, and the lightning-flash (and *ergo* the thunder) the consequent; the electrical discharge being determined by the sudden concentration of the electricity of (say) one thousand corpuscles on the surface of the single resulting rain-drop, in which case its intensity would be increased ten-fold. What causes the coalescence is still a matter of much obscurity, though some light has been thrown upon it by the ingenious experiment exhibited by Mr. Shelford Bidwell at the Royal Society's *conversazione* on May 14, 1890, and described in vol. xlii. (p. 91) of this journal. When the shadow of a small (condensing) steam jet was thrown upon a white screen, under ordinary conditions, it was of feeble intensity and of a neutral tint; but when the jet was electrified, the density of the shadow was at once greatly increased, and it assumed a peculiar orange-brown tint. It appeared that electrification promoted the coalescence of the exceedingly minute particles of water contained in the jet, thus forming drops large enough to obstruct the more refrangible rays of light. On this view, then, electrification would appear to be the cause of coalescence, and the electrical discharge the ulterior result; but as yet we know too little of the molecular processes concerned in the formation of a raindrop to attempt anything like a complete theory.

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[1] This story has probably been told by Sir Samuel in one of his well-known works on Africa, and is too good to be spoilt by condensation. It is, at all events, authentic, the present writer having heard it from his own lips at a Simla dinner-table.

In conclusion, while we cannot but recognize the high interest of General Dyrenfurth's results, with the imperfect information at present before us we cannot regard them as conclusive. It is the characteristic weakness of all experiments of the kind that many of the essential circumstances are scarcely ever recorded, or perhaps even capable of being brought within the limits of observation: and thus the logical conditions of a proved conclusion cannot be fulfilled. For instance, it is very unlikely that anything is known of the state of the atmosphere in respect of its humidity and its vertical temperature decrement at the elevation at which the balloons were exploded, and yet, as we have seen, these data lie at the very root of the whole matter. However, arrangements are being made for further operations at El Paso and in Western Kansas, so that it will not be long before the highly interesting and practically important problem of stimulating the precipitation of rain will receive a more satisfactory solution.

H. F. B.

NOTES.

THE Permanent Committee of the International Committee of Weights and Measures is now holding its meeting at Sèvres, near Paris. The Committee includes: Dr. Foerster (Germany); M. J. Bertrand (France); Dr. Benoît, Director of the Bureau at Sèvres; Mr. H. J. Chaney (Great Britain); Prof. Govi (Italy); Prof. Krusper (Hungary); Prof. Lang (Austria); Mr. H. de Macedo (Portugal); M. Stas (Belgium); Prof. Thalen (Sweden); Dr. Wild (Russia). The Committee has recently lost its President (General Ibañez); and one of the objects of the present meeting is to elect a new President; an election which will doubtless fall on the senior member of the Committee, Dr. Foerster.

THE members of the Heilprin Expedition, who have lately returned from the west coast of Greenland, give an extremely unfavourable account of the position in which they were obliged to leave Lieutenant Peary. His leg was broken in Melville Bay on July 11. Dr. Hughes, who has recorded in the Philadelphia *Press* the adventures of the Expedition, describes how the accident happened. "While we were going astern for the last time," he says, "to make the butt that forced us through a barrier of ice into comparatively clear water, Lieutenant Peary stepped behind the wheel-house to see how things were going. With a crash the rudder struck a piece of ice, and the next instant his leg was crushed between the rudder gearing and the side of the wheel-house. He was carried below into the cabin, when an examination showed that his right leg was broken square across just above the knee. Everything possible was done for him." When he had recovered from the shock, and had thought the matter over, he decided to go on to Whale Sound, trusting that by next spring his leg would be so far mended that he would be able to accomplish the object of his expedition. His friends thought it would be better for him to return, but they could not help admiring his spirit, and resolved to do everything in their power to further his aim. The shores of Whale Sound proved to be completely blocked with ice, so the *Kite* steamed north to McCormick Bay, on the northern shore of Murchison Sound, which they reached on July 25. Here a space of about two miles was comparatively clear; and Lieutenant Peary's men went ashore, and reported that the place was well suited for their head-quarters. A site was selected on the south shore of McCormick Bay, in latitude 77° 43′, and a wooden house erected, which Lieutenant Peary declared to be "substantial and warm enough." On July 30 the Heilprin party had to leave him, which they did with sad forebodings. Mrs. Peary bravely insisted on remaining with her husband, and they have six companions. The Lieutenant hopes to start in the spring for the unexplored interior of Greenland, but Dr. Hughes says: "It is the deliberate opinion of all our party—and this opinion is indorsed fully by all the officers of the *Kite*—that unless a relief expedition be sent to Lieutenant Peary next summer, he and his party will never be seen again alive." It is doubtful whether the food supply is sufficient; and it is thought most improbable that whalers will take them away next summer. In that case their only resource would be the whale boats, in which they would have to traverse 500 miles of ocean "filled with floes and bergs, and often shrouded with fog or swept by terrible storms."

AN earthquake of great violence caused immense damage in the Republic of San Salvador on September 9. According to reports sent from the capital of the country to the *New York Herald*, there had been indications for several days that a seismic disturbance of more than usual power might be expected. The volcanoes of San Salvador, San Miguel, and Izalco had been unusually active, and deep subterranean rumblings with slight earth tremors had been felt. At 1.55 a.m., on September 9, the earthquake began in the city of San Salvador with a slight tremor, which gradually augmented. The duration of the first shock was ten seconds, during which time a frightful subterranean noise was audible in every part of the city. While the shock lasted, the earth rose and fell in long waves, and even strong men were unable to keep their feet. The walls of houses cracked, and then tottered and fell. In the capital alone 40 persons were killed, and 50 or 60 seriously injured. The experience of towns in the country seems to have been still worse. Of 320 houses at Comasagua only eight remain standing, and the loss of life there was great. Analquito has also been almost completely destroyed, and Cojutepeque, Santa Tecla, San Pedro, and Masahuet were so badly shaken as to be practically ruined. It is feared that the earthquake has been even more disastrous than those of 1854 and 1873.

IN the Isle of Fayal, among the Azores, several shocks of earthquake were felt on August 27 and 28.

MR. TUCKWELL writes to us from Loughrigg, Ambleside, that an aurora was seen there on Friday night, September 11. The arch spanned the heavens from south-west to north-east, passing nearly through the zenith. It was white, with slight coruscations at its south-west base. It was first seen at 9 p.m.: it had faded by 10 o'clock.

A NEW department of physics and electrical engineering will be begun this session at the new branch of the Manchester Technical School in Whitworth Street, where a large well-lighted warehouse is being fitted up for the purpose. The building will be lighted by electricity, the installation being fitted up with especial regard to instruction. For the latter purpose, the electric light installation in the Central School in Princess Street will also be available.

THE Library Association is holding its annual meeting this week at Nottingham. Mr. Robert Harrison, of the London Library, presides. The meeting began yesterday in the large theatre of the Nottingham University College.

THE Industrial Society of Mulhouse has issued a programme of prizes which it proposes to give for work done in the year 1891–92. A copy will be sent to anyone who applies for it to the Secretary of the Society. The prizes are very numerous, and are to be granted for work of many different kinds in connection with the application of scientific methods to industry.

A Conference on Conifers will be held at Chiswick, in connection with the Royal Horticultural Society, in October. It is hoped that this Conference will not only draw attention to the best of these trees and shrubs from a garden or landscape point of view, but show what are the best varieties to plant for English-grown timber, as well as the different uses and suitabilities of the various foreign-grown timbers. The co-operation of landowners and others who may have planted these trees or shrubs in years past, or who take a present interest in them, is specially invited.

Dredgers working in the Tiber to prepare for the construction of a new embankment brought up on September 12 a magnificent ancient Roman bronze helmet. It is perfectly preserved, and is decorated with bas-reliefs. Signor Rossi, the Italian archæologist, assigns it to the second century before the Christian era.

According to the Calcutta correspondent of the *Times*, it is understood that the Ameer of Cabul is taking steps to obtain from England a geologist, a chemist, two miners, and a number of mechanics.

The Royal Meteorological Institute of the Netherlands has just issued another useful work in maritime meteorology, viz. "Routes for Steamships between Aden and the Straits of Sunda." A previous edition appeared in 1881, but since that time steam navigation to the Dutch Indies has greatly increased, and consequently the number of logs received has afforded sufficient materials to allow of a fuller discussion of the outward and homeward routes for each month. Although there is a certain amount of regularity both as regards the monsoons and currents, yet there are considerable differences both in force and direction in the same months of different years, which cannot be taken into account in laying down general routes; but tracks laid down with great care from the most complete data available will give the best chance of successful passages. We cannot enter here into the details of the results, but we may mention that the tables and charts contained in the work show for each 10° of longitude the number of vessels which have cut those meridians in different latitudes, and the means of the number of hours taken. The tracks show that a very considerable divergence from the most direct routes is recommended in certain months, according as the east or west monsoon may be blowing. The usefulness of the work is attested by the fact that copies have been ordered for their vessels by the French, Russian, and Italian Governments.

It is expected that in no other department of the "World's Columbian Exposition" will there be a greater diversity of exhibits than in that of mines and mining. Not only will there be a magnificent array of diamonds, opals, emeralds, and other gems, and of the precious metals, but a most extensive collection of iron, copper, lead, and other ores, and of their products; of coal, granite, marble, sandstone, and other building stone; of soils, salt, and petroleum. A sub-department will take special charge of the coal and iron exhibit, and later of that of copper and lead.

Mr. O. Chanute, a well-known engineer of Chicago, has been studying the methods of preparing wood chemically to resist decay, and has expressed the opinion that great economies might be realized in America by the general adoption of these methods on railways. *Science* says he recently examined some experimental railroad ties of the most perishable kinds of wood prepared by what is known as the zinc-tannin (Wellhouse) process, in St. Louis, in 1881 and 1882, and laid in the tracks of the Atchison, Topeka, and Santa Fe Railroad, at Topeka, Kan., and La Junta, Col. After nine or ten years' exposure they show excellent results, whereas they would have lasted but from one to four years if unprepared. Unprepared ties of the same kind of timber, laid at the same time, adjoining to the prepared ties, have all decayed and been taken up, while present appearances indicate that the prepared ties (red oak, black oak, and Colorado pine) are likely to show an average life of ten to fifteen years or more. Mr. Chanute calls attention to the fact that the zinc-tannin process not only preserves ties against decay, but hardens them as well. It is found on one railroad that after three years' exposure treated hemlock ties hold the spike as well, and cut less under the rail than untreated white oak.

Some time ago the Field Naturalists' Club of Victoria organized an excursion to the Kent group of islands, the object being to collect specimens, and to determine whether the group is most nearly related with Victoria, to which it is closest geographically, or with Tasmania. At the annual *conversazione* of the Club, held recently, Mr. C. A. Topp, the retiring President, referred to the results of the expedition. The bulk of the fauna and flora was found to be common to Victoria and Tasmania, but there were six or seven varieties of birds peculiar to Tasmania to two peculiar to Victoria. The conclusion was that the islands had been separated from Tasmania after that island was disjoined from the mainland. Among the plants, several forms were found varying somewhat from the typical forms of the same species on the mainland; while it was interesting to find that the arboreal short-eared opossum had changed his habits so far as not to feed on the leaves of the eucalypt, and to keep to the ground.

In a paper in the American *Engineering Magazine*, on ventilation, Mr. Laurence Allen contends that in very many schools the quantity of pure air admitted is not sufficient to expel the foul air. To maintain the air in a good sanitary condition in a properly constructed schoolroom, his experience confirms the amount required as stated by Billings, to wit, 60 cubic feet of air for each occupant per minute. For 100 pupils this amounts to 360,000 cubic feet per hour. How many schools come up to this requirement? In the United States, says Mr. Allen, there are many schools that contain 100 pupils and do not introduce more than 25,000 feet of pure air per hour, and even that is rendered in a measure ineffective, because the air is not properly admitted. "The pupils do not die in the poisoned atmosphere; many of them will appear reasonably healthy. So do many persons who visit and tarry in malarial districts. But though the effects are not immediate and striking, they are sure, permanent, and easy to be traced to their causes in after years, by those who make a study of disease and its causes. It is scarcely less humane to kill a child than, by wilfully ignoring sanitary requirements, to cripple it for life, physically, mentally, and morally, as children are being crippled to-day in the vile atmosphere of many schools."

In a paper published in the current number of the Journal of the Anthropological Society, Mr. J. J. Lister refers to the great development of the arms and chests of the natives of Fakaofu (Bowditch Island, Union Group). He thinks it may be due to the fact that they are obliged to go about so much in canoes. Sir Joseph Lister, who took part in the discussion which followed the reading of this paper, remarked that he would not have expected the frequently repeated action of paddling to produce lengthening of the arms, although he could understand its resulting in increased size of chest. He pointed out that the natives of Tonga were also accustomed to use canoes, and hence it was not clear that the phenomenon could be traced to the cause assigned. Mr. Lister replied that, although the Tongans use canoes, canoe work is not so essential a part of their lives as it is in the case of the natives of Fakaofu. The natives of the island of Tongatabu have many avocations quite apart from the sea, for they live on an island twenty-two miles long, and many villages are situated some distance from the water. The natives of Fakaofu, on the other hand, live crowded together on a small islet situated on a ring of reefs, and to meet almost every need of their lives they must do more or less paddling.

MR. IVAN PETROFF, the United States special census agent, has been engaged in taking the census of the natives of Nunivak Island, in Behring Sea, in 60° N. lat. He found the population to consist of over 600 natives. It was previously supposed that over 300 people occupied the island. There are no white men there, and the natives live in a most primitive style. Their only food is the flesh of the walrus, and their only wealth consists of ivory obtained from the tusks of that animal. There are few land otter, but, apart from these, the natives catch no fur-bearing animals.

DR. L. WEBSTER FOX is of opinion that savage races possess the perception of colour to a greater degree than do civilized races. In a lecture lately delivered before the Franklin Institute, Philadelphia, he stated that he had just concluded an examination of 250 Indian children, of whom 100 were boys. Had he selected 100 white boys from various parts of the United States, he would have found at least five of them colour-blind: among the Indian boys he did not discover a single case of colour-blindness. Some years ago he examined 250 Indian boys, and found two colour-blind, a very low percentage when compared with the whites. Among the Indian girls he did not find any. Considering that only two females in every 1000 among whites are colour-blind, he does not think it surprising that he did not find any examples among the Indian girls.

DR. J. FRANK lately reported to the Chicago Medical Society the case of a man who periodically sheds his skin. The shedding began in his first year, and has since then occurred regularly every July. He is taken with feverish tremors, increasing almost to paroxysms. He undresses, lies down, and within a few minutes the skin of the chest begins to turn red. The redness rapidly extends over the entire skin, and the feverish tremors continue uninterrupted for about twelve hours. Then he rises, dresses, and walks about in perfect health. The skin now begins to peel, and ten hours later it comes off in great patches. From the arms and legs it can be peeled off exactly like gloves or stockings. As the old skin comes away, a new epidermis, as soft and pink as a baby's, is revealed. This new skin is very sensitive; the patient has to wear softened gloves and moccasins for about a week. After the old cuticle has been entirely removed, the finger and toe nails begin to drop off—new nails literally crowding them out. Finally, the change is complete, the man has a new skin and a new outfit of nails, and is ready to return to the mines. A lady in Washington County, Nebraska, who is thirty-nine years old, has written to Dr. Frank that since 1876 she has had a like experience every second or third year.

THE Orcutt Seed and Plant Company, San Diego, California, have issued an interesting descriptive list of Californian trees and flowers. The writer thinks that there is perhaps no country in the world where the early spring flowers so change the face of the earth from a desolate waste to a beautiful garden as on the Pacific coast—hills, mesas, mountains and valleys, and the arid plains of the desert, alike quickly responding to the vivifying rain. "California," he says, "has probably already furnished to the horticulturist a greater variety of beautiful flowers and stately trees than any other State in the Union. Yet many others are awaiting the appreciation of man, or wasting their sweetness on the desert air."

A PAPER on malformations of the bill in birds, by Mr. W. P. Pycraft, has been reprinted from the Transactions of the Leicester Literary and Philosophical Society. The most common kinds of malformation are those resulting from overgrowth of the horny sheath, and those arising from injury. Mr. Pycraft discusses these first, and then considers malformation due to embryonic disturbance.

"SYMONS'S British Rainfall, 1890," which has lately been published, contains, we need scarcely say, an enormous mass of information as to the distribution of rain over the British Isles during the year to which the volume relates. Mr. Symons points out that the only important alteration in this issue is that due to the completion of the decade 1880–89, which has enabled him to use the average for that period as a basis of comparison. He also calls attention to an article on the evaporation from soil, and to the details given as to the great rain of July 17.

THE operatives' lecture delivered at the Cardiff meeting of the British Association by Prof. Silvanus P. Thompson has been published by Messrs. E. and F. N. Spon. The subject is "Electricity in Mining."

"THE Hand-book of Jamaica for 1891–92" has just been issued. This is the eleventh year of publication. Mr. S. P. Musson and Mr. T. Laurence Roxburgh have done their best to present the fullest and latest information obtainable; and everyone who has occasion to consult the book will appreciate the care and thoroughness with which their task has been fulfilled.

A NEW edition, revised and enlarged, of the "Alkali Makers' Pocket-book," by Prof. Dr. Lunge and Dr. Hurter, will be issued in a few days in Messrs. Whittaker's Specialists' Series. As the size of the page has been somewhat increased, the designation "Hand-book" has been substituted for "Pocket-book." The same publishers are about to issue "A Practical Hand-book on the Telephone," dealing specially with telephonic exchanges, by Mr. Joseph Poole.

MESSRS. RAITHBY, LAWRENCE, AND CO. have issued a second edition, revised and enlarged, of "Simple Recipes for Sick-room Cookery," by Mrs. Buck. The writer produces an excellent impression at once by the sensible tone of the preface, in which she gives some general counsels as to the proper way of dealing with the food of the sick.

THE new number of the Journal of the Royal Horticultural Society contains, besides extracts of proceedings, a number of interesting papers. Mr. W. Warren writes on Persian cyclamen; the Rev. W. Wilks on hardy cyclamen; Dr. M. T. Masters, F.R.S., on germination of cyclamen. Snowdrops form the subject of papers by Mr. J. Allen, Mr. D. Melville, and Mr. F. W. Burbidge. There are also papers on the cultivation of hardy bulbs and plants, by Herr Max Leichtlin; Lachenalias, by Mr. F. W. Moore; Cape bulbs, by Mr. J. O'Brien; and hybrid Rhododendrons, by Prof. Henslow.

THE volume containing the Proceedings and Transactions of the Royal Society of Canada for 1890 includes papers on the American bison, by Charles Mair; the Vinland of the Northmen, by Sir Daniel Wilson; unit measure of time, by Sandford Fleming; a peculiar form of metallic iron found in Huronian quartzite on the north shore of St. Joseph Island, Lake Huron, Ontario, by G. C. Hoffmann; sun-spots observed at McGill Observatory, by C. H. McLeod; a test of Ewing and MacGregor's method of measuring the electric resistance of electrolytes, by J. G. McGregor; the later physiographical geology of the Rocky Mountain region in Canada, by G. M. Dawson; fossil plants from the Similkameen Valley and other places in the southern interior of British Columbia, by Sir J. W. Dawson.

MESSRS. SWAN SONNENSCHEIN AND CO. will issue the following books during the autumn season:—"The Colours of Animals," by Prof. Beddard, with coloured and other plates and woodcuts; "Text-book of Embryology: Man and Mammals," by Dr. Oscar Hertwig, Professor of Comparative Anatomy in the University of Berlin, translated and edited from the third German edition (with the assistance of the author) by Dr. E. L. Mark, Professor of Anatomy in Harvard University, with 389 illustrations and 2 coloured plates; "Text-book of Embryology: Invertebrates," by Drs. Korschelt and Heider, of the University of Berlin, translated and edited by Dr. E. L. Mark, with several hundred illustrations; "Text-book of Animal Palæontology," by Dr. Thomas Roberts, designed as a supplement to Claus and Sedgwick's "Text book of Zoology," illustrated; "Text-book of Geology," adapted from the work of Dr. Emanuel Kayser, Professor in the University of Marburg, by Philip Lake, of St. John's College, Cambridge, with illustrations; "Text-book of Zoology," by Dr. C. Claus, of the University of Vienna, and Adam Sedgwick, F.R.S., Vol. II. "Mollusca to Man," third edition; "The Geographical Distribution of Disease in England and Wales," by Alfred Haviland, M.D., with several coloured maps; "Introductory Science Text-books"—Additions: Introductions to the study of "Physiography," by H. M. Hutchinson; "Zoology," by B. Lindsay; "Amphioxus," by Dr. B. Hatschek, of the University of Vienna, and James Tuckey; "Geology," by Dr. Edward Aveling; "Physiological Psychology," by Dr. Th. Ziehen, of the University of Jena, adapted by Dr. Otto Beyer, with 21 figures. "Young Collector Series"—Additions: "The Telescope," by J. W. Williams; "British Birds," by the Rev. H. C. Macpherson; "Flowering Plants," by James Britten; "Grasses," by W. Hutchinson; "Fishes," by the Rev. H. C. Macpherson; "Mammalia," by the Rev. H. C. Macpherson.

AN instrument for optical comparison of transparent liquids, named a *liquoscope*, has been recently devised by M. Sondén, of Stockholm. Two hollow prisms holding the liquids are separated by a partition at right angles to the refracting angle. The whole is placed in a vessel filled with glycerine, and which allows of vision in a horizontal direction through plane glass plates. The deflection of the light rays through the prisms is thus compensated. So long as the two liquids have the same optical action, one sees a distant mark (say a black paper strip on a window) as a straight connected line; but its halves are relatively displaced if the liquids have different refractive power. The amount of displacement gives a measure of the difference, the positive or negative nature of which also appears from the direction of displacement. The author recommends his apparatus for chemical purposes, especially comparison and testing of fats and oils, analysis of glycerine, &c., and detection of margarine in butter, margarine greatly lowering the index of refraction.

HERR HUFNER has lately pointed out some of the biological bearings of the fact (observed in experiment along with Herr Albrecht) that long light-waves are much more strongly absorbed by water than short ones. If the lower marine animals had, like man, the liveliest light perception with yellow rays, and a certain intensity of light were necessary to them, they must live at a less depth than if their visual organs were most strongly affected by short-waved rays. Thus, *e.g.*, if they needed as much yellow light as that of the full moon, they could not live deeper than 177 metres (say, 590 feet). Yet they are found at all depths where food, oxygen, and a suitable temperature exist. On the other hand, the existence of plants having chlorophyll depends on light, and we might expect that the distribution of non-parasitic plants would be very limited; which is the case, no plant-organisms being found under 200 fathoms. Green plants assimilate best in yellow light; and supposing plants to assimilate in moonlight they would find their limit at the above depth (177 metres). But while yellow is here weakened to 0·0000016 of its brightness, indigo blue has still 0·007829 of its original strength, and the assimilation with blue rays will be 660 times as strong as with yellow. Different coloured marine plants react differently according to the colour of light, and they have accordingly different distribution in depth.

THE additions to the Zoological Society's Gardens during the past week include two Pinche Monkeys (*Midas œdipus* ♂ ♀) from Granada, presented by Mr. A. Aitken; a Fallow Deer (*Dama vulgaris* ♂), British, presented by Mr. J. Johnston; a Persian Gazelle (*Gazella subgutturosa* ♀) from Persia, presented by Baron Ferdinand de Rothschild; a Common Cormorant (*Phalacrocorax carbo*), British, two Yellow-browed Buntings (*Emberiza chrysophrys*), two Red-backed Buntings (*Emberiza rutila*), a —— Bunting (*Emberiza cioides*), two Japanese Greenfinches (*Fringilla kawarahibi*, var.) from Japan, purchased; a Yellow-footed Rock Kangaroo (*Petrogale xanthopus* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE LINEAR ARRANGEMENT OF STARS.—Although the arrangement of stars in curves has often been noted and studied, little attention has been paid to what is apparently a more striking and prevalent feature, viz. straight lines and parallel arrangement of pairs, lines, and bands of stars, and of irresolvable wisps. Our knowledge of the structure of the sidereal universe is therefore extended in the required direction by some results obtained by Mr. T. W. Backhouse from observations which he has made during the last nine years in Sunderland. The area of the sky selected for scrutiny is that portion of the Milky Way included between 15, 13, 8 Monocerotis, α Orionis, ζ Tauri, and 5, μ, ξ Geminorum; and the configurations in this portion have been examined chiefly with a binocular field-glass of 2·05 inches aperture. The observations have been divided into sections, referring respectively to lines and parallel arrangements of stars, to those in clusters, to nebulous wisps, to nebulæ, and to miscellaneous lines. In these are given the detailed structure in different parts of the area showing various systems of parallel lines and wisps, together with their position-angles referred to that portion of Gould's galactic equator which runs through the middle of the area in question. The parallel arrangement of the stars, and an arrangement in straight lines, is strikingly obvious from the maps which illustrate the tabulated results of the observations. Besides the maps, sixteen figures have been drawn to show the various angles of position of the lines and streams with reference to the central line or axis of the Milky Way. From these figures it is apparent that the angles of position are grouped more numerously in certain directions than in others, the principal directions being nearly parallel to the galactic equator. Also, there is a great deficiency of position-angles at right angles to this equator. A wonderful case of radiation of stars and wisps in a fan-shaped group has been found, 68 Orionis being approximately the centre. One conclusion derived from the investigation is, that the stars and wisps in parallel lines are probably in the same region of space; and therefore that the majority of the stars in extensive tracts of the area examined are really near one another.

WOLF'S PERIODIC COMET.—This object can now be fairly seen by means of a small telescope. It will pass through the Hyades about September 25, and be 3° south of Aldebaran on October 2. The following ephemeris, from one given by Herr

Thraen in *Astronomische Nachrichten*, No. 3054, shows that the comet crosses the equator near the end of October :—

Ephemeris for Berlin Midnight.

1891.		Right Ascension.				Declination.			Brightness.
		h.	m.	s.		°	′	″	
Sept.	19	4	9	50·40		19	5	59·0	9·1
,,	21		13	7·99		18	17	29·3	
,,	23		16	16·09		17	27	4·4	
,,	25		19	14·09		16	34	48·7	
,,	27		22	1·58		15	40	44·3	
,,	29		24	38·50		14	44	57·2	
Oct.	1		27	4·25		13	47	32·4	11·2
,,	3		29	19·10		12	48	36·5	
,,	5		31	22·56		11	48	16·6	
,,	7		33	14·86		10	46	39·8	
,,	9		34	55·42		9	43	57·5	
,,	11		36	24·69		8	40	16·9	
,,	13		37	42·51		7	35	49·0	12·0
,,	15		38	49·17		6	30	45·1	
,,	17		39	44·06		5	25	18·0	
,,	19		40	27·92		4	19	38·1	12·1
,,	21		41	0·53		3	13	58·7	
,,	23		41	22·25		2	8	33·2	
,,	25		41	33·30	+	1	3	35·1	12·0
,,	27		41	33·97	−	0	0	47·0	
,,	29		41	24·46		1	3	57·7	
,,	31		41	5·38		2	6	8·0	
Nov.	2		40	37·33		3	6	51·3	
,,	4		40	0·67		4	5	54·6	
,,	6		39	16·50		5	3	7·0	11·2
,,	8		38	25·07		5	58	14·5	
,,	10		37	27·44		6	51	6·6	
,,	12		36	24·07		7	41	33·4	10·4

It will be seen that the comet is now nine times brighter than at the date of discovery (May 4). The maximum brightness will be reached about October 19.

GEOLOGY AT THE BRITISH ASSOCIATION.

THE Address of the President of the Geological Section having been devoted to the general questions involved in the origin, association, and working of coal, it was natural that other papers on the economic side of the science should claim considerable interest. Prof. Boyd Dawkins stated that the Channel Tunnel boring had been carried to a depth of 1500 feet, with the result of penetrating coal-measures dipping gently to the south at 1113 feet. Six seams, containing 10 feet of workable coal, had been pierced between that depth and the present bottom of the boring. The author endeavoured to show the probability that a thick series of coal-measures, with workable coals like those of Liége on one side and Somerset on the other, would be met with if the boring were continued, and pointed out the advantage possessed by the south-eastern coal-field in its moderate depth and the comparatively uncrushed character of the coal.

In an exhaustive paper Mr. Topley summarized the chief facts bearing on the origin of petroleum. He pointed out that, while the American oil was mainly derived from Palæozoic rocks, that in Europe and Asia came largely from Secondary beds, and the large Caucasian supply was drawn from rocks of Miocene age. The essential conditions for the supply of oil appeared to be, a porous rock, generally of sandstone or limestone, which served as a reservoir and was underlain by or contained beds largely consisting of organic remains, with an impervious cover of shale. In many cases the limestone had been dolomitized and transformed into a cavernous rock which was capable of storing the gas and oil. Such rocks can contain from one-eighth to one-tenth of their bulk of oil. The oil was driven to the surface by artesian pressure, and so gas was generally met with on the summits of anticlines and oil on their flanks. Where the rocks were very highly disturbed oil occurred, but not in very great abundance, while gas was rarely found.

Mr. Ross, in a paper on the same subject, endeavoured to prove that the oil was mainly generated by the action of solfataric volcanic energy upon beds of limestone, basing his conclusion on the occurrence of hydrocarbon and sulphurous vapours in solfataras, and the constant association of rock salt, dolomite, and gypsum with the rocks yielding petroleum. He exhibited equations to show that the action of sulphur dioxide and sulphuretted hydrogen on carbonate of lime, with or without water and peroxide of hydrogen, was capable of producing the ethylene and marsh gas derivatives, and he quoted experiments of Bischof to show that sulphur was formed by similar reactions, arguing that the hydrocarbons must be necessary by-products.

Sir Archibald Geikie communicated two most important papers on the results of Geological Survey work in the North-western Higlands. One of these papers, relating to the discovery of the *Olenellus* zone in the North-west Highlands, was as follows :—"Ever since the Geological Survey began the detailed investigation of the structure of the North-west Highlands of Scotland, the attention of its officers has been continuously given to the detection of any fossil evidence that would more clearly fix the geological horizons of the various sedimentary formations which overlie the Lewisian gneiss. A large collection of organic remains has been made from the Durness limestone, but it has not yet yielded materials for a satisfactory stratigraphical correlation. The study of this collection, however, has confirmed and extended Salter's original sagacious inference that the fauna of the Durness limestone shows a marked North American facies, though, according to our present terminology, we place this fauna in the Cambrian rather than in the Silurian system. Below the Durness limestone lies the dolomitic and calcareous shaly group known as the 'Fucoid beds,' which, though crowded with worm-castings, has hitherto proved singularly devoid of other recognizable organic remains. In following this group southwards through the Dundonnell Forest, in the west of Rosshire, my colleague, Mr. John Horne, found that, a few feet below where its upper limit is marked by the persistent band of 'Serpulite grit,' it includes a zone of blue or almost black shales. During a recent visit to him on his ground, when he pointed out to me this remarkable zone, I was struck with the singularly unaltered character of these shales, and agreed with him that if fossils were to be looked for anywhere among these ancient rocks, they should be found here, and that the fossil-collector, Mr. Arthur Macconochie, should be directed to search the locality with great care. The following week this exhaustive search was undertaken, and Mr. Macconochie was soon rewarded by the discovery of a number of fragmentary fossils, among which Mr. B. N. Peach, who was also stationed in the district, recognized what appeared to him to be undoubtedly portions of *Olenellus*. The importance of this discovery being obvious, the search was prosecuted vigorously, until the fossiliferous band could not be followed further without quarrying operations, which in that remote and sparsely inhabited region could not be at that time undertaken. The specimens were at once forwarded to me, and were placed in the hands of Messrs. Sharman and Newton, Palæontologists of the Geological Survey, who confirmed the reference to *Olenellus*. More recently Mr. Peach and Mr. Horne, in a renewed examination of the ground, have found, in another thin seam of black shale interleaved in the 'Serpulite grit,' additional pieces of *Olenellus*, including a fine head-shield with eyes complete. There may be more than one species of this trilobite in these Rosshire shales. The specific determinations and descriptions will shortly be given by Mr. Peach. The detection of *Olenellus* among the rocks of the North-west Highlands, and its association with the abundant *Salterella* of the 'Serpulite grit,' afford valuable materials for comparison with the oldest Palæozoic rocks of other regions, particularly of North America. The 'Fucoid beds' and 'Serpulite grit,' which intervene between the quartzite below and the Durness limestone above, are now demonstrated to belong to the lowest part of the Cambrian system. The quartzites are shown to form the arenaceous base of that system, while the Durness limestones may be Middle or Upper Cambrian. On the other hand, the Torridon sandstone, which Murchison placed in the Cambrian series, can now be proved to be of still higher antiquity. The marked unconformability which intervenes between it and the overlying quartzite points to a long interval having elapsed between the deposition of the two discordant formations. The Torridon sandstone must therefore be pre-Cambrian. Among the 8000 or 10,000 feet of strata in this group of sandstones and conglomerates, there occur, especially towards the base and the top, bands of grey and dark shales, so little altered that they may be confidently expected somewhere to yield recognizable fossils. Already my colleagues have detected traces of annelids and some more obscure remains of other organisms in these strata. These, the oldest relics of life yet known, have excited a vivid desire in the Geological Survey to discover further and more determinable fossils associated with them in the same primæval resting-place. We shall spare no pains to bring to light all that can be recovered in the North-west Highlands of a pre-Cambrian fauna."

In the other paper the Director-General dealt with some recent work of the Geological Survey in the Archæan gneiss of the North-west Highlands. "For some years past," he remarked, "the officers of the Geological Survey have spent much time and labour upon the investigation of the old or fundamental gneiss of the North-west Highlands. They have succeeded in showing that it consists mainly of materials which were originally of the nature of eruptive igneous rocks, but which by a long succession of processes have acquired the complicated structures which they now present. No evidence of anything but such eruptive rocks had been met with until the mapping was carried into the west of Rosshire. In that area it had long been known that the gneiss includes some mica-schists and limestones which were generally believed to be integral parts of its mass. With the accumulated experience of their work farther north, my colleagues were naturally pre-disposed to accept this view, and to look on even the limestones as the result of some crushing down and re-formation of basic igneous rocks containing lime-silicates. But as they proceeded in their work they encountered various difficulties in the acceptation of such a theoretical explanation. In particular, they found that with the mica-schist were associated quartz-schists and graphitic schists, and that the limestone occurred in thick and persistent bands with included minerals like those found in the Eastern Highlands in districts of contact-metamorphism. The microscopic examination of some of these rocks showed them to present close affinities to certain members of the crystalline series of the Eastern and Central Highlands, which can be recognized as consisting mainly of altered sedimentary strata (Dalradian series). Yet the officers of the Survey could not separate these doubtful rocks from the surrounding gneiss. The several materials seemed to pass insensibly into each other in numerous sections, which were examined with great care. Within the present month, however, one of the members of the staff, Mr. C. T. Clough, who has been specially engaged in this investigation, has obtained what may prove to be conclusive evidence on the subject. He has ascertained that the main bands of graphitic schist occur evenly bedded in an acid mica-schist, in which, also, thin graphitic layers are distributed at intervals of an inch or less. These rocks are sharply marked off from the true gneiss, though, where they actually join, they appear to be, as it were, crushed along a line of intense movement. Mr. Clough and his colleagues are at present disposed to believe that these schists are really an older series of sediments, into which the original igneous rocks now forming the gneiss were erupted. If they succeed in demonstrating the correctness of this inference, they will have established a fact of the highest interest in regard to the geological history of our oldest rocks. Already they have shown the thick masses of Torridon sandstone to be an accumulation of sedimentary materials of pre-Cambrian age. They will push back the geological record to a still more remote past, if they can establish the existence of a yet more ancient group of sedimentary strata among which layers of graphite and beds of limestone remain to suggest the former existence of plant and animal life."

The session on Monday was opened by Sir R. S. Ball with a paper on the cause of an Ice age. This communication stated that the author had a work in the press dealing with the question of glacial climates. He had revised Herschel's figures, on which Croll's deductions were based, and discovered an arithmetical error of considerable consequence. If 63 represents the number of heat-units received by any hemisphere during summer, its winter receipt will be represented by 37. Consequently, during a period of high eccentricity the 63 units of heat may be received in 199 days or in 166 days, according to the position of the equinoxes, producing either a long and cool summer or a short and intensely hot one. The paper did not deal with geographical considerations, and advocated the occurrence of clusters of alternate glacial and interglacial periods at each phase of high eccentricity in the earth's orbit. This paper excited considerable discussion, in which Prof. Sollas, Prof. Wright (of Oberlin, Ohio), Mr. Hall, Dr. Crosskey, Dr. Hicks, and many other glacialists took part.

Dr. Crosskey followed with his Report on the Distribution of Erratics in England and Wales, in which he referred to the useful work done by the North of England Boulder Committee, in systematically surveying the north in search of boulders and groups of boulders. Details were given of boulders from Lancashire, Cheshire, Derbyshire, Staffordshire, and Yorkshire, and it was remarked that boulders were being destroyed so rapidly that many described in former reports had totally disappeared. In another paper the same author controverted a statement of Forbes with regard to the glaciation of the Dovrefjeld. Wherever the basement rock is to be seen, it is glaciated, although morainic deposits were swept away and redistributed by torrential action at the close of the Glacial period.

Prof. Wright read a most interesting paper on the Ice age of North America and its connection with the appearance of man in that continent. The glacial deposits, transported from several centres mostly outside the Arctic circle, and the absence of a Polar ice-cap, militated against an astronomical, and for a geographical, cause of the great cold, particularly as an uplift of the glaciated area was coincident with an important subsidence in Central America. The author regarded the so-called "terminal moraine of the second period" as a moraine of retreat due to the first glaciation, and thought the evidence of forest beds, mainly to the south of the area, indicated local recessions of ice, and not a single great interglacial epoch. Palæolithic remains similar to those of the Somme and Thames have been found in several gravel terraces flanking streams which drain from the glaciated region, and made up of glacier-borne detritus; they are regarded by the author as deposits of the floods which characterized the closing portions of the Glacial period. The recession of the falls of Niagara and St. Anthony gives an antiquity of not more than 10,000 years to the end of the Glacial epoch—a conclusion supported by the enlargement of post-Glacial valleys and the silting up of small post-Glacial lakes.

Other papers read on this day were: one by Dr. Hicks, who produced specimens of boulders from Pembrokeshire, which seemed to him like North Welsh or Irish rocks—his picrite was, however, recognized as an Irish rock by geologists in the room, and in any case a flow of ice down the Irish Sea and over Pembrokeshire seemed to be clearly proved; one by Mr. Kendall, on a glacial section at Levenshulme, Manchester, in which he gave evidence from the striation of the subjacent rock, and the intrusion of tongues of boulder-clay into it, the transport of fragments, the orientation of large boulders, and the direction of striæ, together with a consideration of the levels of the different portions of the rock beneath, that the district had been traversed by land ice coming from a direction a few degrees north of west; and one by Mr. Bolton on a group of boulders from Darley Dale, near Matlock, which he regarded as having been washed out of rocks skirting the valley. In connection with these papers may be mentioned a report by Mr. Harrison, who has excavated in the talus under some rock-shelters at Oldbury Hill, near Ightham, from which he obtained forty-nine well-finished Palæolithic implements and over 600 waste flakes, which were described in a separate paper by Prof. Prestwich. Prof. Wright gave also a brief account of the basaltic lava beds of the Pacific coast, which are of post-Tertiary age. New evidence in favour of the genuineness of the Calaveras skull and other human remains found under the lava beds was given; and the discovery of a small clay image in a similar position under the western edge of the lava plains of Idaho at Nampa was recorded; the lava beds are correlated with the glacial deposits of the East.

Mr. Jones's report on the Elbolton cave, near Skipton, was of unusual interest. Long-headed human skulls were found with burnt bones and charcoal in the upper stratum, associated with domestic animals and pottery ornamented with diamond and herring-bone patterns; while at a much lower level—13–15 feet below the floor—there were round skulls, much more decayed, in connection with ruder and thicker pottery than has been found in any other part of the cave. No flints or metal of any kind have been found, and bone pins and other worked bones are the only human implements hitherto discovered. The remains of bear and hare have been found in cave earth below this level, and the investigation is to be continued in the hope that remains similar to those of the Ray Gill fissure may yet be met with.

An interesting discussion was raised on the paper by Dr. Hicks on the Silurian and Devonian rocks of Pembrokeshire and Devon. The Silurian rests transgressively on Ordovician and pre-Cambrian rocks in Pembrokeshire, but is covered by a continuous series up into the Old Red Sandstone and Carboniferous; similarly the Morte Slates, which the author regards as the oldest rocks of North Devon, and in which he has recently found *Lingulella Davisii*, are covered by the Devonian and Culm series of rocks. Mr. Ussher described the occurrence of a volcanic series in the Lower Devonian rocks of Tor Cross, and traced similar diabasic rocks amongst the chloritic series of Prawle Point, the excessive alteration of these rocks being due to the greater nearness to the old resisting rocks of the Channel. In this conclusion he was supported by Mr. Hunt, who described the occurrence of detrital tourmaline in the Devonian cliffs at the north-east end of Straiton Sands, and compared it with the occurrence of similar material in a quartz-schist west of the Start Lighthouse. Both schists and sandstone contain detrital tourmaline, mica, fine-grained quartz, and iron.

Several palæontological papers were contributed. Mr. Montagu Browne exhibited teeth, scales, and bones of Colobodus from Aust, Watchet, and Leicestershire, which seemed to indicate the identity of Colobodus with Lepidotus, and possibly of Heterolepidotus with Eugnathus, and to give Colobodus an extended upward range. Mr. Buckman gave an account of the Ammonite zones in the Inferior Oolite. There is a marked break on the Continent between the *Murchisonæ* and the *Sowerbyi* zones, which appears to be filled up by the zone of *Lioceras concavum* in England. The *Sowerbyi* zone, however, is absent in England from all localities except Dundry, and Coombe near Sherborne; and the author therefore sought and obtained a grant to open an old quarry at the latter locality, in order to fully investigate the fauna of the *Sowerbyi* zone, and its relationship to the *concavum* and *Sauzei* zones. Mr. Storrie, of the Cardiff Museum, exhibited a fine series of slides and drawings of *Pachytheca* and *Nematophycus*, and gave a minute description of them; this elicited some discussion, in the course of which Mr. Murray suggested that the former might possibly be the egg of a Crustacean or some other small organic body completely incrusted by a Nullipore. Mr. Smith Woodward exhibited Pterodactyl and Plesiosaur bones from Brazil, and gave an account of a series of Miocene fish-remains from Sardinia. Other palæontological papers were one containing a record of the occurrence of a variety of *Estheria minuta* in the Lower Keuper building-stone of Chester, by Mr. De Rance, and one by Mr. Vine on the Bryozoa of the Upper Chalk. Mr. B. Thompson gave an exhaustive report of the transition bed between the Middle and Upper Lias in Northamptonshire, from which he had obtained a large and valuable series of fossils. Mr. Newton described the occurrence of *Ammonites jurensis* in the Northampton sands, near Northampton; and Prof. Hoyes Panton gave an account of a mastodon of very large size at Highgate, Ontario, and a mammoth from Shelburne, in the same province.

The occurrence of a strip of Lower Greensand four to five miles long between Shaftesbury and Child Okeford, and running parallel to the valley of the Stour, was described by Mr. Jukes Browne. The same author attempted to explain monoclinal flexure by the recurrence of movement in rocks already faulted, but covered subsequently by unconformable strata; movement along the faults of the older series, under the influence of new pressure, would throw the overlying series into monoclinal folds or faults. The existence of a large area of Kellaways rock, near Bedford, and the extension of Fuller's-earth works at Woburn were commented on by Mr. Cameron.

Several of the Committees appointed last year had done good work. The Photograph Committee had obtained over 250 new photographs of geological interest, many of which were exhibited in the Section-room or at one of the *soirées*, where also Prof. Wright displayed a fine series of transparencies illustrating the lava and glacial deposits of the United States, and Mr. Stirrup a set of slides of the dolomite district of Languedoc. The Earth-Tremor Committee had been testing a number of recording-instruments; Mr. Smith Woodward reported that the lists of type specimens were progressing, and that many large Museums were publishing their own lists of types; Mr. De Rance gave an account of a number of wells in Yorkshire, Lincolnshire, Notts, Cheshire, Shropshire, and Glamorganshire; and Mr. Johnston-Lavis sent a description of the Vesuvian eruption of 1890–91, the chief part of which has already appeared in the columns of NATURE.

BIOLOGY AT THE BRITISH ASSOCIATION.

THE papers read at this Section were fully as interesting, though not quite so numerous, as usual. A good deal of time on one day was occupied by a discussion upon animals and plants; but as several of those who took part in the discussion did not wish their remarks to be reported, it has been thought better to leave out this part of the proceedings of Section D. Botanical papers preponderated over zoological, but it was not found necessary to divide the Section into two sub-Sections.

Mr. Grenfell read a paper upon the structure of Diatoms, describing pseudopodia in these organisms. The pseudopodia are quite easy to see in such a form as *Melosira* with even a comparatively low power. They are very long and stiff, radiating outwards from the periphery, and are apparently non-retractile (they were watched for an hour without any movements being observed); the pseudopodia are sometimes nine times the length of the diameter of the Diatom, and are occasionally branched; adjacent Diatoms were sometimes seen to be connected by a fusion of their pseudopodia. It was suggested that the use of the pseudopodia is to keep the plants floating, and to act as a protective *cheveux de frise* against their enemies. These Diatoms were compared to Heliozoa, with which they have evidently not a little resemblance in the form of the pseudopodia. Incidentally Mr. Grenfell stated that he had found a coating of cellulose upon the green corpuscles of *Archerina*, which were regarded by Lankester as chlorophyll bodies, and not as symbiotic algæ.

Mr. Wager described the presence of nuclei in Bacteria; they were met with in a species of *Bacillus* found in water containing decaying *Spirogyra*.

Dr. Gilson read a paper upon the nephridia of the leech, *Nephelis*. The ciliated funnels appear to lose their connection with the rest of the nephridium, and to perform the function of organs for the propulsion of the blood along the channels in which they lie.

The Plymouth Zoological Station sent a record of work done during the last year by the Director and by Mr. Cunningham.

Mr. Calderwood read a paper upon some economical investigations which had been carried out. He stated that three investigations had been started within the present year, which it was hoped would prove of great value to the fishing population of this country. One was an attempt to produce an artificial bait for use in long line fishing. This investigation was being carried on by a competent chemist, and a considerable advance had already been made towards a satisfactory solution of this difficult problem. Inquiries were also being conducted with regard to the occurrence of anchovies on the south-west coast of England, and Mr. Cunningham, the Naturalist of the Association, had carried out some inquiries at fishing stations on the south coast. At present no net small enough in the mesh to capture anchovies was employed, but that fish appeared so often when the ordinary pilchard nets became entangled, as to suggest that they might be present in considerable quantities. Anchovy nets had, therefore, been constructed, and would be used during the pilchard season this autumn. An investigation was also being carried on into the condition of the North Sea fisheries, which were declared to be rapidly declining. It was proposed to draw up a history of the North Sea trawling grounds, comparing their present condition with their condition some twenty or thirty years ago, when comparatively few boats were at work; to continue, verify, and extend observations as to the average sizes at which prime fish, such as soles, turbot, and brill, become sexually mature, and to collect statistics as to the sizes of all fish captured in the vicinity of the Dogger Bank and the region lying to the eastward, so that the number of immature fish annually captured may be estimated. Also to make experiments with beam trawl nets of various meshes with a view to determine the relation, if any, between the size of mesh and the size of fish taken. Mr. Calderwood added that a regular survey of the English Channel had been commenced, not only in the deep water, but in various estuaries. A meteorological station of the second order had been recently established, where observations at 9 a.m. and 9 p.m. would be taken daily by wet and dry bulb thermometers, barometers, rain-gauges, and sunshine-recorders.

Mr. J. T. Cunningham read a paper upon the reproduction of the pilchard. The ovum of this fish, described as such in the Journal of the Association for 1889, was stated by Pouchet not to belong to the pilchard; Pouchet believed that the pilchard's ovum is not pelagic. The identification of the ovum was shown to be correct by further observations carried out in the Laboratory with the ova obtained from the mature fish. Similar results have been obtained by Marion, of Marseilles.

Another paper, by the same, dealt with the growth of food-fishes, and their distribution at different ages.

(1) *Rate of Growth and Age of Sexual Maturity.*—Numerous specimens of the flounder (*Pl. flesus*) were reared from the larval state in the aquarium of the Plymouth Laboratory. Measured in April, when a year old, they varied from 4 to 19 cm. (about 1½ to 7½ inches). Specimens obtained in the Cattewater, and known to be not less than a year old, are from 12 to 19 cm. in length. None of these captive flounders, nor any taken in the Cattewater, were sexually mature, but, according to Dr. Fulton, of the Scottish Fishery Board, sexually mature flounders have been observed which were only 7 inches long. It was concluded, therefore, that (*a*) the rate of growth varies greatly for different individuals, but its maximum for the first year is 19 cm., or 7½ inches; (*b*) sexual maturity is not reached till the end of the second year, although the minimum size of sexually mature individuals may be slightly exceeded by some specimens in one year's growth.

Similar results were obtained for the plaice (*Pl. platessa*) and the dab (*Pl. limanda*).

(2) *Distribution.*—The young of the above-mentioned species in their first year, and of certain round fish, especially *Gadus luscus* and *G. minutus*, occur in shallow water, within the 10-fathom line. But there has hitherto been considerable difficulty in obtaining young specimens of other more valuable species in order to study their rate of growth. These species—namely, the sole, turbot, brill, lemon sole, megrim (*Arnoglossus megastoma*), do not pass the first year of their lives in shallow water. Young soles in the larval state occur in tidal pools at Mevagissey, and young turbot and brill 2 to 3 cm. in length are commonly found from June to August in Plymouth Sound and Sutton Pool, swimming at the surface in a semi-metamorphosed stage. Soles a little over 16 cm. in length are frequently taken in Plymouth Sound in summer; these are just over one year old, and are not sexually mature. Turbot 23 to 34 cm. long may be taken in 5 to 7 fathoms; these also are over one year old and not sexually mature. But the young stages between 3 months and 12 months old have not been taken in shallow water, and apparently live at depths greater than 10 fathoms. It seems that our commoner and more valuable food-fishes do not attain to sexual maturity till the end of their second year, that their size at this age is subject to great individual variation, and that the young in the first year of growth have a characteristic distribution. Investigation of the deeper water from this point of view is now being carried on at Plymouth.

The distribution of *Crystallogobius Nilssoni* was recorded by the same author. It had been found by Collett in the Christiania Fjord and in other parts of Norway; also at Bohuslan, in Sweden. Mr. Cunningham dredged 100 specimens at a single haul close to the Eddystone, in 27 fathoms of water. Day mentions only one specimen found in British waters—one taken by Thomas Edwards in a rock pool at Banff. Mr. Holt subsequently dredged a number in 30 fathoms in Ballinskelligs Bay. The species is probably fairly abundant between 20 and 30 fathoms on smooth sandy ground all along the British and Irish coasts.

Mr. Cunningham also read a paper upon the larvæ of the sea crayfish (*Palinurus vulgaris*), describing most of the stages, and particularly remarking upon the presence of the first maxillipede in the newly hatched larva, which had been stated by Richter to be absent.

Prof. Herdman and Mr. J. A. Clubb communicated a paper upon the innervation of the epipodial processes of some Nudibranchiate Mollusca. The cerata of the Nudibranchs were regarded by Prof. Herdman as being probably epipodial outgrowths.

The question has, however, been raised lately by Pelseneer and others as to whether the so-called epipodia of Mollusca are all homologous structures, and one of the subjects of controversy now is the origin of the nerve supply in various forms, it being supposed that where the processes are innervated from the pleural ganglia they are pallial in their nature, and where supplied from the pedal ganglia they are to be regarded as outgrowths from the foot.

Consequently it seemed of importance to determine afresh the origin of the nerves supplying the cerata in several different types of Nudibranchiata, especially as the results of former investigations, depending entirely, we believe, upon minute dissection, are puzzling, and to some extent contradictory. We have traced the nerves from the ganglia, by means of serial sections, in representatives of the genera *Polycera*, *Ancula*, *Tritonia*, *Dendronotus*, and *Eolis*, with the following results:—

In *Polycera quadrilineata* the cerebral and pleural ganglia are completely fused to form a cerebro-pleural mass. The "epipodial" nerves are found arising from the ventral and posterior part of this mass (*i.e.* distinctly from the pleural ganglia), and they run along the sides of the back to supply the ceratal ridges.

In *Ancula cristata* the pleural ganglia are fairly distinct from the cerebral. In a specimen cut into about 500 sections we find in the 100th section or so from the anterior end six distinct ganglia (the cerebral, pleural, and pedal pairs) surrounding the œsophagus. A few sections further back, the cerebrals disappear, and then the epipodial nerves are found arising from the dorsal edge of the pleural ganglia. The nerves soon turn posteriorly, and then give off their first branches dorsally. These branches enter the mesoderm of the body wall, and can then be traced back through over a hundred sections to the first pair of cerata, which they enter. The main nerve passes back to the remaining cerata.

In *Tritonia* and *Dendronotus* also the epipodial nerves arise from the pleural ganglia; but in *Eolis* (or *Facelina*) *coronata* we find that the main nerves to the cerata arise distinctly from the pedal ganglia. We have also traced in the same series of sections the ordinary pedal nerves to the foot proper; so there can be no question as to the nature of the ganglia from which the nerves arise. The epipodial nerves spring from about the middle of the pedal ganglion, rather on the dorsal surface, and, after a short course, pass through the muscular layer of the body wall and are distributed to the clumps of cerata.

But, in addition to these main epipodial nerves in *Eolis*, we find also a nerve arising from the compound ganglionic mass, immediately ventral to the eye (probably, therefore, from the pleural element), which goes to the front cerata. This pleural nerve has its origin distinctly anterior to the origin of the main epipodial nerves from the pedal ganglia.

We arrive, then, at the curious result that the innervation of the ceratal processes is not the same in all these Nudibranchs. In *Polycera*, *Ancula*, *Tritonia*, and *Dendronotus*, the epipodial nerves arise from pleural ganglia, or from the ventral and posterior parts of cerebro-pleural masses; while in *Eolis* the chief epipodial nerves are from the pedal ganglia, but there are also smaller nerves from the pleurals. In the ordinary Rhipidoglossate Gastropod, such as *Trochus*, the epipodial ridges and processes are supplied, according to Pelseneer, by nerves arising from the dorsal part of the elongated pedal ganglia. So, judging from the nerve supply alone, it might be said that the cerata of *Eolis* are pedal in their nature, and homologous with the epipodial processes of *Trochus*, while those of *Ancula* and the rest are totally distinct structures of pallial origin. But these dorso-lateral processes in the various Nudibranchs are so much alike in their relations, and are connected by such series of gradations, that it is difficult to believe that they are not all homologous; and the presence of the accessory epipodial nerve in *Eolis* arising from the pleural ganglion suggests the possibility of another explanation, viz. that these outgrowths, starting at first as pedal structures innervated by nerves from the pedal ganglia, may have acquired, possibly as the result of having moved further up the sides of the body, a supplementary nerve supply from the adjacent integumentary nerves arising from the pleural ganglia, and this supplementary supply, while remaining subordinary in *Eolis*, may in the other types have gradually come to supplant the original epipodial nerves, which are now no longer found in such forms as *Polycera* and *Ancula*. This is at present only a suggestion, which may be disproved or supported by the examination of the nerves of a number of additional Nudibranchs.

Prof. W. N. Parker read a paper containing the results of some experiments on respiration in the tadpoles of the common frog. After referring to the great power of adaptation to external conditions seen amongst amphibious larvæ, the author described some experiments on frog tadpoles, which, although not yet complete, show as follows:—(1) Soon after the lungs become functional—*i.e.* in tadpoles measuring more than 2 cm.

in length—the gills are no longer sufficient for purposes of respiration, and the animals die in a very short time if prevented from coming to the surface to breathe. (2) If tadpoles are prevented from using their lungs from an earlier stage onwards, the gills remain perfectly functional, and development proceeds as usual. At metamorphosis, the fore-limbs are slow in becoming free, owing to the retention of the operculum, that on the same side as the spiracle appearing first. Eventually, a slit-like spiracle is present on either side. In respiration, the mouth is opened and closed, as in the tadpole. Specimens of branchiate frogs were exhibited, in which the tail had shrunk to less than half its original length.

Exhibition of, and remarks upon, some young specimens of *Echidna aculeata*, by Prof. W. N. Parker. The specimens are from the collection of the late Prof. W. K. Parker, who received them from Dr. E. P. Ramsay, Curator of the Australian Museum, Sydney. They are much curved towards the ventral side, the snout pointing backwards, and the tail, in the older of the two stages, forwards. The younger stage measures along the dorsal curve, from the end of the snout to the tip of the tail, 12 cm., the greatest diameter of the body being 3 cm.; the corresponding measurements of the older stage are respectively 21·5 cm. and 6 cm. In the latter, the body is covered with short scattered bristles. In both stages the snout is very similar in form to that of *Ornithorhynchus*, and is covered by a thick horny layer, but in other respects the specialization characteristic of *Echidna* is already apparent. The gape is narrow, and extends only a short distance down the snout, and the manus, even in the younger stage, is already much larger and stronger than the pes. The tail is short and conical. There is no caruncle, or "egg-breaker," in the snout, such as is seen in *Ornithorhynchus*. A few points in the structure of the fore-part of the head in the older stage were described. The mouth has the narrow and tubular form seen in the adult, and the long tongue has a horny tip. The glands in relation with the mouth and nose are very numerous. There is no trace of any teeth-rudiments, and in many other respects the structure of the head shows extreme specialization. Jacobson's organ is large, and highly developed. A well-marked "turbinal" is present in it.

Prof. Howes read a paper upon the classification of fishes by their reproductive organs. On comparison of the urino-genital organs of those Osteichthyes having a non-abbreviated kidney with the same organs of the higher Vertebrata and the Elasmobranchs, the female genital duct and the kidney are seen to be inversely proportionate in length. No feature more fully characterizes the development of the Müllerian duct than the accompanying abbreviation of the kidney and the disappearance of its head segment. The persistence of the last-named among the Osteichthyes, and its possible retention of the renal function in rare cases, taken in conjunction with the mode of development of the ovary duct in these fishes, point to the conclusion that the latter is in no way homologous with the Müllerian duct as ordinarily understood. Balfour's belief that the genital ducts are homologous in both sexes of the Teleosteans, is supported by the facts of anatomy; and comparison of the reproductive system of the Ganoids with that of the Teleosteans shows the two to be modifications of the same common type; and the absolute structural community of the parts in the males and females of the Sturiones, while further confirming Balfour's doctrine, is opposed to Jungersen's implication that the subtle differences in the mode of development of the ducts in the opposite sexes of the Teleostei, are indicative of their non-homology. The facts above alluded to justify us in regarding the genital ducts of the Osteichthyes, not only as homologous in the two sexes, and primarily independent of the genital glands, but as distinct structures *sui generis*, probably unrepresented in all other Vertebrates. The Plagiostomi and Holocephali, in which vasa efferentia are present and the kidney becomes an accessory to reproduction in the male, may be grouped together into a *Nephrorchidic Series*, as distinguished from an *Enthorchidic Series*, embracing the Ganoids and Teleosteans. Comparison of the pori genitales in relation to the coalesced ureters of the Marsipobranchii with the corresponding parts of the females of those Teleostei destitute of genital ducts, especially in consideration of the facts concerning the development of the parts recorded by Scott, Liszt, and others, supports Rathke's conclusion that the ancestors of the former fishes must have possessed genital ducts. The Osteichthyes, although specialized in respect to many features of their organization, have, together with the Marsipobranchs, retained the least modified type of urinogenital organs known for living Vertebrates. W. N. Parker's recent and important discovery that, while in *Protopterus* a Müllerian duct is present, vasa efferentia are absent, and the testicular products are discharged through a duct more nearly comparable to that of the bony fishes than to the genital ducts of any other Vertebrates, suggests that the development of vasa efferentia and the assumption of a genital function by the Wolffian duct may have been effected subsequently to the formation of the Müllerian oviduct. And further comparison of the Dipnoi with the Elasmobranchii suggests that the former may have struck off from the Holocephalic branch of the latter before the differentiation of the ancestors of its living members.

Another paper by Prof. Howes dealt with the customary methods of describing the gills of fishes. The gills of Plagiostomes and Marsipobranchs are not unfrequently enumerated in relation to the opposite walls of the visceral sacs which give origin to them, while those of the higher fishes are enumerated in relation to the opposite faces of the septa which bear them. The confusion arising out of this is well known to teachers, and is, in itself, sufficient to justify the introduction of a revised nomenclature for the parts concerned. The facts of development show: (1) [on the assumption that the mandibular or mouth cavity is serially homologous with a pair of post-oral visceral clefts] that each gill lies in front of its corresponding skeletal arch; (2) that the saccular type of gill met with in the Marsipobranchs and Plagiostomes is that from which the pectinate one of the higher gnathostomatous fishes has been derived; and (3) that a mandibular gill has no existence in living fishes. Gills of the Marsipobranch-Plagiostome type may be conveniently described for general anatomical purposes, as *Cystobranchiæ*, and those of the higher Telosteoid type, as *Pectinobranchiæ;* while the parts of the individual gills themselves should be in all cases enumerated in relation to the visceral pouches from which they arise. Thus, the spiracular gill of Elasmobranchs (often termed the mandibular pseudobranch) should be described as the hyoid hemibranch, and the opercular gill of the higher fishes (often termed the hyoid pseudobranch) as the first branchial hemibranch. The well-known series of buccal filaments met with in certain Chelonia appear to have the fundamental relationships of gill-folios, and, in view of the discovery of Dohrn and others that the buccal sac would appear, from its mode of development in the Teleostei, to be the morphological equivalent of a pair of gill pouches, the possibility that these filaments may (at any rate for the most part) represent mandibular gills of a reversional character must not be overlooked.

Dr. Arthur Robinson communicated some facts relative to the development of the rat and the mouse. The most important part of the paper dealt with the relation of the yolk sac to the maternal tissues. The crypt in the uterine wall which lodges the ovum becomes shut off from the rest of the cavity of the uterus by a fusion between the distal proximal walls of the uterus. The greater part of the space so formed is occupied by the ovum; the remaining portions are converted into maternal blood sinuses; the blood in these sinuses bathes the trophoblast and the distal end of the yolk sac. Later, the distal part of the yolk cavity is obliterated by the apposition of its walls, but the proximal portion remains; diverticula grow out from from this into the placenta, which maintain the intimate relation of the yolk sac to the maternal blood. It seems probable, in view of these facts, that the yolk sac plays an important part in the nutrition of the fœtus. The allantois is a solid mass of mesoblast containing no diverticulum from the alimentary tract, and does not become attached to the trophoblast until comparatively late in the life of the embryo, *i.e.* the eleventh day.

Another paper by the same was entitled "Observations upon the Development of the Spinal Cord in *Mus musculus* and *Mus decumanus:* the Formation of the Septa and the Fissures." The anterior and posterior septa of the cord were stated to be formed by the spongioblasts of the cord itself, and not by ingrowths of the enveloping sheath of pia mater.

Prof. Marcus Hartog communicated an outline classification of sexual and allied modes of protoplasmic rejuvenescence.

I. The following modes of rejuvenescence occur in cellular and in certain apocytial organisms:—

A. PLASTOGAMY: the fusion of cytoplasta into a plasmodium, the nuclei remaining free.

B. KARYOGAMY: the union of cells (gametes), cytoplast to cytoplast and nucleus to nucleus, to form a 1-nucleate cell, the zygote. The following variations occur:—

1. ISOGAMY. The union of gametes undistinguishable in size, form, and behaviour; this may vary as follows:—
    - (*a*) MULTIPLE: between several gametes (up to 6).
    - (*b*) BINARY: between a pair of gametes;

    or, from another point of view—
    - (*c*) INDIFFERENT: between any gametes of the species.
    - (*d*) EXOGAMOUS: between gametes of distinct broods only.
    - (*e*) ENDOGAMOUS: between gametes of the same brood only.
2. ANISOGAMY: the union of two gametes differing chiefly in size; the smaller (*micro-*) gamete is *male*, the larger (*mega-*) gamete, *female*.
3. HYPERANISOGAMY: the female gamete, at first active, comes to rest before fusion with the male.
4. OOGAMY: the female is never actively motile; the male is termed a *spermatozoon*, the female an *oosphere*.

From another point of view karyogamy is—

5. ZOOIDIOGAMOUS: one gamete at least is actively motile (flagellate, ciliate, or amœboid).
6. SIPHONOGAMOUS: karyogamy is effected by a tubular outgrowth from one or both of the gametes.

II. In apocytial fungi multinucleated masses of protoplasm (*gametoids*) may conjugate to form a *zygotoid*, by a siphonogamous process. The union may be *isogamous* or *anisogamous*.

III. Gametes may be classified as follows:—

A. According to their *formation*—

1. EUSCHIST: formed by repeated complete divisions from a parent cell, the gametogonium.
    - (*a*) EUTHYSCHIST: each nuclear division is accompanied by cell division.
    - (*b*) BRADYSCHIST: the nuclear divisions are completed before any cell division takes place.
    - (*c*) ISOSCHIST: the brood-cells of a gametogonium are all equal and functional.
    - (*d*) ANISOSCHIST: the brood-cells are unequal, some of them being reduced to aborted or degraded gametes.
2. HEMISCHIST: the divisions are limited to the nucleus, none occurring in the cytoplasm.
3. APOSCHIST: the cell divisions do not occur, but a cell directly assumes the behaviour of a gamete.
4. SYMPHYTIC: the gameto-nucleus is formed by the fusion of several nuclei.

B. According to their *behaviour*, as—

1. FACULTATIVE: retaining the power of development if karyogamy fails to occur.
2. OBLIGATORY: with no power of independent development.

IV. PARAGENESIS will include the following modes, usually grouped under the term parthenogenesis, apogamy (*pro parte*), &c.:—

A. TRUE PARTHENOGENESIS: the direct development of a facultative gamete without karyogamy. This may occur in the case of—

(1) Isogametes; (2) Anisogametes (male and female); (3) Oogametes.

B. SIMULATED PARTHENOGENESIS:—

1. CELLULAR: a cell assumes directly the behaviour of a zygote.
2. APOCYTIAL: a multinucleate mass of protoplasm assumes directly the behaviour of a zygotoid.

C. METAGAMETAL REJUVENESCENCE:—

1. UNICELLULAR: a single cell in the neighbourhood of the gamete assumes the form and behaviour of the zygote.
2. MULTICELLULAR: a mass of cells in the position where gametes should be produced, assumes the character of the young organism formed by the zygote.

D. PARAGAMY or ENDOKARYOGAMY: vegetative or gametal nuclei lying in a continuous mass of cytoplasm fuse to form a zygote nucleus.

1. Progamic paragamy: the fusing nuclei are the normal gametonuclei of the progamous cell (ovum which has formed 1-polar body).
2. Apocytial paragamy: the vegetative nuclei of an apocytium fuse to form a zygote nucleus.

The President of the Section read a paper by himself and Miss Dorothea Pertz, on the artificial production of rhythm in plants. The apparatus, devised by the Cambridge Scientific Instrument Company, was exhibited. The plant is subjected to a series of alternate and opposite influences from light or gravitation, as the case may be. The plant to be experimented with is fixed to a spindle, which, by a clockwork escapement, makes a sudden semi-revolution every half-hour. When the clockwork is stopped, the plant continues to curve with an acquired rhythm, as if the machinery were still in action. This is similar to certain natural rhythms—for instance, to the "sleep" of flowers, which for a short time continue to open and shut although kept constantly in the dark.

Prof. Green read a paper on the occurrence of diastase in pollen. The starch in the pollen grain serves as nutriment for the growing pollen tube, and the presence of the ferment converting it into sugar enables it to travel along the growing tube.

Prof. Vines, in a paper upon diastase in foliage leaves, controverts the opinion of Prof. Wortmann, who stated that diastase was either absent from the foliage leaves of plants, or present in such minute quantities that it could be of no physiological importance. It is this diastase, and not the protoplasm of the cells, which converts the starch accumulated in the leaves into sugar.

Canon Tristram exhibited and made remarks upon the smallest known species of parrot, of which the skin measured only two inches in length.

THE CONGRESS OF HYGIENE.

WE printed on August 20 (p. 303) an account of some of the work done in the Section of Preventive Medicine in the Congress of Hygiene. The following is the conclusion of our report:—

ALCOHOLISM.

Sir Dyce Duckworth, of London, opened a discussion on "The Relation of Alcoholism to Public Health and the methods to be adopted for its Prevention."

Prof. Harald Westergaard, of Copenhagen, followed with a paper on the same subject. What are the losses of life, he asked, caused to a population by intemperance? This question can to a certain extent be answered by examining the causes of death, especially delirium tremens and chronic alcoholism. It has been objected that these causes of death supply an unsatisfactory picture of drinking excess, because the wish to spare the feelings of surviving relatives makes returns of such deaths less trustworthy, and it has therefore been proposed to use other diseases as a measure—such as liver disease (especially cirrhosis of the liver). Yet it is worth while to examine the above-mentioned causes of death. In most countries the statistics of the cause of death do not allow conclusions with regard to alcoholism corresponding to those for Denmark and Norway. But, at all events, the statistical data sufficiently show that a great part of the civilized world is suffering greatly from the effects of alcoholism. The investigations of the Harveian Society make it probable that in London one-seventh of all adult deaths (males and females) is directly or indirectly due to the consequences of alcoholic excess. The mortality in England from alcoholism in 1871–80 among males 25 to 65 years old was about 1 per cent. of all deaths—nearly 800 yearly. What an amount of disease and poverty, of moral and physical degradation, is represented by these 800 deaths! In Belgium the yearly loss of life from delirium tremens among males was 330 in 1870–89. Still greater have been the devastations of drinking in Switzerland. Prussia has a yearly loss of 1100 males from delirium tremens. Undoubtedly we should find, if trustworthy data could be had, that chronic alcoholism and delirium tremens alone kill many thousands of men every year. What is to be done? High excises are generally looked upon as an excellent weapon against alcoholism. But we must not forget that even a very high excise, as in England, does not prevent spirituous liquors from coming within the reach of anybody, so long as the number of public-houses is so exceedingly large as in this country. If a person has to go a long way to get drunk, and if he has in addition to pay a good sum for it, he will stop to think before going. Still, high excises seem to have some effect; the German law of 1887 has, for instance, reduced the consumption of spirits to a certain extent. But generally the reduction of the consumed quantity does not seem to correspond with the increase of the excise. An interesting expedient is the new State monopoly in Switzerland. Ten per cent. of the surplus are left to the cantons for counteracting alcoholism. By regulating the price the monopoly acts like an excise, and the Government takes care that only unadulterated liquors are sold. The monopoly is reported to have had a good sanitary effect, and it has caused some decrease in the consumption of liquors. In connection with excise and duties every effort is to be commended which tends to render the access to intoxicating liquors more difficult. Among these measures, the three popular American systems deserve our attention—viz. the Maine laws, local option, and the high-licence system. The first of these expedients—the prohibitory system—has been tried in Maine and some other American States. According to this system, it is prohibited to manufacture and sell intoxicating liquors, the only exception commonly being that liquors of "foreign production" may be imported and sold in the original packages. But this exception is unjust, permitting the man who can afford it to order as much liquor as he likes, and nearly all reports agree in testifying to the perpetual violation of these laws. One curious fact from Maine, where the system was adopted in 1881 may be mentioned. During the years 1867–86, 8412 divorces of marriages took place, being probably several per cent. of the yearly number of celebrated marriages. Of these no less than 960, or 11 per cent., were caused by intemperance, combined or not with other causes. It thus seems that intemperate habits are rather frequent in this State. Curiously enough, the State of Massachusetts (where there is a considerable revenue for licences) shows, under nearly the same regulations concerning divorces as in Maine, the same proportion—viz. 1054 out of 9853. It seems impossible to suppress the liquor traffic in the larger towns. Between the Maine laws and the high-licence system is an intermediate system—local option. According to this, it is left to the citizens of a village, town, city, or a larger district, to vote for local prohibition. This system seems to work somewhat better than the Maine laws, and it may prove useful in rural districts, the control in small communities being more easily carried through; but in larger towns it is probably ineffective, tempting as it does to a surreptitious liquor traffic. The third system—high licences—has been introduced in several States. Under this system licences for the sale of liquors can be taken out, but the fees are so considerable (for instance, 500 or 1000 dollars yearly) that many small saloons disappear. In some cases the sale of liquors through grocery stores is entirely stopped (Illinois). This system is reported to work well by reducing the number of drinking saloons, thus lessening the opportunity for drinking. It is maintained that "the high-licence system has thrown the liquor traffic into the hands of a more respectable class of dealers," and that those who pay high licences "help the authorities in the conviction of breakers of the law, under the fundamental principle of self-preservation." It is also to be recommended to limit the numbers of licences that may be taken out. This is the case with the Dutch law of 1881. Still more effective have been the efforts in Sweden, Norway, and Finland. The numbers of bars have been gradually greatly reduced, especially in the rural districts; and in most of the towns the so-called "Gothenburg system" has been introduced. According to this system, adopted since 1865 in Gothenburg, all or most of the licences in a town are given to a company which is not allowed to pay more than a fixed rate of interest to the shareholders, the surplus being spent for the benefit of charitable institutions or forming part of the municipal income. The result has been a great reduction of the number of bars. In Gothenburg the company in 1865 took out 40 licenses, but at once reduced the number of saloons to 23. The persons who manage the saloons get a fixed salary for the sale of spirits, and are therefore not tempted to encourage the customers to drinking. Moreover, there is a limitation of the hours during which the saloons are open, and other steps have been taken to prevent abuses. Undoubtedly this system—in connection with the great diminution of the number of bars in the rural districts of the country—has contributed very much to the conspicuous reduction of the alcoholism in the three countries before-mentioned. A very practical expedient is also the prohibition of sale of intoxicating liquors at groceries and similar shops, and this provision ought never to be omitted where steps are taken to limit the number of saloons. And last, not least, it is highly desirable to regulate the opening hours of the saloons.

Dr. Isambard Owen, of London, said he took part in the discussion solely to correct the numerous misquotations current of the "Collective Investigation Report on Intemperance of the British Medical Association," of which Report he was the author. A certain table of figures contained in the Report had been quoted apart from the context in such a manner as to lead the public to believe that, in the view of the author of the Report, the longevity of abstainers fell below that, not only of moderate drinkers, but even of the decidedly intemperate. The conclusions of the Report, as far as concerned the general health of the public, were the following:—(1) That habitual indulgence in alcoholic liquors, beyond the most moderate amounts, has a distinct tendency to shorten life, the average shortening being roughly proportional to the degree of indulgence. (2) That of men who have passed the age of 25, the strictly temperate live, on the average, at least ten years longer than those who become decidedly intemperate. (3) That in the production of cirrhosis and gout, alcoholic excess plays the very marked part which it has long been recognized as playing, and that there are no other diseases anything like so distinctly traceable to the effects of alcoholic liquors. (4) That, cirrhosis and gout apart, the effect of alcoholic liquors is rather to predispose the body towards the attacks of disease generally than to induce any special pathological lesion.

M. Milliet, of Berne, Dr. Norman Kerr, of London, Mr. J. Phillips, of London, Sir V. Barrington, L.C.C., Dr. Robinson, of Maine, U.S.A., Sir Joseph Fayrer, Prof. E. Alglave, of Paris, Dr. Kinkead, of Galway, Dr. Arthur, of London, Prof. Böhmert, of Dresden, and Dr. Sonsino, of Pisa, also took part in the discussion.

On Thursday afternoon, Dr. W. O. Priestley read a paper "On the Improved Hygienic Condition of Maternity Hospitals," of which the following is an abstract:—

During the end of the last century and the first half of the present one, the mortality in maternity hospitals was very large, both on the Continent and in Great Britain. According to Le Fort, it was at the rate of 34 per 1000, while, according to Miss Nightingale, it was only 4·7 per 1000 when patients were confined at their own homes; or, according to Dr. Matthews Duncan, 8 per 1000, equal to 1 in 125. The cause of the increased mortality in lying-in hospitals was the prevalence in these institutions of puerperal fever, 75 per cent. being due to this cause. The infectiousness of puerperal fever, long doubted, was at length established, and also the fact that various poisons, brought from the dissecting room—from patients suffering from erysipelas, eruptive fevers, and the like—became the germs of infection which might cost the lives of many patients. The researches of Pasteur, Koch, Lister, and others have shown that these poisons owed their virulence to the presence of microscopic germs which multiply in the body of patients and produce the deleterious results. Hence it came to be recognized that, by preventing the ingress of these germs to the bodies of puerperal patients, comparative safety, even in lying-in hospitals, was attainable; and the introduction of the antiseptic and aseptic methods has produced not only a remarkable diminution of mortality, but also of the morbidity or illness incident to the puerperal state. A short sketch was given of the modern methods adopted in several countries to insure the greater safety of patients in maternity hospitals, and of the results obtained in Europe and in the United States. The results were very striking, and were attributable mainly to the introduction of the antiseptic or aseptic modes of treatment, although other improvements are not lost sight of. In concluding he called attention to an interesting table in which were thrown together the statistics of maternal deaths in six lying-in hospitals, situated in various countries, since the introduction of aseptic or antiseptic methods. With these he had contrasted the figures of M. Le Fort before the era of antiseptics, and Mr. Newbatt, the distinguished President of the Statistical Society, had kindly computed for him the difference in the proportion of deaths in the two cases:—

Mortality in Maternity Hospitals from all Causes in various Countries of Europe (*Le Fort*).

BEFORE THE INTRODUCTION OF ANTISEPTICS.

	Deliveries.	Deaths.	Per 1000.
Total	888,312	30,394	34·21

AFTER THE INTRODUCTION OF ANTISEPTICS.

	Date.	Deliveries.	Deaths.	Deaths which would have occurred on basis of Le Fort's figures.
Vienna	1881–5	15,070	106	516
Dresden	1883–7	5,508	57	188
Russia	1886–9	76,646	290	2,622
New York ...	1884–6	1,919	15	66
Boston	1883–6	1,233	27	42
General Lying-in Hospital, London	1886–9	2,585	16	88
Total ...	—	102,961	511[1]	3,522

Number of lives saved out of the 102,961 since the introduction of antiseptics—

Expected deaths on Le Fort's basis	3522
Actual deaths	511
Saving	3011

Dr. Priestley said it would be seen that while, according to M. Le Fort, the maternal deaths in European lying-in hospitals were 34·21 per 1000 under the old *régime*, the mortality is now reduced to somewhat less than 5 per 1000. This computation, put in another way, indicates that if the former rate of mortality had been maintained 3522 maternal deaths might have been expected; the actual deaths were only 511. In other words, 3011 lives of mothers were saved as the result of new and purely scientific methods of treatment. This, he thought, might fairly be stated to be one of the most striking triumphs of preventive medicine. It was no mean achievement to rescue from death more than 3000 lives of women in the acme of their maturity, and when their lives were most valuable to their families.

Dr. Graily Hewitt, of London, Mr. F. Fowke, of London, and Dr. Leduc, of Nantes, spoke on the subject.

A paper was read by Dr. J. C. van Dooremal, of The Hague, on "La Prévention de la Cécité professionnelle."

Dr. Sisley, of London, read a paper on "The Prevention of the Spread of Epidemic Influenza."

Mr. Weaver and Dr. Felkin took part in the discussion.

Greene Pasha, of Cairo, read a paper on "The Influence of the Nile on Mortality in Egypt."

Dr. Felkin, of Edinburgh, read a paper entitled "Observations on Malaria and Enteric Fever in Central Africa, and on the possible Antagonism between Malaria and Phthisis."

Inspector-General Lawson and Mr. Weaver spoke on this subject.

Dr. Lewis Sambon, delegate of the Municipality of Naples, read a paper on "Measures adopted for the Prevention of Infectious Diseases and their Relation to our Knowledge of Epidemics." He first pointed out the similarity, which is most striking, between the mode of development and diffusion of infectious diseases and some insect pests, such as locusts for instance. Both have likewise their endemic areas, both their seasons of development, both in some years spread more widely, and at long intervals give rise to regular plagues; both migrate in the same constant direction, and both die away out of their endemic areas, subsiding in the struggle for life. He said that the diffusion of species by currents and winds will make us understand the peculiarities in the spread of infectious diseases, which had given rise, in all time, to the most strange theories. The influence of atmosphere has been very little studied in connection with infectious diseases, and by this he did not mean the registration of the prevailing lower winds during an epidemic, but serious bacteriological researches in the sinking sediment of the atmosphere and in meteoric waters. Instances of animals being carried by regular winds or wind-storms far beyond the limits of their homes are universally known. Insects of all kinds are often caught hundreds of miles from the nearest land, out on the high seas; North American birds not unfrequently are carried across the Atlantic to Scotland. Far more important is the influence of winds and currents in the distribution of microscopic animals. These minute organisms or their germs, generally adhering to other larger elements of dust, are raised and carried by the wind until they are allowed to sink again to the soil when the air is in stillness. About quarantine Dr. Sambon said that not only our modern investigations proved them useless, but that a long experience has utterly condemned them. England has been accused of being commercially and politically interested in the abolition of quarantine, and this preconception has unfortunately prevented many from valuing the most scientific and liberal ideas which have promoted their opposition to quarantine. No nation can boast of having held public health so high above commercial interest; and we must also remember that the English, at one time, have been the most sanguine supporters of quarantine. Quarantine was first instituted by the old republic of Venice, whose life and power lay entirely in commerce; and Dr. Sambon said that, although it had proved so disastrous to finance, so useless to sanitation, and so vexatious to liberty, he was proud that they were a glory of his country. Dr. Sambon concluded that the most important and perhaps the only satisfactory means against infectious diseases was the sanitation of towns and the hygiene of men. In speaking of the sanitation of towns he said how vast areas of the old city of Naples had been recently pulled down and new districts had been built. A large and splendid supply of water has been introduced since 1887, and when the drainage is completed, Naples will be one of the healthiest towns of Europe. He spoke of the poor classes of all our large towns, and said how they were the culture grounds of epidemics, and finished by saying that it is not enough to improve the sanitary conditions of a town, but that the principles of hygiene should be impressed on the minds and consciences of people, because there could be no public hygiene where private hygiene was not understood.

Deputy-Surgeon-General Bostock, C.B., and Sir Vincent Barrington, delegates of the Metropolitan Asylums Board, read a joint paper on "The Hospital and Ambulance Organization of the Metropolitan Asylums Board for the Removal and Isolation of Infectious Diseases." The paper was illustrated by plans, diagrams, and models.

Surgeon-General Bostock said that the present accommodation for fever and diphtheria consists of six hospitals:—

Name.	Position.	Acreage.	No. of beds.	Population served.
1. Eastern	Homerton	9	442	1,114,432
2. South-Eastern ...	Deptford	11	462	941,381
3. South-Western ...	Stockwell	8	340	582,591
4. Western	Fulham	6	224	690,138
5. North-Western ...	Hampstead ...	11	435	882,514
6. Northern	Winchmore Hill ...	36	480	—
			2383	4,211,056

The first five are in London. The Northern is for convalescents, and is four miles outside the northern boundary of the district. The position of these hospitals is shown on the map. The average length of the journey a patient has to be carried to reach the hospital nearest to his home is three and a half miles. During 1886–87 the number of beds in the eastern and western districts was found to be insufficient, and steps are now being taken to establish an additional hospital in the North-East of London, and to increase the number of beds in the Western Hospital to 400. These additions will give a total number of beds for fever and diphtheria of 2959, or one bed for every 1423 inhabitants. The total number of cases of fever and diphtheria admitted into the managers' hospitals from 1870 to the end of 1890 was 55,204. The accommodation for small-pox is the Floating Hospital at Long Reach, fifteen miles below London Bridge. It contains 350 beds for acute and severe cases on board the *Atlas* and the *Castalia*, the *Endymion* being used for administrative purposes, and 800 in the convalescent hospital at Gore Farm, four miles distant from the ships, giving a total of 1150 beds. The number of small-pox cases admitted into hospital since 1870 to 1890 is 56,979. To this number must be added 1028 cases other than small-pox, making a total of 58,007 admissions. The river service is exclusively used for small-pox cases, and consists of three wharves on the Thames in London for the embarkation of patients. The wharves, as shown on the map, are the "West" at Fulham, the "North" at Poplar, and the "South" at Rotherhithe. In each there is a floating pier in deep water, approached by a bridge, and a shed into which the ambulance carriage drives, with an examination room. As an example of the work, it may be stated that during the small-pox epidemic of 1884–85, 11,060 cases were removed from their homes to the Floating Hospital, 175 doubtful cases were sent from the wharves to the land hospitals, 38 cases were detained in London on account of fog, and 35 persons, not having small-pox at all, were vaccinated and taken home. The greatest number of patients taken down to the Floating Hospital in one day was 104, by the *Red Cross*, in three trips. At the close of the epidemic the Ambulance Committee were able to report the satisfaction they felt that so large a number of persons of both sexes and all ages, most of them in physical suffering, and many helpless from disease, had been carried in all weathers, throughout all seasons of the year, and to a great extent during the hours of darkness, without discomfort or detriment to the patients, and without mishap to any person whatever.

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[1] 4·363 per 1000.

Sir Vincent Barrington, after urging the importance of preserving statistics of work done from an economical, as well as a sanitary point of view, presented statistical papers of fever and small-pox cases treated in Board hospitals. He commented upon the supposed prevalence of disease in 1887, and urged every publicity to be given to Board work, to get over the old prejudices of the working classes against sending patients to the isolated hospitals. He showed a chart demonstrating that the increased use by the public of the Board hospitals and the transport from 1879 to 1890, had been followed by steadily decreasing fever mortality in London. Now over half the cases of scarlet fever in all London are probably treated in Board hospitals. He referred to the improved sanitation of dwellings and the decreasing severity of the type of the disease as factors in the decreased mortality observed. He presented small-pox pedigrees in non-epidemic times, showwing in one case that 19 persons, in another 10 persons, were infected from a single case. Also that 20 cases of the 53 treated this year had been barren of infecting others as they were so rapidly removed to floating isolated hospitals. The deduction drawn was that the rapid system of removal of recent years by the combined land and river service of the Board had a sensible effect in checking a possible epidemic. He presented the forms for recording the evidence of the existence of vaccination cicatrices on the improved system adopted after conferences with Board medical officers and the Local Government Board, and advocated other sanitary bodies adopting the same system, thus facilitating the compilation of statistics, invaluable for the advance of science, and therefore for the treatment and check of small-pox, and the consideration of protection by vaccination.

Dr. Seaton, of London, Dr. Armstrong, of Newcastle, Dr. Dudfield, of London, Prof. Stokvis, of Amsterdam, and Dr. Hauser, of Madrid, also spoke on this subject.

Surgeon-General Beatson, M.D., of Eastbourne, read a paper on "Prevention of Disease in Growing Towns."

Prof. Stokvis and Dr. Dickson spoke on the subject.

Dr. Pistor, of Berlin, read a paper entitled "Ueber die Desinfection," of which the following is an abstract. Dr. Pistor dealt with the general rules and methods to be observed in the disinfection of infectious diseases. Such rules should be short, clear, and capable of being understood by everyone. Incineration and boiling for half an hour are, of course, very effectual disinfectants, but they are not always applicable. A 1 to 2 per cent. solution of caustic soda is a very useful disinfectant. Other methods are steaming, mechanical cleansing (such as rubbing, brushing, &c.), carbolic acid solution (2 to 5 per cent.), lime-water containing about 20 per cent. of caustic lime, and a 1 to 2 per cent. solution of calcined carbonate of soda. These methods and solutions are effective against all the poisons of infectious diseases. The head of the house or institution ought to be responsible for the disinfection under the direction of the doctor, and a record ought to be preserved of the mode of disinfection used.

Sir William Moore, K.C.I.E., Q.H.P., read a paper on "The Prevention of Fevers in India."

A discussion followed, in which Surgeon-General Cook of Bombay, the President, Surgeon-General Beatson, Dr. Leduc of Nantes, Dr. Payne of London, Surgeon-Major Poole of London, and Dr. W. Dickson, R.N., took part.

Dr. Prospero Sonsino, of Pisa, read a paper on "The Principal and most Efficacious Means of preventing the Spread of Entozoal Affections in Man."

Dr. Sandwith, of Cairo, and the President, made a few remarks.

Dr. F. M. Sandwith, of Cairo, read a paper on "Cholera in Egypt."

Dr. Stekoulis, of Constantinople, and Dr. Simpson, of Calcutta, took part in the discussion.

Dr. Curgenven, of Teddington, read a paper on "The Disinfection of Scarlet Fever and other Infective Disorders by Antiseptic Inunction."

Dr. W. Gemmell, of Glasgow, spoke.

Dr. Phineas S. Abraham, of London, read a paper entitled "On the Alleged Connection of Vaccination with Leprosy."

Mr. Milnes, of London, Dr. Cassidy, of Toronto, and Surgeon-Major Pringle spoke on this subject.

Dr. J. P. Williams Freeman, of Andover, read a paper entitled "Importance of more actively enforcing Ventilation: suggesting a Standard of Air Impurity as a Basis of Prosecutions." Dr. Freeman said that ventilation is of well-recognized importance; the causation of phthisis is a good example of it. Foul air is a cause of tuberculosis in three ways: *directly*, by supplying the bacillus to the lungs, and through the saliva to the intestinal canal; *indirectly*, by causing tuberculosis in cattle, and by so reducing the human body's vitality as to render it a suitable nidus. The bacteriologist leads us to expect that fresh air will be hostile to the virus; the demographist shows that the death-rate from phthisis increases from islands, coast districts, agricultural districts, small towns, to large towns; also in occupations, according to their exposure to the open air, from farmers and fishermen up to drapers and printers (see Dr. Ogle's table). The loss of health from want of ventilation is so familiar as to be little thought of, but the deaths from phthisis alone, fully preventable, must be enormous. The Public Health and Factories Acts provide for proper ventilation of buildings. Any standard that public opinion, lay and medical, may demand might be enforced. Beyond seeing to the cubic space in common lodging-houses, practically nothing is done, and the air of buildings is often "dangerous and injurious to health." An inspector should frequently "sample" the air of buildings, and if it exceed a certain limit of impurity the owner should be prosecuted, cubic space and means of ventilation being left for the architect; the limit to be when the air inside a building contains twice as much carbonic acid gas as the air outside at the same time. This would usually correspond to De Chaumont's "Rather close, organic matter becoming perceptible." Students of preventive medicine should demand this reform from the administrators of the law. Polluted air is as recognizable, preventable, and harmful as unsound food or bad water, and should be treated on the same lines.

Two other papers were taken as read, one by Dr. S. Lodge, Jun., of Bradford, entitled "On the Occurrence of the Broncho-pulmonary form of Anthrax amongst Rag-pickers in England, and Suggestions for its Prevention," and one by Dr. H. Rident, of Elbœuf-sur-Seine, entitled "Des Troubles du Côté des agents de la Respiration chez les Fileurs, et de leur Conséquences."

After a speech by the President, complimenting the Secretaries on their work, and a vote of thanks to the President, the meetings of the Section terminated.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, September 2.—Mr. Frederick DuCane-Godman, F.R.S., President, in the chair.—Mr. G. F. Scott-Elliot exhibited a series of various species of Diptera collected on *Ranunculaceæ*, *Papaveraceæ*, and *Cruciferæ*. He said that during the past summer he had studied about forty species of plants belonging to the orders named, and that they had all been visited by insects which were probably necessary for nectariferous flowers. The majority of the Diptera caught were not confined to one species or even genus, but, in view of the unmodified character of the flower in the orders named, this was only to be expected. Mr. Verrall observed that certain insects affected certain plants, but that the *Geraniaceæ* were seldom visited. The discussion was continued by Mr. McLachlan, Mr. Kirby, and others.—Mr. W. L. Distant exhibited a specimen of the orthopterous insect *Hemisaga hastata*, De Sauss., which, in the Transvaal, he observed to attack and feed on *Danais chrysippus*, a butterfly well known from its protective character and distasteful qualities to have a complete immunity from the usual Lepidopteral enemies. The *Hemisaga* lurked amongst the tops of tall flowering grasses, being consequently disguised by its protective resemblance to the same, and seized the *Danais* as it settled on the bloom. From close watching and observation, Mr. Distant could discover no other danger to the life of this well-known and highly protected butterfly.—Mr. T. R. Billups exhibited four species of Diptera, which he believed to be respectively *Oxycera terminata*, *Pipezella annulata*, *Clidogastra puncticeps*, and *Oxyphora arnicæ*, taken at Oxshott, Surrey, on July 11 last. He mentioned that all of them were recorded in Mr. Verrall's list only as "reputed British." He also exhibited a specimen of *Hypoderma bovis*, Deg., taken at Plumstead on July 29 last.—Dr. D. Sharp, F.R.S., exhibited several species of *Forficulidæ*, and called attention to the diverse conditions of the parts representing the wings in the apterous forms.—Mr. H. Goss exhibited living larvæ of *Scoria dealbata*, reared from ova. They were feeding on *Polygonum aviculare*, but not very freely; *Brachypodium sylvaticum* had been named as a food-plant for this species, but he did not find that the larvæ would eat this or any other grass.—The Rev. Dr. Walker exhibited, and read notes on, a collection of Lepidoptera, Hymenoptera, Coleoptera, Neuroptera, and Diptera, which he had recently made in Norway.

PARIS.

Academy of Sciences, September 7.—M. Duchartre in the chair.—Remarks on the influence that the aberration of light may exercise on spectroscopic observations of solar prominences, by M. Fizeau. Several observers have recently measured remarkably high velocities in solar prominences by the application of the Doppler-Fizeau principle. It is evident that if the matter of which the eruption consists be ejected in the neighbourhood of the ecliptic with a velocity equal to that of the earth in its orbit, the prominence will suffer an apparent displacement of 20″·445, in the same manner that a star is displaced by 20″·445 owing to the motion of the earth combined with the velocity of light. Aberration should therefore be taken into account in determining the positions and heights attained by the phenomena in question.—On the number of roots common to several simultaneous equations, by M. Émile Picard.—On the blending of separate chromatic sensations perceived by each of the two eyes, by M. A. Chauveau. If two colours are simultaneously and separately received on the corresponding points of the two retinas and transmitted respectively to the nervous centres, do they blend together at these centres and give rise to the sensation of the resultant colour? This is the question investigated by the author. And he finds that there is a real blending of the colour perceptions resulting from the independent excitation of each of the two retinas.—On the influence of the products of the culture of *naphyloccque doré* on the vaso-motor nervous system and on the formation of pus, by M. S. Arloing.—Observations of the asteroid discovered by Dr. Palisa on August 30, made at Toulouse Observatory, by M. E. Cosserat. Three observations for position were made on September 1 and one on September 2.—On the distribution in latitude of the solar phenomena observed at the Royal Observatory of the Roman College during the first half of this year, by M. P. Tacchini. Prominences have been most frequent in the southern solar hemisphere, as was also the case in 1889 and 1890, and the maximum of frequency in the zones ± 40°–50°. The spots and faculæ have preserved their preponderance north of the equator, with maxima of frequency in latitudes slightly lower than the prominences. All the phenomena have been rare near the solar equator.—Direct synthesis of primary alcohols, by M. Paul Henry.—On some attempts to reproduce acid rocks, by M. H. Le Chatelier.—On the quantity of starch contained in the tubercles of the radish, by M. P. Lesage.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Livingstone and the Exploration of Central Africa: H. H. Johnston (Philip).—My Water Cure: S. Kneipp, translated (Blackwood).—Monthly Weather Reports of the Meteorological Office, May to December 1887 (Eyre and Spottiswoode).—Hourly Means, 1887 (Eyre and Spottiswoode).—Meteorological Observations at Stations of the Second Order, 1887 (Eyre and Spottiswoode).—Quarterly Weather Report of the Meteorological Office, July to December 1880, and October to December 1880 (Eyre and Spottiswoode).—Cyclone Tracks in the South Indian Ocean (Eyre and Spottiswoode).—Manufacture of Sulphuric Acid and Alkali; vol. i. Sulphuric Acid, 2nd edition: Dr. G. Lunge (Gurney and Jackson).—A Hand-book of the Destructive Insects of Victoria, Part 1: C. French (Melbourne, Brain).—Notes on Elementary Physiography: H. C. Martin (J. Heywood).—Peloponnesische Bergfahrten: Dr. A. Philippson (Wien).—An Account of British Flies, Part 1: M. C. E. Leigh and F. V. Theobald (E. Stock).—Studies from the Kindergarten, vol. iv., No. 1 (Laurie)—Carta delle Strade Ferrate Italiane al 1° Aprile, 1891 (Roma).—Jahrbuch der k. k. geologischen Reichsanstalt. Jahrg. 1890, xl. Band, 3 and 4 Heft (Williams and Norgate).—Himmel und Erde, September (Berlin, Paetel).—L'Anthropologie, 1891, Tome ii., No. 4 (Paris, Masson).—Journal of the Royal Horticultural Society, vol. xiii. Part 2 (117 Victoria Street).

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THURSDAY, SEPTEMBER 24, 1891.

PHYSICAL UNITS AND CONSTANTS.

Illustrations of the C.G.S. System of Units, with Tables of Physical Constants. By Prof. Everett, F.R.S. (London: Macmillan and Co., 1891.)

THIS may be taken to be the fourth edition of a work first published by the Physical Society in a somewhat different form. Those who know Dr. Everett need not be told that he has done everything that it is possible for an accurate, painstaking author to do, to bring each successive edition as near to perfection as possible. The value of the work to the physics investigator is exceedingly great, as everybody knows; but it is not so generally well known that it is an excellent class exercise book for students. There is much new matter in this edition, including determinations of viscosities, terrestrial magnetic elements, magnetic properties of iron and other substances, and heat measurements.

The labours of many men have given to the present generation this beautiful system of units, which has made physical calculation so easy, and which has pointed out in certain cases the directions in which new discoveries might be expected. And it only requires a short study of certain parts of this book to put any student in such possession of the system that he can use it with certainty and ease. Indeed, to become well acquainted with the scientific method of calculation has almost been made too easy for certain clever men of our acquaintance. It is far nobler to swim the Hellespont than to cross in a steamer. At the present time many clever men are possessed by a mania for crossing the Atlantic in boats of eighteen feet keel. It adds much more to one's credit to talk of all kinds of hybrid and home-made magnetic influences, than to use the simple idea of self-induction. In the same way it is unfair to say that certain practical engineers shirk the study of Dr. Everett's book; it is much better to put it that these gentlemen have too much originality to follow the easy path, and when in their practical applications of physical principles they adopt all sorts of ingenious units of their own manufacture to whose use there are limits in all sorts of ways, we can even feel sorrowful over their skilfulness, without attempt-to thwart their ambition.

The mechanical engineer is accustomed to the use of a curious unscientific want of system in his calculations. His unit of force is the weight of a pound in London. His velocity is in feet per second, perhaps, in the very same calculation as that in which his pressure is in pounds per square inch. It seems to be too late to change this. No engineer can venture to educate his pupils in the use of the C.G.S. system for mechanical engineering calculations. Mrs. Ali Baba measured her gold by the quart, and a mechanical engineer thinks and designs and talks with other engineers in the usual shop units; and we may as well think of altering our decimal system to a duodecimal one, as to talk of an alteration in the mechanical engineer's methods of calculation. It is a very great pity, but the difficulties in the way of reform seem to be insurmountable. The story of these difficulties is too long for the present notice. But in new applications of physics, in electrical engineering, for example, the use of the C.G.S. system is not only easy; it requires a large amount of ingenuity in any engineer to calculate in any other than in C.G.S. units, unless, indeed, he ignores all the experimental determinations already made for him and tabulated in the C.G.S. system. And yet such ingenuity has already been exercised, and laborious investigations have been carried out by some electrical engineers, with the result that certain parts of electrical engineering are getting to be even more unscientific in the units employed than any part of mechanical engineering. On behalf of the culprits we may say, however, that even Dr. Everett's book—their best guide—has not given them the precise information that it might have done. In the subject of heat, we can now ignore the steam-engine constructor; we can say to him, "Go on using your wretched pounds per square inch and your foot-pounds per minute, and we will go on using our dynes per square centimetre and our ergs per second because we are nearly independent of one another"; but we can make no such speech to the electrical engineer. We physicists have to say to him that we rely upon him to make new discoveries, to state to us new problems; and if he gives us information in vague units of his own, we cannot tabulate it for general use, and if he does not state to us his problem in the usual language, we are unable to understand him, and we can be of no mutual use to each other. But when he says to us that our language is cumbrous, that he has ideas to express for which we have no words, when he uses towards us, properly for once, that adjective "academic" which has been more misused than Shakespeare's word "occupy," the culprit and the judge change places.

We can blame him if he invents unsystematic units, but not until we have given him the language and units that are correct. And in some particulars the electric engineer has the right to blame us. For example, our definition of unit electric current is so stupid that a multiplier or divisor of π or 4π enters quite unnecessarily into all electro-magnetic calculation.

Concerning electro-magnets and the magnetic circuit of a dynamo machine or a transformer, the practical engineer has a simple and quite modern way of considering problems, not yet recognized in such orthodox books as this of Dr. Everett. *Magneto-motive* force and the magnetic *resistance* of a circuit are expressions which cannot be found in such a book, and it is not at all unusual for the orthodox physicist to treat the idea underlying the use of such expressions with profound contempt. The engineer and experimenter care less than nothing for "magnetic susceptibility" or for "intensity of magnetization," or for "free magnetism"; these are, to him, mementos of the time of twelve years ago, when the inventor made bricks in Egypt, and the very cleverest mathematical electricians were only distinguished from other inventors by the greater magnitude of their blunders. Dr. Hopkinson and Mr. Kapp and Mr. Bosanquet have given us simple ways of dealing with practical problems, and some of these are now known to every apprentice of an electric engineering factory; but we know of no mathematical treatise in which they are recognized. Is it too much to hope that Dr. Everett, in his next edition, will ignore the orthodox critics, and mention *ampere-hours*, and *ampere-turns*, and *Board of Trade units?* It would perhaps be going too far to expect him to speak of the drop of potential per ampere in 100 yards of "a cable of nine-seventeens," for he does not aim at displacing the electricians' pocket-books; but it is to be remembered that of all engineers the electrical engineer is the one who is most inclined to orthodoxy, who most leans upon the mathematician and physicist, who is most likely to use such a book as this; and if Dr. Everett can stretch a point in his favour, and devote, say, four pages to "electrical engineers' pocket-book" information, it will bind the electrical engineer to orthodoxy for ever. Why, for example, should Dr. Everett define the "impedance" of a circuit merely with reference to the circuit when conveying one particular kind of alternating current?

This book deserves much more than a short notice, and the time may perhaps come when one of our leaders will write a long critical article on the whole subject of units, pointing out the great differences in derivation of calorimetric units, for example, and the mere dynamical units employed in mechanics and electricity—an article which will teach the student that, although electric resistance has the same dimensions as a velocity, yet this is a very different thing from the statement that it *is* a velocity; that, in spite of Paris Congresses and Committees of the British Association, *sec-ohm* is a scientific name, and *quadrant* is not. But, over and above all this, the writer of the article must not be, as the present reviewer is, a poor specialist; he must criticize this book from the point of view of the general physicist. This book contains the results of all the best experimental work of more than a century. It is a book of mnemonics. A single line in the whole book recalls to us those magnificent memoirs of Dr. Andrews which revolutionized our ideas on liquids and gases, and yet that single line is quite enough to the physicist. It is dreadful and yet pleasing to think that all the work of a great man, or perhaps of a generation of great men, may be condensed into a single line of information in such a book as this. Would Dr. Andrews trouble himself very much over this fact if he were alive? or would he console himself with the thought that every physical fact discovered since 1869, and here recorded, was, to some extent, discovered through him, because he had made all physical workers his pupils? Would he need the consolation that Newton is not once mentioned, and that Sir William Thomson has less space devoted to him than the meanest of his pupils? Hundreds of years hence, the scientific world will be the better for the experimental work now going on, and it will have forgotten the name of almost every worker. Our determination of something is only right to four significant figures, and so it will never be quoted because a man of next century will have measured it with accuracy to five significant figures. How many of us can be sure that a single line of such a book as this, published a century hence, will be devoted to the record of any of his experimental results? Is there or is there not a satisfaction in knowing that, one thousand years hence, the names of even Faraday and Maxwell and Thomson will be as little known as ours. The age deserves a Homer, and a memory of thousands of years; and one book of the epic ought to be a list of all the men mentioned by Dr. Everett, saying what weapons each of them had brought for the common fight against the powers of darkness. But alas, the new Homer will probably not come into being for another three hundred years, and he will be a blind poet, and he will probably immortalize the wrong people.

JOHN PERRY.

OYSTERS.

Oysters and all about them. Being a Complete History of the Titular Subject, exhaustive on all points of necessary and curious information from the Earliest Writers to those of the Present Time, with numerous Additions, Facts, and Notes. By John R. Philpots (London and Leicester: Richardson and Co., 1891.)

The Oyster: a Popular Summary of a Scientific Study. By Prof. W. K. Brooks, of the Johns Hopkins University. (Baltimore: Johns Hopkins Press. London Agents: Messrs. Wesley, 1891.)

HISTORIANS of the oyster revel in ambitious titles. "The Oyster: Where, How, and When to Find, Breed, Cook, and Eat it" suggested a somewhat extensive field for the tiny octavo which Cruikshank illustrated, but yet greater anticipations are raised by the title of Mr. Philpots's contribution to the subject.

Unfortunately, this promise is not borne out; not from lack of labour on the writer's part, but from the want of that critical knowledge which can alone make a compilation of this nature valuable. Mr. Philpots has thrown together, with but little arrangement, into two volumes of 1300 pages, scraps from every conceivable source relating to the oyster, and this without any critical treatment whatever: all are oysters that come to his dredge. Since at least as much erroneous information is current about the oyster as about any other well-known animal, and since it appears to exert nearly the same deleterious influence as the horse on the truthfulness of those who deal in it, it will be readily understood that the 1300 pages abound with errors and contradictory statements, and form a most untrustworthy guide to the complicated subject of which they treat.

The melancholy side of the situation is that, had the compiler, evidently an enthusiast for his subject, devoted the time and labour expended on the collection of paragraphs from untrustworthy authorities, to qualifying himself for his task by obtaining a personal and practical acquaintance with the oyster in all its relations of life, he might have produced a less bulky work, but one of permanent value; as it is, the only passages which we have been able to identify as indicating that Mr. Philpots has seen an oyster or an oyster-bed, are to be found in his account of ten sorts of oysters sent to him by a London dealer, among which, by the way, the real native does not occur (pp. 332–36), and in chapter xix., containing a short account of the Poole fisheries.

To correct the errors of Mr. Philpots's authorities, and to indicate his omissions, would be to criticize, not one book, but all the readily accessible matter which has been written on oysters for the last half-century; accessible matter only, for even as a compiler Mr. Philpots has not the requisite qualifications for his task, being seemingly dependent for his information about foreign oysters upon the translations and abstracts which have appeared from time to time in the Report and Bulletin of the United States Fish Commission, and upon the Hand-books, &c., to the International Fisheries Exhibition. These, with Grenville Murray's "The Oyster, Where, How, When," &c. (1861 and 1863), Williams's "Silvershell; or the Adventures of an Oyster" (1856), and Eyton's "History of the Oyster" (1858), are the chief part of his stock-in-trade; to which may be added newspaper articles, reviews, extracts from popular natural histories, &c. Besides these "authorities," some fifty pages, largely taken from Gwyn Jeffreys's "Conchology," deal with Brachiopoda (!), Anomiadæ, Pectinidæ, and Ostreidæ; under the latter family there is an account of *Ostrea edulis*, but none of *Ostrea* (*Gryphæa*) *angulata* and *virginica*, although the book does not profess to be confined to the former species; and about 212 pages are occupied by reprints of Parliamentary papers of various sorts.

The only chapter in which we are at one with Mr. Philpots is that in which an appeal is made to the Government to take the "oyster question" seriously in hand, though even here we cannot but regret the tone in which he speaks of the Board of Trade. Unhappily, however, there is no denying the fact that the inspectors sent by the Board to report on oyster fisheries have often been unfit for their task, and have, sometimes at any rate, been freely fooled by interested parties, for want of a little practical acquaintance with their subject. This has been pointed out again and again, not only as regards oyster fisheries, but also in connection with other fishery questions; but it cannot be pointed out too often. A point to which Mr. Philpots should have drawn public attention is that, if the proposition to move the London drainage outfall to Foulness take effect, the best of the few remaining grounds for breeding the almost extinct "native" (*sensu stricto*) will in all probability be ruined.

A book of a different calibre is that of Prof. Brooks. It is avowedly merely an attempt to rouse the State of Maryland to take such measures with regard to the oyster-fisheries as can alone prevent their ruin, measures such as some other States have already taken with marked success. It is hardly necessary to say of Prof. Brooks that his little book is a clear and accurate summary of what is known about the American species, for few men can speak with more authority on the subject. We can only hope that the Legislature to which he appeals may be more far-sighted than our own. Had the restrictions which he advocates been laid on our English public beds fifty years ago, the rare "native" might be almost as cheap now as in those almost forgotten days when the market was not yet flooded with French and Dutch produce posing as the genuine article, and oyster grottos were a familiar feature of the streets.

THE DESTRUCTION OF MOSQUITOES.

Dragon-flies v. Mosquitoes. (New York: D. Appleton and Company, 1890.)

THE book before us consists of three prize essays written in response to a circular issued in 1889 to "The Working Entomologists of the Country," offering certain prizes for essays containing original investigations on methods for destroying the mosquito and the house-fly. The prizes were offered by Mr. R. H. Lamborn, whose position as Director of the Lake Superior and Mississippi Railway had caused him to spend a considerable time encamped in the swampy forests which surround the head of the great lake. Here he came into contact with mosquitoes of the most irritating kind, and here he made the interesting observations on their destruction by dragon-flies which stimulated him to offer the above-mentioned prizes. The lines laid down in the circular as to the direction which the investigations should follow have reference chiefly to the destruction of these insect pests by dragon-flies. The competitors were also required to examine which species of Odonata are best adapted for the purpose, to investigate their habits, and the possible methods of breeding them in large numbers. But although this line of inquiry is suggested, the practical object of the investigation is to determine whether it is possible to diminish or extinguish the noxious Diptera, and if so, by what means.

The essay which gained the first prize is by Mrs. C. B. Aaron, who gives a careful account of the habits and life-history of both the Diptera in question, and of the Odonata, and then considers the advisability and the means of exterminating the former. The gravest charge which is adduced against these Diptera, apart from the irritation they cause, is that they act as carriers of such parasites as Filaria, and possibly of some species of Tænia, whilst they undoubtedly serve to disseminate Bacteria associated with certain infectious diseases. In their favour it may, however, be said that they act as very efficient scavengers, especially during the larval period of their life-history; and it is a very open question whether the world would be much benefited by the total extinction of the two genera Culex and Musca. Without attempting to decide this point, Mrs. Aaron proceeds to consider the possibility and the cost of attempting their extermination.

The plan of pitting the dragon-fly against the gnat—a plan similar to that which Prof. Riley has brought to such a successful termination by encouraging the destruction of the orange scale, *Icerya purchasi*, by means of a small beetle, the *Vedalia cardinalis*, imported from Australia—is dismissed in a few words, for reasons which are considered at greater length in the following essays; but several mechanical means are suggested, the most promising and cheapest of which, in the case of the mosquito, is to spray with crude petroleum all collections of stagnant water which cannot be easily drained. The oil forms a thin film on the surface of the water, and effectually clogs the aperture of the breathing tubes as soon as the larvæ come to the surface, as they must do, for air.

The authors of the two remaining essays, Mr. Weeks and Mr. Beutenmuller, divide the second and third prizes. The former commences his essay with a valuable table, giving details of the time of appearance, of the comparative voracity, and of the habitat of sixteen species of dragon-fly found in the neighbourhood of New York. From these, three are selected—*Anax junius*, and *Æschna constricta* and *heros*—as the most likely to prove destroyers of mosquitoes. When, however, the life-histories of the opposed insects are compared, it becomes at once evident that we must not trust to the Odonata to rid us of the biting Culicidæ. The breeding and artificial rearing of dragon-flies present almost insuperable difficulties, for, when the larval stage is attained, each individual would have to be isolated, because they are apt to devour each other when confined in a limited space. Irrespective of the question of breeding, an insect which produces but one brood a year, and lives but a few days in the imago condition, has little chance of seriously affecting a race whose numerous annual generations succumb only to the severest weather. In its natural condition the dragon-fly does not correspond sufficiently closely with the mosquito, either in time or space, to give it any real chance of effecting the destruction of the latter; its breeding-places are also more restricted, as it requires a volume of water which is constant for some little time, whereas the mosquito, with its quicker metamorphosis, can make use of any temporary puddle.

The conclusion to be drawn from all three essays is, that if a serious attempt is to be made to combat these most annoying insects, the means to be adopted with most chance of success lie rather in the direction of draining swamps, raising fish, and encouraging water-fowl in the infested ponds, and, where it would not be injurious, using crude oil, than in any efforts to increase the supply of dragon-flies.

Mrs. Aaron and Mr. Beutenmuller have appended to their essays useful lists of papers on the subject of their work; and the latter has added a preliminary list of the Odonata in the State of New York, and a very useful catalogue of the "described transformations of the Odonata of the world." The book is illustrated with several plates, which depict stages in the life-history of the insects in question, and various mechanical devices for attracting mosquitoes, by means of lamps, to an oily grave; and for spraying with petroleum the water in which they breed.

A. E. S.

OUR BOOK SHELF.

Materials for a Flora of the Malayan Peninsula. No. 3. By George King, M.D., F.R.S., &c. Reprinted from the Journal of the Asiatic Society of Bengal, Vol. LX. Part 2.

DR. KING'S third contribution towards a flora of the Malayan Peninsula contains the *Malvales*, and comprises almost as large a proportion of new species as the two preceding parts, but no new genus. The Malvaceæ number twenty-four species belonging to eleven genera; the Sterculiaceæ, forty-eight species belonging to twelve genera; and the Tiliaceæ, fifty-eight species belonging to nine genera. Although 25 per cent. of the species are new, there are only three of the first natural order and five of the second; the rest belong to the Tiliaceæ, of which nearly half are new. Nine out of ten species of *Pentace* were previously undescribed, and only two others are known. There are seven additional species of the characteristic genus *Elæocarpus*, out of a total of twenty-three. This is the largest number of any one genus, though *Sterculia* comes next with twenty-two species. It will be perceived that the new species are almost exclusively trees. The flora of Malacca and Cochin-china is exceedingly rich in the arboreous element; the number of new species described by Dr. King in his various monographs and by Dr. Pierre in his "Flore Forestière de la Cochinchine" being something enormous.

W. B. H.

Zoological Wall Pictures. Three Diagrams, each 32 inches by 42 inches. (London: S.P.C.K.)

The Animals of the World, arranged according to their Geographical Distribution. Third Edition, Revised and Re-drawn. Size, 58 inches square. (London: Moffatt and Paige.)

THE first named depict (1) fishes, as represented by the cod, eel, and herring; (2) chelonians, as exemplified by the common water tortoise and the Greek land tortoise, together with drawings of parts of the chelonian skeleton; (3) insect pests, in the *personæ* of the Pine Bark and Colorado beetles, the larvæ of which are delineated. The diagrams are both bold and accurate, and good of their class.

The second named embodies an attempt to represent the distribution of the animals selected in latitudinal series. The plan, although a good one, is manifestly insufficient, inasmuch as by its means no provision can be made for overlap. However, for a bold wall diagram, the picture may be recommended. Its meaning is at once obvious; and a fact such as the occurrence of seals and whales at extreme latitudes, which at once arrests the attention, is sufficient in itself to arouse the spirit of inquiry in any active mind. In future editions the word "Some" might with advantage be substituted for the article "The" which heads the title.

Crozet's Voyage to Tasmania, New Zealand, the Ladrone Islands, and the Philippines, in the Years 1771–72. Translated by H. Ling Roth. Illustrated. (London: Truslove and Shirley, 1891.)

IN 1769 a Tahitian was brought to Europe by Bougainville as "a human curiosity." Afterwards he was sent to the Mauritius, the Governor of which was instructed to forward him to his destination. The task of restoring him to his native land was undertaken by Marion du Fresne, who was then a well-to-do resident in the Île de France; and thus originated the expedition the story of which is recorded in the present volume. The party started in two vessels, and Marion proposed, in the course of the voyage, to do much exploring work—a kind of enterprise for which he seems to have been well fitted, as he had been a distinguished officer of the French navy. Unhappily, some members of the expedition, including Marion himself, were massacred by the Maories. The voyage, however, was continued, and in 1783 an account of it was published which had been compiled and edited by the Abbé Rochon, the well-known traveller, from the log of M. Crozet, who, after Marion's death, commanded one of his two ships. It is this account which Mr. Ling Roth has translated. The work will be read with interest by students of the history of geographical discovery, and a good many of M. Crozet's statements about savage life have considerable value from the point of view of the ethnographer and the anthropologist. A preface, and a brief reference to the literature of New Zealand, are contributed by Mr. J. R. Boosé, Librarian of the Colonial Institute; and the volume contains, besides maps, very good illustrations of some works of Maori art.

Livingstone and the Exploration of Central Africa. By H. H. Johnston, C.B., F.R.G.S., &c. (London: G. Philip and Son, 1891.)

THIS volume ranks with the best of the series to which it belongs—"The World's Great Explorers and Explorations." Mr. Johnston realizes fully the splendour of Livingstone's achievements, and has succeeded admirably in bringing out their significance in the history of African exploration. He begins with two excellent general chapters dealing with the "natural history" and the "human history" of Central Africa; and afterwards he gives vivid accounts of all the various regions traversed by his hero. Thus the reader is enabled to form his own opinion as to the value of Livingstone's services. The strictly biographical part of the work is equally well done. All the world agrees that Livingstone was one of the noblest men who have ever devoted themselves to travel. This is felt strongly by Mr. Johnston, and he has been able to express his feeling effectively without extravagance and without any attempt at fine writing. The book will especially interest young readers, but may be studied with pleasure and profit by readers of any age. There are many good illustrations from photographs or drawings by the author, and seven maps by Mr. E. G. Ravenstein.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The National Home-Reading Union.

WHEN one remembers the difficulties with which one's own first efforts to study Nature were beset, it seems a pity that any youthful student should be ignorant of the existence of an organization which can do much towards making his path smooth.

The National Home-Reading Union endeavours to guide those who cannot obtain aural instruction into the safest and most attractive roads. Lists of books are drawn up; difficulties and discrepancies in systematic reading are, as far as possible, foreseen and removed in the pages of the magazine; questions are answered by those who conduct the courses. Last year and the year before, the courses on organic and inorganic Nature were in the charge of Mr. Francis Darwin, Dr. Hickson, and Dr. Kimmins. This year, geology is undertaken by Mr. Marr, and cryptogamic botany by Mr. Murray; and any persons who wish to work at these subjects may save themselves much labour and misplaced reading by writing to the Secretary of the Union, Surrey House, Victoria Embankment, for a prospectus. Mr. Murray tells me that it is often pitiful to see how mnch effort has been wasted by people who come to the British Museum to educate themselves, owing to the need of guidance to the right books with which to commence their studies.

I trust that this good work will commend itself to you as worthy of notice. ALEX. HILL.

Downing Lodge, September 17.

Notoryctes typhlops.

ALLOW me to protest against the misnomer "Marsupial Mole" applied to Dr. Stirling's marvellous mammal by Mr. Sclater, both in the *Times* and in NATURE. "Mole-like Marsupial" it may be, but the other phrase has quite a different meaning, and either shows a want of appreciation of important characters, or implies a theory which, however plausible, has not been proved. ALFRED NEWTON.

September 12.

"$W = Mg$."

I WISH that Prof. Greenhill would kindly explain to a bewildered reader of your paper the nature of his quarrel with "$W = Mg$," and with the writers of "theoretical" treatises who use this equation.

To those trained to regard quantity of matter as measured by its inertia, and who regard the "mass" of a body as the quantity of matter, so measured, which it contains, the equation $W = Mg$ has a pretty clear meaning.

A certain body "has a mass M," this being the measure of its inertia in terms of that of the mass-unit. This body is observed to have an acceleration g. We argue, from Newton's experimental laws, that there is a force acting on it; and we measure this force by a number which is the product of the two numbers, M (the measure of the mass of the body), and g (the measure of the acceleration observed).

If we observe a tight string attached to the body in question, and have every reason to believe that there is no other cause for the observed acceleration, we say that Mg measures the tension T of the string; or write "$T = Mg$." If the acceleration be due to the presence of the earth only, we say that the earth exerts a force [the "half" of the mutual stress] on the body, measured by Mg. This force we call the "weight of the body"; and the equation $W = Mg$ gives us the measure of the "weight" as deduced from the observation of rate of change of momentum produced by it.

If I felt sure that Prof. Greenhill considers M to be *still* merely a convenient abbreviation for $\frac{W}{g}$, I would say more on this matter; but I am in doubt as to what are the views of which he is so strong an opponent.

I see that he wishes to abolish "g" from works on hydrostatics. Why? I do not see how we can conveniently indicate the dependence [*cæteris paribus*] of hydrostatic pressure on the strength of the earth's gravitational field of force at any given place otherwise than by the introduction of g. But, as I have already implied, I am as yet in the dark as to the precise nature of the quarrel between Prof. Greenhill and the theorists.

Devonport, August 17. W. LARDEN.

[WE look to America for clear, unprejudiced ideas on the definitions of elementary dynamics, and Mr. Frederick Slate's letter from California is a valuable contribution, to which I hope Mr. Larden has directed his attention.

The quotations from certain elementary treatises which form Mr. Larden's letter are the statements it was my chief object to dispute; according to this school of writers, the Standard Pound Weight is not the lump of platinum preserved at the Exchequer, but rather it is the pressure on the bottom of the box in which it is kept.

When goods are sold in commerce by weight, they are weighed in scales, and the weight is the same wherever the weighing is carried out, whether at the equator, or the poles, or in the Moon, Sun, or Jupiter; so that the weight cannot be said to depend on the local value of g, the only effect of which is to slightly alter the infinitesimal strain of the balance.

Let Mr. Larden consult the recent Report of the Committee on Electrical Standards, to see how carefully the units must be defined to satisfy practical commercial requirements.—A. G. G.]

WHEN I was young, I never had the presumption to understand the use of "g" in questions connecting mass and weight, and I fear my boy takes after me.

He told me the other day that he understood how a falling body could have its velocity increased per second with a velocity of g, or 32 feet per second; and that he knew that m = stuff in a body, and w = its weight, but he could not see what the "blooming g" (I think that is what he called it) had to do with the matter.

I replied that no doubt, if we could only understand it, it had a beneficent use in the economy of nature.

TOMMY ATKINS, Senior.

Sleep Movements in Plants.

I READ the other day in a local paper that "Mr. Seemann, the naturalist of Kellett's Arctic Expedition," states that plants undergo sleep movements at regular intervals (presumably once in 24 hours) during the long period when the sun never sets. Has this been authenticated? I thought it was well known that a plant does not undergo periodic variations of the kind if it has never been subjected to the regular succession of light and darkness. Other instances are the daily periodicity of the strength of so-called "root-pressure" and of the rate of growth. But if the above observations are correct, not only have the sleep-movements become independent of the ordinary determining conditions in the individual, but they have become hereditary in the species. If the movements really possess the significance usually assigned to them (of checking excessive radiation) this would seem to negative the prevalent view that the state of panmixia alone suffices for the disappearance or degeneration of a structure or mechanism. A. G. TANSLEY.

September 19.

An Oviparous Species of Peripatus.

MR. DENDY's observation of the extrusion of incompletely developed eggs in Peripatus is not, as he appears to think, entirely new. Captain Hutton was the first to observe it, in *P. novæ-zealandiæ*, and I confirmed his observation for the same species in my monograph of the genus. No one knows whether the eggs so extruded undergo complete development. I am inclined to think that the process, which has only been observed in animals in captivity, is an abnormal one, and is caused by the alteration in the conditions of the animal's life. We know that the New Zealand species does bring forth fully-developed young.

I hope that Mr. Dendy will carry out his intention of fully investigating the development of the Australian species.

A. SEDGWICK.

Trinity College, Cambridge, September 18.

A Rare Phenomenon.

ON a visit to Dunecht, I was just leaving the Observatory about 11.18 G.M.T. on the 10th inst., when I saw a sharply-defined straight streak of light arching the sky from east to west. It was about 1° in width, and of uniform brightness from side to side, but more intense towards the western horizon, where it disappeared behind the trees at an altitude of some 4°. Eastward it extended across the constellation of Andromeda, near the girdle, quite beyond the convergence point of auroral rays, or fully 120° from the western horizon. This much I saw, but cannot say if the streak passed north or south of the Great Nebula.

Endeavouring to lay down its course, I perceived that it was rapidly fading, and at the same time drifting southwards at a rate of, perhaps, 1° in five minutes. At 11h. 21·0m. G.M.T. the western portion was considered to cross the celestial equator in R.A. 262½°, passing through a point in R.A. 310° and Decl. + 23° (1840·0). In the meantime the eastern portion had faded away. Although there was a bright aurora in the north-north-west, I did not think that the streak was auroral in character, but rather that it had been caused by the passage of a large meteorite. Next day, however, I stumbled on an account of a similar appearance seen, together with an aurora, by the Rev. Edmund Barrel, at Sutton at-Hone, in Kent, on March 30, 1717 (O.S.). In the Philosophical Transactions, vol. xxx., after describing an ordinary aurora, the account runs:—

"Near Eleven a Clock, there was (besides the *Northern* Brightness) a long Streak, not very broad, extended *East* and *West:* Which beginning in the *Serpent's-Head*, near *Hercules'-Club*, and covering *Arcturus*, proceeded near *Berinices Hair*, and so went over *Cor Leonis*, and thence to *Canicula*, [Procyon, for Sirius had already set] and ended a little beyond that Star. It shone very bright at first, but faded away in about Eight or Nine Minutes. If it had Motion (which I am not sure of) it was Southward. I waited for the next Fit of Brightness of the *Aurora;* and in about Seven Minutes, the *Eastern* Part of the Streak, viz. from the *Serpent's-Head* to near *Berinices Hair*, became visible again tho' dim, and was quite effaced in Four or Five Minutes more: And I did not yet perceive any Change of its Place."

The course described agrees fairly with the arc of a great circle 120° in length, joining Procyon and the head of Serpens.

Assuming the Dunecht arch to have been also part of a great circle, its highest point must have been 8° 50′ east of the meridian, at an altitude of 62° 24′ above the southern horizon. The Magnetic Survey of Profs. Rücker and Thorpe gives the point to which the dipping-needle is directed as 19° 49′ E.; altitude 71° 3′, for 1891·69.

A letter signed "Wigtownshire" in the *Scotsman* of September 14, dated September 12, says:—"There appeared here last night, between nine and ten, a very bright, luminous arch, reaching from south-west to north-east. It extended directly over the zenith from horizon to horizon, and formed a very interesting spectacle while it lasted, which was only about half an hour. It seemed to be of electric origin from its wavy motion, and was slightly tinged pink at the eastern point just above the horizon. . . ."

Assuming the correctness of the dates on which the arch was observed—and of the Dunecht date I am quite certain—it seems that this rare phenomenon was visible on two successive nights.

RALPH COPELAND.

Royal Observatory, Edinburgh, September 21.

LAST Friday, the 11th, my attention was called at 9 p.m. to a most remarkable appearance in the sky. It consisted of a luminous band stretching from the eastern horizon to the west, and passing a little to the south of the zenith. It was first seen here at 8.20, and began as a luminous ray coming up from the west, but when I first saw it, it had extended as described from west to east. It was like a straight tail of a large comet with its head below the horizon, or the track of the beam from a powerful electric search light. Its eastern end lay a little to the south of the Pleiades, which were just rising; and in the west it passed through Corona Borealis. The night was a brilliant starlight one, and small stars could be seen through the luminous band. It was seen in the Co. Kildare, 50 miles from here, and there it passed through the zenith also, which would show that it was at a great altitude. It gradually faded away, and was gone at 9.30. It would be of interest to know if it was observed in other parts of the country.

W. E. WILSON.

Daramona, Streete, Co. Westmeath, September 16.

SOME NOTES ON THE FRANKFORT INTERNATIONAL ELECTRICAL EXHIBITION.

I.

ON arriving in Frankfort one finds oneself in a lofty, palatial railway station, compared with which King's Cross looks mean and Victoria Station is a shanty. This new terminus at Frankfort is not, as with us, an hotel with trains whistling and shunting in the back premises; it is essentially a railway station, standing proudly alone at the western extremity of the town. And the practical Englishman is as much impressed by the completeness of its internal arrangements as by the anti-Ruskin lesson it teaches, that architectural skill when fitly applied to a railway station can produce as noble an edifice as when bestowed on a temple.

Leaving the railway station all is changed. We are on the outskirts of the town, amid unfinished houses, heaps of bricks, vacant plots strewn with rubbish, and the restless hammering of the house contractor. The Exhibition is close at hand, composed at first sight mainly of wooden hoardings, temporary structures, "restaurations," and *bier hallen:* it is the Chalk Farm fair again of our early youth, or Chicago in 1873, a month after the great fire. Presenting at the entrance a letter bearing the magic pass-words "Prüfungs Commission der Internationalen Electrotechnischen Ausstellung," we are ushered past the barrier with bows, and find ourselves surrounded on all sides by shows—Siemens and Halske's Miniature Theatre, admission 2½*d.*; Electrical Ballet, admission 1*s.*, 2*s.*, and 3*s.*; Diving Pavilion, seats 5*d.*, standing room 2½*d.*; Electrical Race Course, 2½*d.*; Siemens and Halske's Dancing Flames, 2½*d.*; and so on, all over the Exhibition grounds. Have we come all these miles, at an invitation conveyed to us through the English Foreign Office, merely to visit a collection of what are literally twopenny-halfpenny shows?

We try one of them, the Miniature Theatre, passing in by the stage door, through the courtesy of Messrs. Siemens' representative, and thus avoiding the crowd of people that flocks in at every one of the many afternoon and evening performances. In view of the audience are 48 handles, which work a large puppet show, but a puppet show without puppets, without music, without acting, without even a joke. Turning any one of 36 of these handles towards the left turns on a group of little white or red or blue incandescent lamps placed at the sides, at the top, and at the bottom of the little stage, but hidden by the scenery from the audience. Turning any one of these handles to the right also turns on the respective set of lamps, but now their brightness can be gradually diminished by revolving one of the remaining 12 handles, which gradually introduces resistance into the particular circuit. For example, either the red, or the white, or the blue lamps behind any side wing, top drop, or set piece, can be separately turned on, or all can be turned on and the brightness of the lamps of any one colour varied independently of the brightness of the remainder.

A bell tinkles, and the curtain rises, showing a pretty set scene of a Swiss village with mountains in the background. It is late in the afternoon. The attendant slowly revolves one of the resistance handles—the daylight wanes, the shadows grow long, the sun sets, and the snowy peaks of the mountains are ruddy with the Alpine glow. The effect is so lifelike and so beautiful that a spontaneous gasp of admiration is forced from the audience.

Then the stage grows gradually dark, lights are seen at the cottage windows, but the night is stormy, for the attendant now works the handles rapidly, as does the organist the stops when performing one of Bach's fugues: lightning plays on the hills, now a blinding flash lights up the road, the houses, and the waterfall; but the flashes grow less vivid, and one sees, or thinks one sees, the storm blowing away over the mountain tops. Presently the moon rises, the audience feel the quiet of the bright moonlight night, then the dawn, and finally the sunshine bathes the scene with light.

Since the opening of the Exhibition many theatrical managers, we were told, had ordered complete sets of this electric apparatus; and no wonder, for on it can be played a symphony in the music of colour.

We next went to see Messrs. Siemens and Halske's "dancing flames," the seats at this show being also well filled with a twopence-halfpenny paying audience. First, Koenig's manometric flames were described and shown in action; then Dr. Froelich's method of working them from a distance, the elastic membrane of the little gas-bag being pushed in and out, not directly by the air puffs, but by the motion of the ferrotyped iron disc of a telephone, the current through which was varied by speaking to a microphone. Next were shown some experiments, extremely interesting to the electrician, for illustrating graphically how self-induction, mutual induction, capacity, &c., affected the current produced by an alternate current dynamo.

We presume that the considerable number of people who, having paid for their entrance to the Electrical Exhibition, are willing to form group after group and pay an extra fivepence at the many performances that are given daily of these two shows by Messrs. Siemens and Halske, are not wholly ignorant of what they are paying to see. Probably, therefore, the continued attraction which such shows have for audiences drawn from the people is only another proof of the fact that science, and a love of science, have permeated to a much lower stratum of the nation in Germany than in England.

Numerous must be the Germans not much above the level of the sightseers at a village fair who have already listened to the explanation of Dr. Froelich's method for exhibiting these alternate current phenomena, and yet the method is novel to the majority of the English scientific visitors. For it was only some three months ago, when Prof. Perry showed his new steam-engine indicator to the Physical Society of London, that the President suggested how he thought it possible that that instrument might be converted into an oscillating telephone with a mirror on its iron disc, and used for projecting on a screen the current curve of an alternate current dynamo. But nobody at the meeting was apparently aware that Dr. Froelich had been employing a telephone with a mirror on its disc for this very object—such is the resistance to the spread of ideas introduced by difference of language.

The apparatus employed by Dr. Froelich is as follows:—A large telephone iron disc has a small piece of looking-glass stuck on it eccentrically, and at the back is a horse-shoe permanent magnet, the soft iron pole-pieces of which are wound with a coil carrying the current produced by an alternate current dynamo. The iron disc is therefore pulled more or less by the magnet, depending on the strength and direction of the current passing round its poles. A beam of light from an electric lamp is reflected from this mirror on to a screen, and as the alternating current flows round the magnet a vertical line of light is formed on the screen, the position of the spot of light on this line being at any moment a measure of the strength and direction of the current produced by the machine. At least, this will be the case if the natural period of vibration of the telephone plate be very small or very large compared with the periodic time of the current—a condition we presume Dr. Froelich has attended to.

To produce a motion of the spot of light at right angles to the former line, Dr. Froelich does not cause the telephone to be moved backwards and forwards with an oscillatory motion, by the rotation of the dynamo armature, as suggested at the Physical Society of London; but before the beam of light reaches the screen, he causes it to suffer a second reflection from one of a series of small plane vertical mirrors, arranged around the surface of a cylinder parallel to its axis. By suitable worm-gearing, the quick rotation of the dynamo causes a somewhat slow rotation of this cylinder, but quick enough to produce an apparently continuous horizontal beam of light along the screen if there be no current flowing—that is, if the mirror on the telephone plate be at rest. Hence, the combination of the vertical and horizontal motions of the beam produces a curve which shows the shape of the current-wave extending over some four or five periods.

The effect of adding self-induction or mutual induction or capacity to the circuit is instantly seen by the change in the shape of the current-curve on the screen, and the change of phase is also evident from the shifting of the whole series of waves sideways. The comparison between the current waves in the primary and secondary circuits of a transformer is also very prettily illustrated.

This lecture concluded with an exhibition of an apparatus that has been constructed for Dr. Froelich for the examination of compound sounds. On a shaft, turning at a uniform velocity, are eight little alternate current dynamos, and by pressing down a piano key, which closes the circuit of the particular dynamo, a current is sent round the soft iron pole-pieces of the horse-shoe permanent magnet at the back of a telephone disc. The number of pole-pieces and armature-coils on the respective dynamos are such that, on pressing down the keys in succession, the telephone emits the notes of an ordinary musical octave, and by pressing down two or more the compound sound is heard.

An Englishman finds it somewhat exasperating, if he desires to see the whole Exhibition, to have to be constantly taking out his purse to make small payments for entrance here and entrance there; but, as half the receipts for the shows go to the Exhibition authorities, they will be saved from the financial *fiasco* that attended the Edinburgh Exhibition of last year, for that Exhibition had to be finally declared bankrupt, even after all the money guaranteed by the promoters had been called up. Further, the shows are themselves illustrations of the application of electricity to industry and art: the mere bazaar element, that has been so prominent a feature at some of the Exhibitions held at Earl's Court, is practically non-existent at the Frankfort International Electrical Exhibition.

International, however, the Exhibition is but in name, the comparatively small exhibits of one or two English and American firms only serving as a reminder of the magnificent collections of electrical machinery and apparatus England and America could have contributed. As a display, however, of the part Germany is playing in the development of electrical industry, the Frankfort Exhibition is most interesting.

Two separate buildings are devoted respectively to electrical railway signalling and to telegraphic and telephonic exhibits. The Government have contributed an interesting collection of historical telegraphic apparatus, from which it may be seen that the signalling instruments have been going through the same sort of evolutionary changes in Germany as in England, with this difference, however, that our apparatus has reached a much later stage of development than theirs. The German telegraph wires have been well erected, although less attention than would satisfy an English telegraph engineer has been paid in obtaining that perfect symmetry in the hanging of the wires which is necessary to avoid contacts being produced between them as they are swayed backwards and forwards by the wind. The underground wiring is especially good, but the methods of testing and signalling are antiquated, and the routine of the Telegraph Department generally is fettered with red tape.

There is one detail, however, in connection with the German Post Office, that forces itself on the admiration of the foreigner. If you desire to send money, you hand in the sum at the post-office, with a postcard costing 2½*d*, which you address to your correspondent with details of the sum sent, and receive a receipt in exchange. But you need write no letter, send no postal order nor receipt, nor trouble your correspondent to go to the post-office; the postman delivers to your correspondent at his house or office your postcard, and in return for half of it hands him at once in cash the sum of money sent.

The display of telephonic apparatus at the Exhibition is large and complete, but owing to the activity of the commercial traveller of the day in keeping English engineers acquainted with practically all that is being done abroad, there is little that strikes the English telephone engineer as new. A new telephone exchange switch-board, constructed by Messrs. Mix and Genest, contains, however, a point of novelty, and a switch-board of this description has just been adopted at the Berlin Telephone Exchange.

The general arrangement of an exchange switch-board is as follows:—The wires from all the subscribers are brought to all the clerks at the exchange, so that it is possible for any clerk to connect any subscriber with any other, to enable the two subscribers to talk to one another. The calls, however, from certain subscribers only are received by any particular clerk; for example, of all the wires coming to clerk A, only those from, say, 1 to 100 are provided with drop shutters, so that if any subscriber from 1 to 100 rings up the exchange, one of the drop shutters in front of clerk A will fall, whereas if a subscriber from 200 to 300 rings up the exchange, it will be a drop shutter in front of clerk C that will fall. Each clerk, therefore, deals with the calls from a certain set of subscribers only, but this clerk may have to connect any one of this set of subscribers with any other of the same set or with any subscriber of any of the other sets; since, of course, any subscriber to the exchange has the right to be put in communication with any other.

Suppose, now, that clerk A receives a request from subscriber 85 to be put in communication with subscriber 560, the first thing to find out is whether the line of subscriber 560 is free, or whether it has been already connected with some other subscriber by one of the other clerks. This is usually ascertained by means of what is known as a "testing wire," which permeates all the switch-boards of all the clerks, and enables any clerk to see whether any line coming into the exchange is free or not. But in a large exchange the running of this testing wire throughout all the switch-boards necessitates the employment of many miles of wire, and it is to avoid this that Messrs. Mix and Genest have adopted the following new device:—

The ends of the plugs which the clerk presses into the various holes, or "spring jacks" as they are technically called, for the purpose of connecting one subscriber with another, are made electrically in two parts, the tip of the plug being insulated from the remainder by a piece of ebonite; a couple of cells are joined up at the exchange to each pair of plugs, in such a way that on inserting the tip of the second of a pair of plugs into a spring jack, an instantaneous current passes, deflecting the needle of a galvanoscope if the second line be free. For example, clerk A receives a call from subscriber 85 to connect him with subscriber 560: he inserts one of a pair of plugs into the spring jack 85, he then inserts the second plug into spring jack 560, and as the top of this second plug enters the spring jack there will be an instantaneous swing of clerk A's galvanoscope if line 560 be free, in which case the clerk pushes the plug home, and completes the connection between subscribers 85 and 560. If, however, the needle of the galvanoscope does not deflect, the clerk knows that line 560 is occupied, having been connected up by one of the other clerks, and instead of pushing home the plug he pulls it out, and tells subscriber 85 to wait, as line 560 is engaged.

Long-distance telephony is admirably illustrated by the opera at Munich being heard every evening with marvellous clearness at the Frankfort Exhibition, some 200 miles away.

The most striking feature of the Exhibition—indeed, the exhibit that has brought many a foreigner hundreds of miles to Frankfort—is the electrical transmission of power from Lauffen, over a distance of 109 miles. No measurements have yet been made by the jury of the exact amount of power that is received, or of the efficiency of the transmission; but as over 1000 sixteen-candle lamps are daily fed by the current, as well as an electro-motor pumping up water to form a large artificial waterfall, the actual power received must be something like 100 or 110 horse.

The plans had to be rapidly formed, for it was not until May 1 that it was definitely decided to carry out the experiment. The transformers have, on the one hand, been duplicated, from an anxious dread on the part of each firm of contractors that the other would not have finished their work in time; while, on the other hand, the insulators of the proper size are yet only partly ready, and many are defective from too hurried baking. Permission to carry the wires had to be obtained from the four Governments of Baden, Hesse, Würtemburg, and Prussia, and every step of construction had to be taken under the depressing influence of cavilling criticism. But in spite of all these difficulties, it has been conclusively proved that, by means of three overhead bare copper wires, each only 0·158 inch in thickness, supported on poles such as are used for ordinary telegraph lines, it is possible to deliver some 110 horse-power at a distance of nearly 110 miles from the water stream where the power is produced; and further, that this may be done without excessive loss by actually maintaining a potential difference of some 18,000 volts between each pair of wires.

The result is of international importance. The methods that have been employed (and which will be fully described) will probably not be copied in detail on a future occasion; there are doubtless faults which the cautious engineer can criticize; but the broad fact still stands out prominently, that, by an experiment as bold in conception as it has been successful in its realization, the Allgemeine Electricitäts Gesellschaft of Berlin, in conjunction with the Oerlikon Works of Zürich, have made the thoughtful realize that towns like Milan, which are within 30, 40, or 50 miles of vast water-power, may become the industrial centres of the future. It is, indeed, as if it had been shown that such towns stood on an inexhaustible field of smokeless, dustless coal.

(*To be continued.*)

SOME POINTS IN THE PHYSICS OF GOLF.

II.

IN my former paper (*Nature*, Aug. 28, 1890) the main conclusions were based to a great extent upon the results of mere eye observations, often of a very uncertain and puzzling kind. The data so obtained were unfortunately *not* those required for a direct investigation, so that my processes were necessarily of a tentative character. During and since the last College session I have been endeavouring to obtain some of the more important data in a direct manner. I am thus in a somewhat more favourable position than before but, as will soon appear, the new information I have obtained has complicated rather than simplified the singular problem of the flight of a golf-ball.

One point, however, which is both curious and important, has been clearly made out:—*hammering has no effect* (or, to speak more correctly, *only an inconsiderable effect*) *on the coefficient of restitution of a golf-ball.* This conclusion, which may have to be modified if the striking surface be not plane, had for some time appeared to me as almost certainly correct, and I have recently verified it by means of the Impact apparatus with which I have been working for some years. I procured from St. Andrews a number of balls of the same material and make, half of them only being hammered, the others plain. The results obtained from a hammered, and from an unhammered, ball did not differ much more from one another than did those of a number of successive impacts on one and the same ball. [In the *Badminton Library* volume on Golf, Mr. Hutchinson quotes a statement of mine which appears at first sight diametrically opposed to this experimental result; and thus puts me in the position *de nier ce qui est et d'expliquer ce qui n'est pas.* But he has omitted to mention that my statement was expressly based on the allegation that a hammered ball had been definitely found to acquire greater speed than an unhammered one. This seemed to me even at the time very doubtful, and I now know that it is incorrect.] Thus it is clear that the undoubtedly beneficial effects of hammering must be explained in some totally different way. There is another, and even more direct, mode of arriving at the same conclusion. To this I proceed, but unfortunately the new point of view introduces difficulties in comparison with which all that has hitherto been attempted is mere child's play. In short, it will be seen that the problem of a golf-ball's flight is one of very serious difficulty.

In my former article I took no account of the *rotation* of the ball, treating the problem in fact as a case of the motion of a particle in a medium resisting as the square of the speed. The solution I then gave was only approximate, and limited by the assumption that the cosine of the inclination of the path to the horizon might be treated as unity throughout. The illustrations and extensions given were founded on the same basis as was the solution of the simpler problem. Shortly after it was published I made, by the help of Bashforth's tables, a more exact determination. The data I thus arrived at were (in Bashforth's notation)

$$\lambda = 1\cdot9, \quad u_0 = 131 \text{ feet-seconds}, \quad \phi = 13^\circ\cdot5.$$

From these the tables give at once

Range of Carry... ... . = 542 feet
Maximum Height ... ... = 58 ,,
Horizontal Distance of Highest Point
from Tee ... ... = 350 ,,
Initial Speed ... ... = 480 feet-seconds
Terminal ,, ... = 80 ,,
Terminal Inclination ... ... = 38°·5.

As a contrast, take $\lambda = 1\cdot1$, so that $u_0 = 100$ feet-seconds. To obtain the observed range we must take $\phi = 23^\circ\cdot25$, which is considerably too great. The other numbers then become

Range of Carry... ... . = 543 feet
Maximum Height ... ... = 100 ,,
Horizontal Distance of Highest Point
from Tee ... ... = 350 ,,
Initial Speed ... ... = 393 feet-seconds
Terminal ,, ... = 80 ,,
Terminal Inclination ... ... = 54°·6

The first numbers are in remarkable accordance with the numerical details of really good drives which I obtained from Mr. Hodge; and, were there no other crucial test to be satisfied, the problem might have been regarded as solved to at least a first approximation. But I felt very suspicious of the sufficiency of such a solution; especially as it made no place (as it were) for the possibility of a path in part straight, or even occasionally concave upwards, which I have certainly seen in many of the very best drives. And my doubts were fully justified when I calculated from Bashforth's tables the time of flight under the above conditions. For they give 1·51s. for the first, and 2·13s. for the second, part of the path:—3·6 seconds in all; while the observed time of flight in a really good drive is *always* over 6 seconds, and sometimes quite as much as 7. This I have recently verified for myself with great care in the competition for the Victoria Jubilee Cup, where one of the unsuccessful players distinguished himself by really magnificent driving. The time of flight in the second of the above forms of path is about 4·8 seconds.

The initial speed in the first estimate seems to be excessive, as will appear from the experiments to be described below. This, of course, is one mode of explaining how the time of flight is so much underrated. But, if we keep to Bashforth's value of the coefficient of resistance, it is impossible to reduce the initial speed (while preserving the observed range) without increasing the angle of projection and, with it, the greatest height reached. The second set of numbers conclusively proves this. On the other hand if, with the view of reducing the initial speed and thus increasing the time of flight, we assume a smaller resistance, we may keep range, height, and initial angle, nearly as observed; but we shift the vertex of the path unduly towards the mid-range. The only way, it would therefore seem, of reconciling the results of calculation with the observed data, is to assume that for some reason the effects of gravity are at least partially counteracted. This, in still air, can only be a rotation due to undercutting.

During last winter I made a considerable number of experiments with the view of determining the initial speed by the help of a ballistic pendulum, but the results of these cannot be regarded as very satisfactory. My pendulum was a species of stiff but light lattice-girder constructed of thin, broadish, laths. This hung from hard steel knife-edges set well apart, and supported a mass of moist clay of about 100 lbs. The clay was plastered into a nearly cubical wooden frame, and swung just clear of the floor. The ball was driven into it from a distance of about six feet, and as near as possible to the centre of one face. The effective length of the corresponding simple pendulum was about 10 feet, and the utmost deflection obtained (measured on the floor) was about two inches. From these data I deduced an initial speed of about 300 feet per second only. But the experiments were never quite satisfactory, as the player (however skilful) could not free himself entirely from apprehension of the consequences of an ill-directed drive. In fact, several rather unpleasant accidents occurred during the trials, especially in the earlier stages; when the pendulum was mounted in a stone cellar, and without the hangings and the paddings which were employed in the later work. Although the clay was so stiff as to preserve its form under gravity, the ball (when it struck the face near the centre) always penetrated to a depth of more than one diameter, and splashed fragments of the clay to a considerable distance. These were usually replaced, and the surface levelled for a fresh experiment, as soon as the ball was dug out. The speed of 300 feet per second, thus measured, may be taken as an inferior limit to the initial speed in a really fine drive.

It thus appears that the resources of mere particle dynamics are quite insufficient for the adequate solution of the problem of long driving; though, of course, they fully meet all questions connected with mere approach shots; and that the rotation of the ball must play at least as essential a part in the grandest feature of the game, as it has long been known to do in those most distressing peculiarities called heeling, toeing, slicing, &c. But when this is once recognized, it is only the beginning of sorrows; for even the approximate treatment of the eddies produced by the rotation appears to be at present beyond our powers.

In order that the path of the ball may be (for a short time) approximately straight, still more if it is to be concave upwards, the downward acceleration due to gravity must be neutralized by the effects of a rotation due to *undercutting*. [Of course enormous speed could produce the approximately straight path, but not the concavity.] Hence the necessity for a tee, unless the turf be exceptionally soft, in order that the club may impinge on the lower part of the ball. Hence also one important use of hammering, viz. that the undercut ball may take as much angular velocity as possible:—the other being that the spin, so acquired, may tell as much as possible during the flight. The gist of the matter is thus seen to be:—For steady flight the ball must have rotation of some kind. The best mode, that of a rifle-ball, is of course unattainable. The others produce respectively heeling, toeing, dooking, and soaring. Of these the last, alone, is not necessarily disastrous; and it is therefore to be adopted.

I have not hitherto succeeded in my attempts to apply even approximate calculation to this altered set of conditions:—but it is easy to see, without calculation, that the longer the path of the ball retains nearly its initial inclination to the horizon (even if, in achieving this, it should have to expend part of its energy of translation along with that of rotation, and thus diminish the range) the longer will be the time of its flight during the carry.

And, as a practical deduction from these principles, it would appear that to secure the longest possible carry the ball should be struck so as to take on considerable spin:—so that the ideal driver should be in truth a Bulger, but with the important variation that its bulge should be of considerable curvature and in a *vertical*, not a horizontal plane. The height of the most prominent part of the face (above the horn) must of course be less than the radius of the ball How much less can be found only by trial. And, in addressing the ball, the player must stand directly opposite to it. Such clubs, however, could be profitably used only by really good players:—men who can hit with what part of the club they please. The reckless swipers of the present generation, who slash away anyhow, and (with ordinary clubs) manage occasionally to make a really "tall" drive, will probably smash the proposed form of club on the very first appearance of topping. As to those who propel the ball by "skittling" rather than driving, any change *must* be an improvement, so that they should welcome the proposed novelty. The matter is a very simple one. A few touches skilfully applied with a rough file, and the new system rises at once out of the old.

There is one other point on which opinion seems to be so unsettled that an allusion may be made to it here:—the effects of weather on the carry of a ball. Of course, other circumstances being the same, the only direct effect is on the coefficient of resistance. If this be taken as proportional (roughly) to the density of the air, it may vary, in this climate, to somewhere about ten per cent. of its average amount, by increase or by diminution. It has its greatest value, and the drive is accordingly shortest, on a dry cold winter day with an exceptionally high barometer. The longest drive will of course be when the air is as warm and moist as possible and the barometer very low.

P. G. TAIT.

HOOKER'S "ICONES PLANTARUM."

THE recent issue of the fourth part of vol. xx. of the entire work completes the volume, and closes the third series, with a total of two thousand plates. This useful, and now indispensable, publication was commenced by the late Sir William Hooker in 1837, and the first volume was dedicated to the late George Bentham, who is described in the dedication as an "ardent promoter, not less by his patronage than by his writings, of botany and horticulture." Sir William Hooker started the "Icones" to illustrate some of the numerous novelties in the collections which were pouring into his herbarium from various parts of the world, especially from the southern hemisphere, at that period. With a few exceptions by Harvey, Gardner, and others, the drawings and descriptions were by Hooker himself, and a volume, containing one hundred plates, appeared annually, or nearly so. The first series closed with the fourth volume in 1841. At this date the founder was already Director of Kew Gardens, and he continued the work to the tenth volume, which terminated the second series. Two or three of the later volumes of this series were illustrated by the then rising botanical artist, W. H. Fitch. In the tenth volume we find a dedication of the whole ten volumes to George Bentham, in much the same words as the first. This was in 1854. After an interval of thirteen years, the third series was commenced, under the editorship of Dr. J. D. (now Sir Joseph) Hooker; and G. Bentham, D. Oliver, and J. G. Baker were contributors. Mr. Bentham, we believe, financed the undertaking. This, the eleventh volume, was not completed until 1871; but it is a most interesting volume, illustrated by Fitch, and containing among other things many of the endemic plants of St. Helena. The second volume of this series, the twelfth of the whole, was also illustrated by Fitch, and is valuable for the figures of curious new genera founded by Bentham and Hooker when elaborating their "Genera Plantarum."

On the completion of this volume, in 1876, a difficulty arose, consequent on the retirement of the artist, though there was no actual interruption in the appearance of the parts. But it was impossible to replace an artist like Fitch. Indeed, the only alternative was to train a person to do the work. This was not so easily accomplished; there were failures, and so high a standard of excellence has not since been reached. Nevertheless, the present artist gives as good drawings as could be expected from dried, flat specimens, and the botanical details are usually as full as is necessary, if not all that could be desired.

Since Mr. Bentham's death, in 1884, the work has proceeded with greater rapidity, and is now appearing at the rate of a volume per year. It is now published at the expense of the Bentham Trustees,[1] and sold at about half the former price; and since his retirement Prof. D. Oliver has undertaken the editorship. Under such favourable auspices, together with the abundance of material in the Kew Herbarium, it is confidently hoped that the interesting character of the work will be fully maintained, and that the mechanical production of it will be improved, resulting in a larger sale. The later volumes contain a large number of Chinese novelties. One part of the last volume is devoted to the Stapeliæ of South Africa. The seventeenth volume is wholly devoted to new ferns; and the first volume of what it is intended to call the fourth series will consist entirely of orchids. Three parts of this have already appeared.

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[1] Of a fund bequeathed by Bentham for the advancement of botanical science.

ON VAN DER WAALS'S TREATMENT OF LAPLACE'S PRESSURE IN THE VIRIAL EQUATION: A LETTER TO PROF. TAIT.

MY DEAR PROF. TAIT,—In Part IV. of your "Foundations of the Kinetic Theory of Gases,"[1] you take exception to the manner in which Van der Waals has introduced Laplace's intrinsic pressure K into the equation of virial. "I do not profess to be able fully to comprehend the arguments by which Van der Waals attempts to justify the mode in which he obtains the above equation. Their nature is somewhat as follows:—He repeats a good deal of Laplace's capillary work, in which the existence of a large, but unknown, internal molecular pressure is established, entirely from a statical point of view. He then gives reasons (which seem, on the whole, satisfactory from this point of view) for assuming that the magnitude of this force is as the square of the density of the aggregate of particles considered. But his justification of the introduction of the term a/v^2 into an account already closed, as it were, escapes me. He seems to treat the surface-skin of the group of particles as if it were an additional bounding-surface, exerting an additional and enormous pressure on the contents. Even were this justifiable, nothing could justify the multiplying of this term by $(v - \beta)$ instead of by v alone. But the whole procedure is erroneous. If one begins with the virial equation, one must keep strictly to the assumptions made in obtaining it, and consequently *everything* connected with molecular force, whether of attraction or of elastic resistance, must be extracted from the term $\Sigma(\mathrm{R}r)$."

With the last sentence all will agree; but it seemed to me when I first read Van der Waals's essay that his treatment of Laplace's pressure was satisfactory, and on reperusal it still appears to me to conform to the requirements above laid down. As the point is of importance, it may be well to examine it somewhat closely. The question is as to the effect in the virial equation of a mutual attraction between the parts of the fluid, whose range is small compared with the dimensions of bodies, but large in comparison with molecular distances.

The problem thus presented may be attacked in two ways. The first, to which I will recur, is that followed by Van der Waals; but the second is more immediately connected with that form of the equation which you had in view in the passage above quoted.

In the notation of Van der Waals (equation 8)

$$\tfrac{1}{2}\Sigma m\mathrm{V}^2 = \tfrac{1}{2}\Sigma f\rho - \tfrac{1}{2}\Sigma \mathrm{R}r\cos(\mathrm{R}, r),$$

where V denotes the velocity of a particle m, which is situated at a distance r from the origin, and is acted upon by a force R, while (R, r) denotes the angle between the directions of R and r. The intermediate term is to be omitted if R be the total force acting upon m. It represents the effect of such forces, f, as act mutually between two particles at distances from one another equal to ρ. In the summation the force between two particles is to be reckoned once only, and the forces accounted for in the second term are, of course, to be excluded in the third term.

In the present application we will suppose all the mutual forces accounted for in the second term, and that the only external forces operative are due to the pressure of the containing vessel. No one disputes that the effect of the external pressure is given by

$$-\tfrac{1}{2}\Sigma \mathrm{R}r\cos(\mathrm{R}, r) = \tfrac{3}{2}pv;$$

so that

$$\tfrac{1}{2}\Sigma m\mathrm{V}^2 = \tfrac{3}{2}pv + \tfrac{1}{2}\Sigma\rho\phi(\rho),$$

if with Laplace we represent by $\phi(\rho)$ the force between two particles at distance ρ. The last term is now easily reckoned upon Laplace's principles. For one particle in the interior we have

$$\tfrac{1}{2}\cdot 4\pi\int_0^\infty \phi(\rho)\rho^3 d\rho,$$

and this, as Laplace showed,[1] is equal to 3K. The second summation over the volume gives $3\mathrm{K}v$, but this must be halved. Otherwise each force would be reckoned twice. Hence

$$\begin{aligned}\tfrac{1}{2}\Sigma m\mathrm{V}^2 &= \tfrac{3}{2}pv + \tfrac{3}{2}\mathrm{K}v \\ &= \tfrac{3}{2}v(p + \mathrm{K}),\end{aligned}$$

showing that the effect of such forces as Laplace supposed to operate is represented by the addition to p, the pressure exerted by the walls of the vessel, of the intrinsic pressure K. In the above process the particles situated near the surface are legitimately neglected in comparison with those in the interior.

Van der Waals's own process starts from the original form of the virial equation—

$$\tfrac{1}{2}\Sigma m\mathrm{V}^2 = -\tfrac{1}{2}\Sigma \mathrm{R}r\cos(\mathrm{R}, r),$$

where R now refers to the *whole* force operative upon any particle; and it appears to me equally legitimate. For all particles in the interior of the fluid R vanishes in virtue of the symmetry, so that the reckoning is limited to a surface stratum whose thickness is equal to the range of the forces. Upon this stratum act normally both the pressure of the vessel and the attraction of the interior fluid. The integrated effect of the latter throughout the stratum is equal to the intrinsic pressure, and, on account of the thinness of the stratum, it enters into the equations in precisely the same way as the external pressure exerted by the vessel. The effect of Laplace's forces is thus represented by adding K to p, in accordance with the assertion of Van der Waals.

I am in hopes that, upon reconsideration, you will be able to admit that this conclusion is correct. Otherwise, I shall wish to hear more fully the nature of your objection, as the matter is of such importance that it ought not longer to remain in doubt.

Believe me yours very truly,

RAYLEIGH.

L'Abbaye de St. Jacut-de-la-Mer, September 7.

NOTES.

THE French Association for the Advancement of Science met at Marseilles on September 17, under the presidency of M. P. P. Dehérain, who chose as the subject of his address the part played by chemistry and physiology in agriculture. The meeting comes to an end to-day. There were general excursions on Sunday to Arles, and on Tuesday to Aix; and it is proposed that to-morrow, the 25th, there shall be a final excursion to the Mediterranean coast.

THE Congress of German Naturalists and Physicians was opened at Halle on Monday by Prof. His, of Leipzig. The meeting was attended by 1215 persons, including many distinguished foreign physicians and men of science and 280 ladies.

THE Helmholtz celebration, deferred from August 31, is now fixed for November 2. After the ceremony the delegates and others will dine together at the Hotel Kaiserhof.

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[1] Ed. Trans., vol. xxxvi, Part 2, p. 261.

[1] See also *Phil. Mag.*, October 1890, p. 292.

By the death of August von Pelzeln, which took place on the 2nd inst. at Ober-Döbling, near Vienna, Europe has lost one of her foremost ornithologists. He had been in failing health for some years, and had recently retired, after forty years' service, from his post of Custos of the Imperial Museum at Vienna, where he had charge of the collections of Mammalia and birds. Von Pelzeln will be always celebrated in the memory of zoologists by his important essays on the collections in the Vienna Museum, but his most enduring work will be found in the famous "Ornithologie Brasiliens," wherein he gave a detailed account of the collections made by the great traveller Natterer in the early part of the present century. Only last year he published in the *Annalen des k.k. naturhistorischen Hofmuseums*, an account of the formation of the collections of Mammalia and birds in the Imperial Cabinet, which is a very valuable historical record. The amiability of his character and his great knowledge of zoology had raised up for Von Pelzeln a host of friends in every country, and the news of his death will be received with wide-spread regret.

A Reuter telegram from New York announces the death of Prof. William Ferrel, the meteorologist.

The Royal Academy of Sciences at Lisbon send official notice of the decease of their Secretary, José Maria Latino Coelho, who died on the 29th ult. at Cintra, at the age of sixty-six. Besides his Secretaryship of the Académie Royale des Sciences, Prof. Coelho held the post of Director of the Mineralogical Section of the Museum at the École Polytechnique de Lisbonne.

The death of M. Wilken, the well-known Dutch ethnologist, has excited much regret in Holland, where his scientific work was greatly appreciated. He was forty-four years of age, and had spent some time as a Government official in the Dutch East Indies, where he had ample opportunities for carrying on his favourite studies.

Prof. K. Goebel has been appointed Professor of Botany in the University, and Director of the Botanic Garden at Munich, in the place of the late Carl v. Nägeli.

The Photographic Society of Great Britain announce the holding of an exhibition, which will be open from September 28 to November 12.

The most interesting part of the Royal Horticultural Society's exhibition on Tuesday was a series of the so-called carnivorous and insect-eating plants. It was hoped that the display of this series would tend to correct some very mistaken ideas which are said to be current on the subject. According to Mr. Weathers, the Assistant Secretary of the Royal Horticultural Society, some persons, relying on what they have heard, will assert that "these plants can easily dispose of a beefsteak or mutton chop if their digestive organs are in thorough repair."

The annual meeting of the Federated Institution of Mining Engineers was held on Tuesday at the Mason College, Birmingham, and was attended by about 120 members. Mr. T. W. Embleton, of Leeds, presided. In the report it was stated that the Council had not yet undertaken any special inquiry connected with the objects of the Institution, but their attention had been directed to the question of safe explosives for use in mines, the mechanical ventilation of mines, and other subjects. By the permission of the Durham Coal-owners' Association and the Durham Miners' Association, a report upon the fumes produced in mines by roburite, tonite, and gunpowder had been printed in the Transactions. The North of England Institution had appointed a committee to examine and report upon the so-called "flameless" explosives for use in mines. A paper sketching the geology of the Birmingham district was read by Prof. Lapworth. A paper was also submitted by Messrs. W. F. Clark and H. W. Hughes, in which the local method of working the thick coal was described to the visitors, and the peculiarities of the South Staffordshire coal-fields were described in technical detail. Mr. Arthur Sopwith supplied some similar information with reference to the North Staffordshire portion of the coal-field. These two papers were taken as read, and the discussion was deferred until the members of the Institution had visited the principal Staffordshire pits.

A Report for the year ending May 31 last, by Mr. G. J. Swanston, the Assistant Secretary of the Marine Department of the Board of Trade, upon the colour tests used in the examination of candidates for masters' and mates' certificates in the British mercantile marine has been issued as a Parliamentary paper. The number of persons who presented themselves for examination for masters' and mates' certificates of competency under Form "Examination 2" amounted to 4688, being an increase of 26 over the previous year, when 4662 were examined. In the past year 31 persons were rejected for their inability to distinguish colours, as compared with 23 rejected in the previous year. The number of persons examined in colours only under Form "Examination 2*a*" amounted to 601. Of these, 32 were rejected, being an increase of over 1·8 per cent. as compared with the previous year, when, out of 839 candidates examined, 29 were rejected. A few of those who failed to pass succeeded afterwards in satisfying the examiners. One man, who, on March 3, described a green card as drab, drab as green, pink glass as salmon and green, standard green as blue, bottle green as red, and neutral as green, passed a fortnight later, having apparently learned to distinguish the colours in the intervening period. The mode of conducting the colour-test examination described in the Report for the year 1887 is still in operation; but Mr. Swanston notes the fact that the whole subject of colour-vision and the best mode of conducting the examinations are now being investigated by a Committee appointed by the Royal Society.

On his return from Japan, sixteen years ago, Prof. Rein, the well-known authority on Japanese art and industry, planted in the Botanical Garden at Frankfort some specimens of the lacquer-tree (*Rhus vernicifera*), from which the Japanese obtain the juice employed in the production of their famous lacquer work. According to the *Times*, there are now at Frankfort thirty-four healthy specimens of the lacquer-tree, 30 feet high and 2 feet in girth a yard from the ground; and the young trees, which have sprung from the original tree's seed, are in a flourishing condition. It seems to be proved, therefore, that the lacquer-tree is capable of being cultivated in Europe, and it only remains to be seen whether the juice is affected by the changed conditions. The *Times* says that, to ascertain this, Prof. Rein has tapped the Frankfort trees, and has sent some of the juice to Japan, where it will be used by Japanese artists in lacquer work, who will report on its fitness for lacquering. In the meantime, some of the most eminent German chemists are analyzing samples of the juice taken from the trees at Frankfort, and samples of the juice sent from Japan; and should their reports and the reports from Japan be favourable, it is probable that the tree will be largely planted in public parks and other places in Germany. In course of time a skilled worker in lacquer would be brought over from Japan to teach a selected number of workmen the art of lacquering wood, and in this way it is hoped that a new art and craft may be introduced into Europe. Prof. Rein has been conferring with the authorities at Kew as to the results of his experiment.

The Hydrographic Department of the Admiralty has just published full details of the determinations of the latitudes and longitudes of six stations on the west coast of Africa—namely, Port Nolloth, Mossamedes, Benguela, St. Paul de Loanda, São Thomé, and Bonny. The observations were made in 1889 by Commander T. F. Pullen, R.N., and Mr. W. H. Finlay, under the direction of Dr. Gill, of the Cape Observatory. Whilst stationed at Bonny, Commander Pullen succumbed to malarial fever, and Dr. Gill has since taken charge of the reductions. The observations would not have been possible but for the courtesy of the officials of the Eastern and South African Telegraph Company, who placed their cables at the disposal of the observers.

Neptunia for July gives a description of the frigate *Scilla*, set apart by the Italian Government for the hydrographic exploration of the Mediterranean, and of its scientific fittings and instruments. By the end of September the *Scilla* was expected to be at work along the Italian possessions in the Red Sea, investigating the fauna and flora, and the temperature at different depths.

DR. A. ALCOCK, the Surgeon-Naturalist of the Marine Survey of India, is able to give a most favourable report of the work done in natural history on board the *Investigator* during the year ending March 1, 1891. The deep-sea researches made great progress. Not only has the work of collection been much more successful since the use of the reversible trawl and wire-rope, but the collections themselves are becoming better arranged; so that should it ever be decided to report upon them, group for group, in systematic detail, there will be abundance of material all sorted ready to the hand. Dr. Alcock is most anxious that such a report should at some time be undertaken; for apart from the Marine Survey of India nothing whatever, he thinks, is likely to be made known of the life of the depths of the Indian Seas, and of the physical and chemical characters of the deposits now being laid down on the bottom of those seas. Further, there are good reasons for supposing that an economic return would follow from the careful investigation of the little-known semi-bathybial fauna of Indian waters, and from a comparison between it and the semi-bathybial faunæ of the Mediterranean Sea on the one hand and the Japanese Seas on the other.

WE have received from Messrs. Philip and Son a new orrery for finding roughly the positions of the sun, moon, and planets for any hour of the year, and their times of rising, southing, and setting. In general appearance it resembles their well-known planisphere, but, in addition, it is provided with two index arms graduated in degrees of declination—one for the sun, and the other for the moon or planet. The operations are simple, but the instructions given scarcely do justice to the arrangements for carrying them out. An almanac is, of course, a necessary accompaniment to the orrery. We can recommend it to young students of astronomy.

A BOTANICAL Club for California has been instituted under the presidency of Dr. H. W. Harkness.

WE learn from the *Botanical Gazette* that Prof. J. M. Coulter has been spending the summer in studying the Cactaceæ of the borders of the United States and Mexico, under the direction of the Department of Agriculture at Washington; and that an expedition has been organized to investigate the flora of Mount Orizaba, Mexico, under the superintendence of Mr. H. E. Seaton.

A QUARTERLY Review of Geological Science in Italy will shortly appear at Rome, edited by Sigg. M. Cermenati and A. Tellini.

MR. CHARLES TODD, in his Report on the Rainfall in South Australia and the Northern Territory during 1890, says that without doubt "the feature" of the year was the extraordinary rainfall (especially in the first three months) over the eastern and north-eastern portions of the continent, which continued throughout the whole year, more or less, in New South Wales, and, whilst giving that colony the wettest year on record, caused some stations to register over 100 inches.

THE Pilot Chart of the North Atlantic Ocean for September states that the most important storm of the month was the hurricane that devastated the island of Martinique on the evening of the 18th, causing the loss of 378 lives. The storm seems to have been of comparatively small diameter, and it probably originated south-east of the island, which it passed directly over, on a west-north-west track towards San Domingo. It recurved over the eastern Bahamas, and thence moved north-east close to Bermuda; where at noon of the 27th the wind blew with hurricane force from north-north-west. The weather, the same as in this country, was unsettled and rainy over the North Atlantic generally, especially off the Atlantic coast of the United States, and a considerable amount of fog has been reported. A submarine earthquake was experienced at 10h. 30m. a.m. on August 23, in latitude 36° 44′ N., longitude 59° 47′ W., by the s.s. *Robert Harrowing*; Captain Hughson reports that a strange commotion of the sea increased until the decks were filled with water. At 1h. p.m. the sea suddenly fell calm.

COLORADO apparently intends to be well represented at the great Chicago Exhibition. Besides the mineral, agricultural, and educational exhibits, the flora and fauna of the State will be shown in great completeness. Already more than 1000 specimens of plants have been pressed; nearly 200 varieties of fruit have been duplicated in wax, and more than 2000 species of insects have been mounted.

RETURNS have been collected in Prussia, showing the extent to which buildings belonging to the State, or entitled to State subsidy for rebuilding or repair, were damaged by lightning from the year 1877 to 1886. The number of buildings to which the returns relate is 53,502. Of these, 264 were struck during the period in question, or about five for every 1000 buildings in ten years; and in 81 cases a fire resulted. The following facts, given originally in the *Reichsanzeiger*, are reproduced in the current number of the *Board of Trade Journal*:—Of the 264 buildings struck, 107 had towers, and in six cases only the tower escaped being struck. Of the total number of buildings struck, fifteen were fitted with conductors, and of these latter only one building escaped injury. In two cases the conductor was injured, and on one occasion the lightning passed from the conductor to an iron water pipe. In five cases they were so constructed as to be either dangerous or useless; in six cases they were not struck at all, being inadequate for the size of the building, from which it will be seen that conductors are a safeguard against lightning only when carefully constructed and repaired, and fitted in numbers according to the size of the building which it is intended to protect. The amount of injury wrought by lightning on the 53,502 houses was, on the whole, inconsiderable, being only 1,136,683 marks (£56,834), or 4306 marks (£215) for each casualty, or 21 marks (a guinea) per building in ten years, that is 2·1 marks (about 2*s.*) per building per annum.

SOME parts of Australia seem to be admirably suited for the growth of the olive. Mr. Principal Thompson, of Dookie, says in a recent report that 700 olive trees planted in that district are robust and healthy, and that they produce splendid oil. He strongly recommends the planting of the olive around vineyards and homesteads for shade and shelter, and to give a picturesque appearance to the rural home. Apart from the making of oil, he believes it would pay handsomely to grow olive berries to feed pigs alone. Last winter the pigs at Dookie (about 80 head) were allowed to eat up the fallen berries in the olive grove; they had no other food for upwards of two months, and throve amazingly, their skins having a peculiar shining appearance, characteristic of animals being well fed.

TOBACCO is being cultivated with much success in the German part of New Guinea, and is said to be better than the tobacco produced in Sumatra. It is expected that there will be a great increase in the amount grown during the coming year.

ACCORDING to M. d'Amagher, the Russian correspondent of the *Monde Economique*, a central Agricultural Institute is to be established in Russia. It will include several sections—agricultural, geological, meteorological, botanical, chemical, and technological; and branches will be formed in the provinces.

UNUSUALLY fine atmospheric effects were produced by the clear weather of the Mediterranean during the month of July. According to the *Mediterranean Naturalist*, the new monthly periodical issued in Malta, the phenomenon of irregular diffraction was especially shown by the raising of the line of sight to such an extent that objects at great distances, at other times completely concealed from view, were apparently raised so much above their true position as to be clearly discernible from the shores of Malta and Gozo. The cliffs of the coast-line, and the undulatory contour of the mountains of Sicily, were to be seen distinctly with the naked eye on July 11 and 12, while the outlines of Etna stood boldly out against the clear azure sky. Although more than 100 miles away, the form of the mountain was perfectly recognizable.

THE honey of the Malta bees has long been noted both for its purity and for its delicious flavour. A writer in the *Mediterranean Naturalist* says the flavour is largely due to the extensive crops of sulla (clover) that are annually raised throughout the islands, from which the bees derive the largest proportion of their material. It is estimated that to collect one pound of honey from clover, 62,000 heads of clover must be deprived of nectar, and 3,750,000 visits must be made by the bees.

SOME excellent directions for the collection, preparation, and preservation of birds' eggs and nests have been put together by Mr. C. Bendire, and published by the United States Nationa Museum. He begins his counsels by telling the would-be collector that unless he intends to make an especial study of oology, and has a higher aim than the mere desire to take and accumulate as large a number of specimens as possible regardless of their proper identification, he had better leave nests and eggs alone. The mere accumulation of specimens, Mr. Bendire points out, is the least important object of the true oologist. The principal aim of the collector should be to make careful observations on the habits, call-notes, song, the character of the food, mode and length of incubation, and the actions of the species generally from the beginning of the mating season to the time the young are able to leave the nest.

AT one of the meetings of the Wellington Philosophical Society in 1885, Sir Walter Buller, F.R.S., exhibited a series of the so-called wandering albatross, and expressed his belief that there were two species under the common name of *Diomedea exulans*, one of them being highly variable in plumage, and the other distinguished by its larger size and by the constancy of its white head and neck. But, although that was his conviction, he did not feel justified in setting up the new species and giving it a distinctive name until he could produce incontestable evidence of its existence. From a paper read by him before the same Society in February last, and published in the new volume of the Transactions of the New Zealand Institute, we learn that he had lately had an opportunity of examining sixteen beautiful specimens of both sexes and of all ages, and that as the result of his study of these specimens he had no hesitation in speaking of a new species. "It is undoubtedly," he says, "the noblest member of this group, both as to size and beauty, and I have therefore named it *Diomedea regia*." He exhibited before the Wellington Society a series of both species, and in the course of some remarks on them stated that they keep quite apart from one another on their breeding-grounds, and do not commingle "except when sailing and soaring over the mighty deep, where a community of interest and a common pursuit bring many members of this great family together."

IN the paper in which he deals with the species called by him *Diomedea regia*, Sir Walter Buller refers to a remarkable characteristic of the wandering albatross—a characteristic which has been carefully studied by Mr. Harris. At a certain time of the year, between February and June—Mr. Harris cannot exactly say when—the old birds leave their young and go to sea, and do not return until October, when they arrive in large numbers. During their absence the young birds never leave the breeding-ground. Immediately after the return of the old birds, each pair goes to its old nest, and, after a little fondling of the young one, turns it out, and prepares the nest for the next brood. The deserted young ones are in good condition, and very lively, frequently being seen off their nests exercising their wings; and, when the old birds come back, a young bird will often remain outside the nest and nibble at the head of the old one, until the feathers between the beak and the eye are removed, and the skin made quite sore. The young birds do not go far from land until the following year, when they accompany the old ones to sea. When the young are left in the nest at the close of the breeding-season, they are so immensely fat that Sir Walter Buller thinks they can subsist for months without food of any kind. Captain Fairchild has described to Sir Walter from personal observation the coming home of the wandering albatross, and the peremptory manner in which the young bird in possession is ordered to quit the nest, so as to make room for its successor.

THE habits of the kingfisher (*Halcyon vagans*) formed the subject of an interesting paper read some time ago by Mr. J. W. Hall before the Auckland Institute, and now printed in the Institute's Proceedings. He raised the question, Is it customary for the kingfisher to capture live birds? Last winter he saw one with a live white-eye in its mouth. The tree the kingfisher was perched upon was not many yards distant from him, and he distinctly saw the little wings flutter convulsively as the kingfisher was preparing to beat its prey against the branch. So it could not have been a dead bird casually picked up. Perhaps this, he said, was an application of the *lex talionis*, for, besides being mercilessly persecuted by the small boys with their catapults, the kingfisher was not infrequently captured by the common hawk. But sometimes the hawk does not come off best. One day at Parawai (Thames) a hawk sailed round the bend of a hill, followed (accidentally, he supposed) by a kingfisher. There at once arose a great outcry, and the hawk came again in sight, bearing the kingfisher in its talons. But, nothing daunted, the kingfisher with its pickaxe of a bill pegged away at the breast and abdomen of its captor to such good effect that the hawk was glad to liberate its prey, whereupon the kingfisher flew away apparently but little the worse for the encounter, and carrying with it, he need hardly say, the full sympathy of the onlookers. A friend of the author had seen a kingfisher dive under water to escape the pursuit of a hawk.

MR. J. CRAWFORD, State Geologist and Mineralogist of Nicaragua, visited in 1888 the Amerrique Indians, from whose ancestral name "America" may have been derived; and he has lately submitted to the Boston Society of Natural History some interesting notes about them. They occupy a hilly region in the gold-mining part of the district of La Libertad, Nicaragua, where there are "true fissures," each containing gold in sufficient quantities to give profits to the mine and mill owners now "operating" them. A few melted masses of gold, weighing from half an ounce to two ounces each, pierced with holes, and in form supposed to have been made and used as ornaments before the Spanish occupation, have been discovered in the district; and Mr. Crawford regards it as a fair inference that the Amerrique Indians who dwelt in that part of Nicaragua at the time of its discovery by Columbus, September 1502, picked up and occasionally mined, melted, and used gold for sacred or ornamental purposes. The Amerrique Indians are usually well formed, 6 feet 6 inches to 6 feet 8 inches tall; and they are active, and appear to be strong and healthy. Nevertheless, they are dying out rapidly. Probably not more than 275 or 300 of them are now living. They live in dim pathless forests, and their occupation is to find in the woods various species of trees (*Siphonia*, *Castilloa*, &c.) They deeply scarify these, collect the exuding emulsion, and separate the contained elastic ("India") rubber; and this "India" rubber they carry on their backs more than 100 miles to sell to merchants in Rama or at the mouth of Rio Matagalpa. They have cleared some patches of ground, and plant corn by making holes in the soil with pointed sticks. They believe that with allied tribes they had in very ancient times a mighty prophet or cacique, who appeared suddenly, full grown, in their territory, and that to him many tribes of Indians gave allegiance. The impalpable form of this ancient chief has been seen by very old Indians proudly walking and gesticulating on the top of Mesa Totumbla. He is buried in, or returns by day to, a deep cavern in this Mesa (a mass of gneiss); and he indicates, by gestures, that he will one day collect the Indians into a great army, and lead them in person to many victories. Mr. Crawford found his way into the cavern, and discovered in it three crania of Indians with other bones of their bodies. These were sent in 1889 to the Paris Exhibition, and were afterwards transferred to the U.S. National Museum. A few crude beads or ornaments, evidently earlier than the Spanish occupation of Nicaragua, were also found.

THE following are the arrangements for lectures during October at the Royal Victoria Hall:—October 6, Prof. T. Hudson Beare, the steam-engine, with experiments; October 13, Rev. Canon Browne, the invasion of England and Battle of Hastings, with illustrations from Bayeux tapestry; October 20, Mr. J. R. Green, flowers and their helpers; and October 27, Rev. E. Hill, the Channel Islands.

AT a meeting held last year by the students of the Kindergarten department of the New York College for the Training of Teachers, various papers were read on the principles and methods of the Kindergarten. These papers have now been issued as one of the educational monographs of the New York College. Miss A. Brooks, who contributes an introduction, says the School Board of New York City is considering plans for the introduction of the Kindergarten system into its schools; and a movement begun by the New York Kindergarten Association is destined, she thinks, "to accomplish great things for the neglected children of the city."

"EGYPTIAN SCIENCE," by N. E. Johnson, is the title of a work which will shortly be published by Messrs. Griffith, Farran, and Co.

THE Durham College of Science, Newcastle-upon-Tyne, has issued its Calendar for the session of 1891–92. This College represents the faculties of science and engineering in the University of Durham, and thus constitutes an important portion of the University of the north of England. But it does not restrict its work to science and engineering; it fulfils all the functions of a University College.

THE following works will shortly be published by Messrs. Crosby Lockwood and Son:—"The Mechanical Engineer's Pocket-book of Tables, Formulæ, Rules, and Data," a handy book of reference for daily use in engineering practice, by D. Kinnear Clark; "The Metallurgy of Argentiferous Lead," a practical treatise on the smelting of silver-lead ores, and the refining of lead bullion, including reports on various smelting establishments, and descriptions of modern furnaces and plants in Europe and America, by M. Eissler; "Engineering Chemistry," a practical treatise for the use of analytical chemists, engineers, iron masters, iron founders, students, and others, comprising methods of analysis and valuation of the principal materials used in engineering work, with numerous analyses, examples, and suggestions, by H. Joshua Phillips; "A Handbook of Brewing," a practical treatise for the use of brewers and their pupils, by Herbert Edwards Wright; "Condensed Machines," a selection of formulæ, rules, tables, and data, for the use of engineering students, science classes, &c., in accordance with the requirements of the Science and Art Department, by W. G. Crawford Hughes; "Milling," a treatise on machines, appliances, and processes employed in the shaping of metals by rotary cutters, including information on making and grinding the cutters, by Paul N. Hasluck, with upwards of 300 engravings; "Star Groups," a student's guide to the constellations, by J. Ellard Gore, with thirty maps; "Lessons in Commerce," by Prof. R. Gambaro, of the Royal High Commercial School of Genoa, edited and revised by James Gault, Professor of Commerce and Commercial Law in King's College, London.

AMONG the books announced by Messrs. George Philip and Son are the following:—"Delagoa Bay: its Natives and Natural History," by Rose Monteiro, with 20 original illustrations, after the author's sketches and from the natural objects, by A. B. and E. C. Woodward; "Paraguay: its History, Commerce, and Resources," by Dr. E. Bourgade, with 13 illustrations and a large coloured map; "Makers of Modern Thought," by D. Nasmith, Q.C.; "The Teacher's Hand-book of Slöjd," as practised and taught at Nääs, by Otto Salomon, Director of the Nääs Seminarium, with over 130 illustrations; "Hughes's Class-book of Modern Geography," an entirely new and completely revised edition, much enlarged by J. Francon Williams; "Geography of the British Colonies and Foreign Possessions," by the Rev. J. P. Faunthorpe, new and revised edition; "Systematic Atlas," for higher school and general use, a series of physical and political maps of all the countries of the world, with diagrams and illustrations of astronomical and physical geography, specially drawn by E. G. Ravenstein; "The Handy Volume Atlas of Astronomy," a series of 72 plates, with notes and index, by Sir Robert Stawell Ball, F.R.S.; "The Handy Volume Atlas of London," a series of 64 maps, with notes, compendium, directory, and complete index; "Atlas of Modern Geography," new and enlarged edition.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*) from India, presented respectively by Mr. G. H. Sasse and Mrs. Gregorey; two Sykes's Monkeys (*Cercopithecus albigularis*) from East Africa, presented by Mr. F. Pardage; one Mozambique Monkey (*Cercopithecus rufo viridis*), one Garnett's Galago (*Galago garnetti*) from East Africa, one Blotched Genet (*Genetta tigrina*), one Ostrich (*Struthio camelus*) from East Central Africa, presented by Mr. Freith Anstruther; one Coypu (*Myopotamus coypus*) from South America, presented by Mr. Spencer H. Curtis; one Golden Eagle (*Aquila chrysaëtus*), European, presented by Mr. Herbert Bray; one —— Sand Grouse (*Pterocles* ——) from South Africa, presented by Mr. Max Michaelis; two Trocary Pigeons (*Columba trocary*) from Madeira, received from Dr. F. J. Hicks; one —— Elaps (*Elaps* ——) from Australia, presented by Mr. E. H. Meek;

one Rhomb-amarked Snake (*Psammophis rhombeatus*), four Crossed Snakes (*Psammophis crucifer*), one Hygian Snake (*Elaps hygiæ*), two —— Snakes (*Dasypeltis scabra*) from South Africa, presented by Messrs. Herbert Melville and Claude Beddington; one Smooth Snake (*Coronella lævis*), two Common Snakes (*Tropidonotus natrix*) from Oxfordshire, presented by Mr. A. W. S. Fisher; one Otter (*Lutra vulgaris*) from South Wales, received in exchange; two White-tailed Sea Eagles (*Haliaëtus albicillay's*) from Norway, three Indian Python (*Python molurus*) from India, deposited; one Macaque Monkey (*Macacus cynomolgus*) from India, one Pardine Genet (*Genetta pardina*) from West Africa, purchased; one Vinaceous Turtle Dove (*Turtur vinaceus*), bred in the Menagerie.

OUR ASTRONOMICAL COLUMN.

LIGHTNING SPECTRA.—Mr. W. E. Wood, of Washington, has continued his observations of lightning spectra for the purpose of determining the origin of some of the lines previously recorded by him (NATURE, vol. xlii. p. 377). The result is that he is now able to say, in the *Sidereal Messenger* for August:—"Lightning spectra present but the characteristic lines of oxygen, hydrogen, nitrogen, and carbonic acid, and—what was puzzling to me—the line of the vapour of sodium. The absorption bands which I find in lightning spectra I think might be produced by the moisture in the air, a large quantity being present during thunderstorms." It is suggested that the sodium line owes its presence to the existence of meteoritic *débris* in the atmosphere.

A NEW ASTEROID.—The 315th asteroid was discovered by Charlois on September 1.

THE INTERNATIONAL GEOLOGICAL CONGRESS: WASHINGTON MEETING.

THE fifth meeting of the International Geological Congress, being the first ever held in America, was held at the Columbian University, Washington, from August 26 to September 1, with an attendance of sixty or seventy foreigners, from Austria-Hungary, Canada, Chili, France, Germany, Great Britain, Mexico, Peru, Roumania, Russia, Sweden, and Switzerland, and about two hundred members from the United States. The papers and discussions were generally in English, though French and German were to some extent spoken. French has been the language of all the previous Congresses.

Profs. James Hall and James D. Dana were elected Honorary Presidents, and J. S. Newbery Acting President. Owing to the absence of the latter, the chair was filled in turn by several of the Vice-Presidents.

FIRST DAY.—After the election of officers, as nominated by the bureau, Prof. Joseph Le Conte, as senior Vice-President, took the chair, and delivered the opening address, in which he said that the idea of an International Congress was born in America in 1876. Previous meetings have been held at Paris in 1878, Bologna in 1881, Berlin in 1885, and London in 1888. He briefly stated the purposes of this Congress, which were afterwards carried out—namely, to discuss classification of the Pleistocene rocks, of correlation, and of map notation. He compared the maps of Europe and America, showing the complexity of the former and the simplicity of the latter. He then considered some points in American geology:—(1) The general continuity of the record. (2) The prevalence of extensive faults, ranging from 100 to 2000 feet, and extending over great distances. (3) Peculiarities of mountain structure. Prof. Gilbert has discovered a new type of mountains formed by uptilted strata. The Sierra Nevada is an illustration. (4) Extensive lava floods, covering areas from 10,000 to 100,000 square miles in extent, and from 2000 to 4000 feet deep. No such floods are found elsewhere. Those of India are the nearest approximation; but in Europe the lava beds are small and much cut up. (5) The great continental movement, commencing in the later Tertiary, and terminating in the beginning of the Quaternary, which has caused changes of level amounting to 2500 or 3000 feet on both sides of the continent. (6) The ice-sheet of the glacial epoch was first and most completely demonstrated in America.

Other addresses were delivered by Mr. Hubbard, Chairman of the Local Committee; Mr. Noble, Secretary of the Interior, who has official control and supervision of the Geological Survey of the United States; Prof. Hughes of England, Prof. Gaudry of France, and Major Powell, Chief of the Geological Survey.

SECOND DAY.—The entire day was occupied by a discussion on classification of the glacial Pleistocene deposits. Prof. T. C. Chamberlin opened the discussion by stating that classification might be made on three grounds: (1) structural; (2) chronological; (3) genetic. The first was very easy, being an obvious division into assorted and unassorted drift. The second was extremely difficult, and could not be accurately made till after a full determination of the third. He accordingly proposed the following general classes: (1) formations produced by the direct action of Pleistocene glaciers; (2) formations produced by the combined action of Pleistocene glaciers and accompanying glacial drainage; (3) formations produced by glacial waters after their issuance from Pleistocene glaciers; (4) formations produced by floating ice derived from Pleistocene glaciers; (5) formations produced by shore ice and ice floes due to low Pleistocene temperature, but independent of glacier action; (6) formations produced by winds acting on Pleistocene glacial and glacio-fluvial deposits under the peculiar conditions of glaciation.

This paper was discussed very thoroughly. Prof. T. McK. Hughes pointed out that the classification suggested by Prof. Chamberlin was purely genetic. He then explained the abundance of striated boulders in one part of the glacial deposits and their absence in another. If the supply of material (that is, of rock bosses above the ice) ceases at any point, then all the boulders will gradually sink through the ice and become glaciated at the bottom. Prof. Hughes also thought that two distinct types of ridges formed of glacial material were confused under the names—kames, osars, and eskar. He also explained the "pitted plains" as due to an unusual interruption between the hills or ridges of eskar character. He expressed his opinion that the glacial period was a continuous one, in England at least, except for slight changes due to unimportant oscillations.

Mr. McGee mentioned the importance of land forms in interpreting geological processes. Any primary geological classification must be genetic. He discussed in detail the following scheme of classification of Pleistocene deposits:—

Classification of Pleistocene Formations and Land Forms.

A. Aqueous:
  1. Below base level.
    a. Marine.
    b. Estuarine.
    c. Lacustral.
  2. At base level.
    a. Littoral.
    b. Marsh.
    c. Alluvial (certain terraces, &c.).
  3. Above base level.
    a. Torrential.
    b. Talus (including playas).
B. Glacial:
  1. Direct (Chamberlin's Class I.).
  2. Indirect (Chamberlin's Classes II. to V., in part).
C. Aqueo-Glacial (Chamberlin's Classes II. to V., in part).
D. Eolic (Chamberlin's Class (?) VI.).
E. Volcanic:
  1. Direct.
    a. Lava sheets.
    b. Cinder cones.
    c. Tuffs, lapilli sheets, &c.
  2. Indirect.
    a. Ash beds.
    b. Lapilli sheets.

Prof. Chamberlin, in closing the discussion, said that there was great difficulty in applying a chronological classification, and that such a classification might even act as a barrier to observation and to the recognition of the truth. Chronological classification is the ultimate goal of glacial studies, but it is something for which we are not as yet prepared. Red, oxidized sub-soils are not developed in northern latitudes. Organic deposits between glacial layers are abundant in the West, but do not belong to a single horizon. Many facts of erosion and physical geology indicate that the Glacial epoch in America was widely differentiated and of long duration. How many distinct periods it embraced we do not as yet know.

Prof. Cope said an abundant tropical fauna is found in the "Equus beds," which, if they be of interglacial age, indicates at this time a very warm climate. This fauna is succeeded by a truly boreal fauna. In this is contained material for a chronological subdivision of Pleistocene deposits.

THIRD DAY.—The President announced as the subject for discussion, the correlation of geological formations.

Mr. Gilbert opened this discussion by presenting a general classification of methods of correlation.

Strata are locally classified by superposition in chronologic sequences. Geologic correlation is the chronology of beds not in visible sequence. For convenience in discussion, methods of correlation are classed in ten groups, of which six are physical and four biotic.

Physical Methods of Correlation.

(1) Through visible continuity. The outcrop of a bed is traced from point to point, and the different parts are thus correlated one with another.

(2) Strata are correlated on account of lithologic similarity. This method, once widely prevalent, is used where the distances are small.

(3) Correlation by the similarity of lithologic sequence has great and important use where the localities compared fall within the same geologic province, but is not safely used in passing from province to province.

(4) Physical breaks, or unconformities, have a limited use, especially in conjunction with other methods. The practice of employing them in the case of localities wide apart is viewed with suspicion.

(5) Deposits are also correlated with their simultaneous relations to some physical event—for example, a beach with the lake beds it encircles; a base level plane with a contiguous subaqueous deposit; and alluvial, littoral, and subaqueous deposits standing in proper topographic relation. In the Pleistocene, glacial deposits are widely correlated with reference to a climatic episode assumed to arise from some general cause.

(6) Deposits are correlated through comparison of changes they have experienced from geologic processes supposed to be continuous. Newer and older drift deposits in different regions are correlated according to the relative extent of weathering and erosion; induration and metamorphism afford presumptive evidence of age, but yield to evidence of other character. Metamorphism holds prominent place in the correlation of pre-Cambrian rocks where most methods are inapplicable.

These physical methods are qualified by the geographic distribution of geologic processes of change and of geologic climates.

Biotic Methods of Correlation.

(7) A newly-discovered fauna or flora is compared with a standard series of faunas and floras by means of the species it holds in common with them severally.

(8) It is also compared by means of representative forms, or through genera and families.

(7*a*) and (8*a*) These comparisons are strengthened if two or more faunas in sequence are found to be systematically related to the faunas of a standard series.

(9) Two faunas or floras otherwise related are compared in age through their relation to the present life of their localities. This method was applied by Lyell to Tertiary rocks.

(10) Faunas are correlated by means of their relation to climatic episodes taken in connection with station. For example, boreal shells found in latitudes below their present range are referred to glacial time.

In general the limitations to accurate correlation by biotic methods arise from the facts of geographic distribution. Correlations at short range are better than those at long range.

Biotic correlation by means of fossils of different kinds may have different value. In general, the value of a species for the purposes of correlation is inversely as its range in time, and directly as its range in space. The value of a biotic group depends (1) on the range of its species in time and space; (2) on the extent to which its representatives are preserved.

Prof. K. von Zittel spoke in reference to the biotic methods, and gave his opinion of the relative value of plants and animals for purposes of correlation. He regarded plants as relatively unimportant. Among animals, those which are marine, lacustrine, and land animals may be distinguished. Of these classes marine invertebrates are most valuable for purposes of correlation. The vertebrates change rapidly, but are frequently altogether wanting. For instance, no vertebrates occur in the Alpine beds corresponding in age to those which contain the mammalian fauna of the Paris basin. In certain lacustrine deposits invertebrates may be absent, and in such cases the vertebrate fauna is the surest guide.

Baron de Geer emphasized the importance of a numerical comparison between different species. The actual counting of individuals in a given formation is of great value.

Prof. Marsh expressed his agreement in general with the conclusions communicated by Prof. von Zittel, but would give special weight to vertebrate fossils. In the Mesozoic and Tertiary beds of the Rocky Mountains he had found that the vertebrates offer the surest guide for correlation. This is in part because invertebrates are either wanting or are lacustrine. Prof. Marsh in 1877 named a sequence of horizons after the most characteristic vertebrate genus in each which is confined exclusively to it. He presented an outline of such classification brought down to date, with a section to illustrate vertebrate life in America.

Mr. C. D. Walcott spoke of the value of plants for purposes of geologic correlation.

Prof. T. McK. Hughes spoke of the present and growing tendency towards a natural classification. The evidence is complex, and includes a considerable variety of diverse relations. He pointed out exceptions to the normal conclusions deduced from superposition, lithological character, and similarity of sequence. We must have a system of criteria so varied that if one or more fails others can be employed. All classes of evidence are useful, both positive, negative, and circumstantial.

Major J. W. Powell spoke of the necessity of specialization on the part of geologists engaged in the work of correlation. The evidence derived from physical and biotic facts might apparently disagree. But that a satisfactory result may be reached, these two classes of evidence must be brought into harmony. He cited an example from his own experience, of how an identification of synchronous formations might be made over a wide area through a union of physical and biotic methods.

Mr. W. J. McGee remarked that in the coastal plain of the United States physical correlation alone is employed. The bases accord with those outlined by Mr. Gilbert, with certain minor modifications and an important addition, as follows:—

For local discrimination and correlation	Visible continuity; Lithologic similarity; Similarity of sequence.
For correlation throughout the province... ...	Physical breaks viewed as indices of geography and topography.
For correlation with contiguous provinces ...	Relation to physical events, including continental movements, transportation of materials, land sculpture, &c.
For general correlation...	Homogeny or identity of origin.

By correlation upon these bases the physical history of a considerable fraction of the continent may be so definitely ascertained as to permit fairly accurate mapping of the geography, and even the topography of each episode in continent growth. After these episodes are clearly defined, and the fossils found in the formations are studied, it will be possible definitely to ascertain the geographic distribution of organisms during each episode; then palæontology may be placed on a new and higher plane.

Prof. W. M. Davis showed that it was possible to decipher geological history not only through the records of deposition, but also by processes of degradation. As an example of this method he explained a topographical section from the city of New York westward. In this we have evidence of the existence of an ancient *peneplain*, or base-level lowland of Cretaceous age. This surface was subsequently elevated (more toward the west than toward the east) at the end of Cretaceous, or at the beginning of Tertiary time. It has since been dissected by the excavation of more recent valleys. The Hudson Valley lowland was cited as an example of this recent dissection.

Prof. E. W. Claypole considered that the different methods of geologic correlation differed very greatly in their value. It is improbable that the plant or mammalian record will ever equal in its perfection that of the marine invertebrate fauna. The marine fauna is to the geologist what a primary triangulation is to the geodesist. It marks out the main divisions, which are subsequently further subdivided through the aid of other fossils, such as plants and vertebrates.

Prof. C. R. Van Hise spoke of the methods of correlation employed for pre-Cambrian rocks, which occur in widely separated areas and are devoid of fossils. Physical data only are available for correlating these formations. Experience has shown that, among all physical methods, unconformity is by far the most important. Other physical criteria, such as the degree of induration, metamorphism, and relation to eruptives, are valuable for the subdivision of single areas, but cannot be safely used in identifying synchronous formations in widely-separated areas. The idea that lithological character is any direct proof of geological age has retarded the scientific subdivision of pre-Cambrian rocks. The researches of Pumpelly and others in the eastern United States have demonstrated that Silurian, Devonian, and even Carboniferous deposits might become, under certain physical conditions, as highly crystalline as much more ancient rocks of the West. For this reason it has been found necessary to abandon such terms as *Huronian* and *Keweenawan*. Evidences of life are not lacking in pre-Cambrian rocks, and it is to be hoped that the palæontologist will succeed in differentiating several separate formations below the Cambrian, as the Cambrian itself was differentiated from the base of the Silurian.

FOURTH DAY.—Prof. E. W. Hilgard laid stress upon the importance of the abundance or scarcity of species in the correlation of strata. He thinks some quantitative estimation of the species should be made. He is of the opinion, also, that, as compared with marine fauna, plants have but little value for purposes of correlation owing to their local distribution, their accidental proximity to water, transportation, and preservation. Plants can be so used only after large areas are worked over.

Prof. Lester F. Ward continued the discussion. He developed two of the more general principles of correlation by means of fossil plants, as follows :—

(1) That the great types of vegetation are characteristic of the great epochs in geology.

This principle is applicable in comparing deposits of widely different age when the stratigraphy is indecisive. For example, even a small fragment of a Carboniferous plant proves conclusively that the rocks in which it occurs are palæozoic, or a single dicotyledonous leaf proves that they must be as late as the Cretaceous.

(2) That for deposits not thus widely different in age, as, for example, within the same geologic system or series, ample material is necessary to fix their position by means of fossil plants.

Neglecting this principle has led to the greater part of the mistakes of palæobotanists, and has done most to bring palæobotany into disrepute. Geologists have expected too much of them, and they, in turn, have done violence to the truth in attempting to satisfy extravagant demands. On the other hand, where the material is ample, fossil plants have often corrected the mistakes of stratigraphical geologists, and solved problems concerning geologic age, which seemed impossible of settlement by any other class of evidence.

Prof. Henry S. Williams laid stress upon the relations of species to the conditions of deposition. The abundance of a species varies with environment, and a study of correlation should embrace a study of these conditions. Sandstones deposited near shore may have a fauna different from that of a limestone deposited off shore at the same time, and a change of fauna may be induced by a change of the conditions of deposition. The age of beds should be determined by comparing species of the same genera rather than by comparing those of different genera. There are centres of abundance which exhibit great variability in their characters; outside of these centres the species exhibit varieties which may be called extralimital, and which are not typical though they have often been published as types.

Prof. Charles Barrois said that there was no general basis, either biologic or lithologic, for the correlation of the pre-Cambrian rocks of Europe with those of North America ; even the terms applied to these rocks were liable to be misunderstood. Certainly the divisions used in France cannot be correlated with those now used in the United States. General correlation cannot, as yet, be based upon nonconformities ; autopsy is the only basis upon which a comparison can be instituted. He pointed out certain parallelisms between the histories of the crystalline schists of America, as illustrated by Mr. Pumpelly, and the gneissic rocks of Brest, where the Cambrian slates are altered to gneisses of Archæan aspect, while the alternating fossiliferous quartzites are changed to crystalline quartz. Geologists must see the beds together in order to reach a common understanding of the crystalline rocks.

Prof. E. D. Cope discussed the question from a general point of view with especial reference to the value of vertebrates for purposes of correlation, particularly for inter-continental correlation. He pointed out that there is a marked difference in the present verebrate faunas of continents, and that the variation of such forms must be sought in vertical rather than in horizontal ranges. Such study shows that we have had invasions of a given region by a fauna from without ; for example, a South American fauna invaded North America at one time and then retreated, while a North American fauna once invaded South America, and traces of it still remain in that country. He is inclined to believe that certain vertebrate forms did not spread over the earth from a single place of origin, but that they originated at different places upon the earth. We have parallelism in separate places, but the parallelism is defective in the Laramie.

Mr. G. K. Gilbert was of the opinion that many methods of correlation must be used. He doubted the trustworthiness of the correlation of non-fossiliferous rocks by comparative change, even locally. He thought the abundance and scarcity of fossil forms comparable with lithologic differences, and considered the simple occurrence of a species as valuable for purposes of correlation as its abundance.

FIFTH DAY.—Subject for discussion : map-colouring and cartography.

Major J. W. Powell exhibited charts illustrating the colour system used by the U.S. Geological Survey, explained the methods of using the colours, and gave the reasons for them. The colours assigned to rocks of different ages are as follows :—

	Period.	Period colour.	Mark.
1.	Neocene	Orange	N.
2	Eocene	Yellow	E.
3.	Cretaceous	Yellow-green	K.
4.	Jura-Trias	Blue-green	J.
5.	Carboniferous	Blue	C.
6.	Devonian	Violet	D.
7.	Silurian	Purple	S.
8.	Cambrian	Pink	Ꞓ.
9.	Algonkian	Red	A.

The colours are used to designate geologic periods, patterns of these colours designate formations ; minor divisions are usually relegated to the text. The number of patterns for designating formations can be indefinitely enlarged, but follow a definite system.

Mr. Joseph Willcox showed that in the scheme described by Major Powell the colours were not evenly distributed through the chromatic scale.

Prof. C. R. Van Hise pointed out that Archæan rocks are shown by a brown underprint, and that metamorphic rocks of known age are given the colour of the corresponding unaltered rocks.

Major Powell explained that it was not attempted to select colours equally distributed through the chromatic scale, but to use those that may be most readily recognized.

Mr. H. M. Cadell asked why black and gray were not used.

Major Powell replied that blue was used in place of the dark shades for the Carboniferous ; that dark colours are misleading in regard to the occurrence of coal, which occurs in the Cretaceous and Tertiary as well as in the Carboniferous.

Mr. Christie found the black colour very inconvenient, because it often made the details of the map covered by such colours illegible.

Mr. H. M. Cadell said that the maps of the Geological Survey of Great Britain were coloured by hand, and that the system used by the U.S. Geological Survey could not for this reason be economically employed.

Major Powell explained that the U.S. Survey system is very economical when the colour patterns are transferred to stones.

Prof. T. McK. Hughes thought it very difficult to devise a scheme that will meet the demands of everyone. Some reference must be had to the permanence of the colours, the readiness with which they can be applied, and the distinctness with which they show what is desired. He thinks the fittest scheme must survive.

In the afternoon, brief lectures were given by Prof. Chamberlin, Mr. Gilbert, Major Powell, and Mr. Emmons upon the geology of the country to be traversed by the long excursion.

SIXTH DAY.—A Committee on International Bibliography was appointed.

The Secretary announced that Messrs. Golier and Schmidt convey an invitation from the Swiss Government to hold the sixth International Congress, in 1894, in Switzerland. Mr. Golier delivered an address in which he presented the invitation, and the Congress unanimously accepted it. The following Swiss members were appointed a local committee, with power to add to their number and to appoint the time and place of meeting: viz. Messrs. Heim, Renevier, Lang, Balzer, Schmidt, and Golier. On the motion of Prof. Pumpelly, a vote of thanks was passed to the Swiss Government and delegation. It is thought that Berne will be selected as the place of meeting.

The Geological Survey of Russia sent an invitation to hold the seventh Congress in Russia. The Czar joined in the invitation. Prof. Tschernychew made the formal presentation of the subject to the Congress. A vote of thanks to the Survey and the Czar was passed, and the Secretary of the Congress was authorized to send a despatch by cable, transmitting the vote.

The President of the Congress, Prof. Le Conte, delivered a brief closing address, summarizing the work of the session, and after passing several votes of thanks the Congress adjourned.

THE SOCIETY OF FRIENDS OF ASTRONOMY AND COSMIC PHYSICS.

THE Society of Friends of Astronomy and Cosmic Physics, founded May 19, 1891, has been formed with a view to the organization of systematic activity and co-operation in research in the subjects named. It is intended to embrace, chiefly, workers in astronomical science in Germany, Austro-Hungary, Switzerland, and other neighbouring countries, and natives of these countries in the colonies and elsewhere. Members of other nationalities are, however, offered a welcome.

The head centre of the Society is Berlin. The subscription is 5 marks.

Communications are invited from individual members, which will be published together with the notices of meetings and other business of the Society. These publications will bear the title "Mittheilungen der Vereinigung von Freunden der Astronomie und kosmischen Physik"; they will be numbered consecutively, and will be supplied to all members gratis, but will not be issued at regular or stated intervals.

These communications will form at present the only direct publication of the Society, until it is formed on a more substantial financial basis and consists of a larger number of members (in the first four weeks the number rose from 50 to over 100). Contemporaries are at liberty to borrow any matters of interest contained in the Society's communications, of course acknowledging the source from which they are derived.

Endeavours will be made to keep the Society carefully within the limits in which alone it can be successfully active, leaving on one side other closely related branches: for instance, those of the Meteorological and Photographic Societies; but, nevertheless, endeavouring to preserve the closest amity and co-operation with the related Societies.

The Astronomische Gesellschaft, founded in Germany in 1863, is regarded by the new Society as the principal Society, whose office it is to foster astronomical research throughout the whole earth. The new Society bears the same relation to this international association as do those Astronomical Societies already established in England, France, Russia, and North America.

The principal object of these smaller societies is to collect observations made in the largest possible number of districts, inasmuch as researches in astronomy and cosmic physics are very largely dependent on the state of the weather, and the relation of the place of observation to the phenomena in the heavens.

In the new Society the following branches of work have been selected:—(1) Observations of the sun; (2) of the moon and surface of the planets; (3) of the intensity and colour of the light of the stars and of the Milky Way; (4) of the zodiacal light and meteors; (5) of the polar light, magnetism of the earth, earth currents, and air electricity; (6) of the clouds and halos, and thunder and lightning (care being taken in the two last groups not to encroach upon the ground already covered by the Meteorological and Photographic Societies).

Each of these groups is presided over by a member of the Society whose attention is especially directed to the respective subject. The duty of these Presidents is to organize the correspondence, hold branch meetings, and preserve the connection which binds each group to all the others.

The Society will endeavour to further the organization of all these researches, not merely by the publication of communications and by correspondence, but also by advice and aid in the providing of apparatus, especially of suitable optic, electric, and magnetic measuring instruments, charts, books, &c.

The statutes of the Society will be sent post free on application to the Secretary, Herrn Cand. G. Witt, Berlin, N.W., Invalidenstrasse 57.

The President of the Society is at present Prof. Dr. R. Lehmann-Filhés, Berlin, W., Wichmannstrasse 11*a*.

The Committee consists of the six members presiding over the several groups of research.

The Librarian of the Society is Herr Dr. P. Schwahn, Berlin, N.W., Invalidenstrasse 57; and the Treasurer, to whom subscriptions should be sent, Herr Rendant Bruck, Berlin, N.W., Invalidenstrasse 57.

THE PROTECTIVE DEVICE OF AN ANNELID.

AMONGST a gathering of small Serpulids, &c., received from Mr. Sinel, of Jersey, I find some interesting little worms related to the Sabellidæ. They build a thin membrane-like tube, about one-seventieth of an inch in diameter, coated externally with flat translucent particles of sand. Its lower end is closed, and embedded in sponge or other growths, but the upper end is free, and, when the head of the inmate is protruded, stands about a quarter of an inch high in the water. On this head are two branchial tufts, each having five branches beset with a double row of long ciliated filaments. When all are fully expanded they curve backwards, and cover an area of about one-tenth of an inch in diameter. The branches decrease in size from the inner to the outer pairs, and at the back of the longest but one in each tuft, near its base, is a chocolate or brown coloured vesicle. The two smallest branches curve backwards round the mouth of the tube, and keep up a constant whipping or flicking motion.

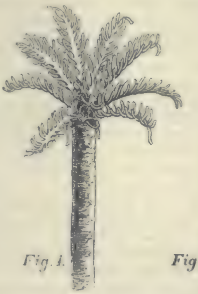
Fig. 1.

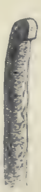
Fig. 2.

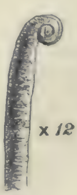

Fig. 3.

But the peculiarity is, that, upon the retreat of the animal, the mouth of the tube not only instantly closes flatly and tightly by collapse of the sides, but the tube itself, beginning at the tip, proceeds to coil up like a spiral spring, looking very much like a young fern-frond. This is, of course, an effectual protection against the intrusion of enemies, and the coiling and uncoiling, which I have witnessed many times, is a most curious sight.

Fig. 1 shows the branchial tufts expanded. Fig. 2, tube beginning to coil up. Fig. 3, tube partly coiled up—a process which is sometimes continued much further. I do not know whether this annelid has previously been noticed or described, but, if so, I shall feel obliged to any of your readers who can refer me to a description. ARNOLD T. WATSON.

Sheffield, August 19.

GEOGRAPHY AT THE BRITISH ASSOCIATION.

THERE was at least one very satisfactory feature about the Geographical Section at the Cardiff meeting. It has been the practice in all the other Sections to appoint as Presidents men who have gained a high reputation as specialists in their own departments. For some reason this practice has not been followed in the Geographical Section. True, in past years we have had such men as Murchison, Markham, Galton, General J. T. Walker; but too often the President of this Section, while eminent as a soldier, or a colonial Governor, or as a Society man, has known as much about geography as "the man in the street." It must be admitted that this has in part arisen from the fact that scientific geographers in England could have been counted on the fingers of one hand. Happily, through the recent efforts of the Royal Geographical Society, this is ceasing to be the case, and when the Chairs at Oxford and Cambridge, and the other influences which are at work, have had time to produce results, geography, in one or other of its aspects, may become as much of a career in England as it is in Germany. It was regarded as to some extent a triumph, and an earnest of what is coming, that the President of the Section at Cardiff was a geographer pure and simple. Mr. E. G. Ravenstein has long been regarded as the one scientific cartographer in the United Kingdom (where he has been naturalized for many years); and as a geographer, in the best sense of the term, he is not surpassed. It was natural that in his address he should deal with the progress of the subject in which he is master. His address, while ostensibly dealing with cartography, really showed the growth of our conception of the earth's surface, and indicated the most profitable aspects in which we may deal with that department of knowledge whose business it is to investigate.

Amid a good deal that was trivial, and notwithstanding the usual modicum of sensation, Section E did some solid work at Cardiff. The fact is that the only incident which could be regarded as sensational was the appearance on the platform of Mrs. French Sheldon, evidently suffering greatly from the accident with which she met on her return from Kilimanjaro. But Mrs. Sheldon was able to tell us some things about the people in East Africa that had never come within the ken of the male traveller. Moreover her account of the curious crater lake Chala, at the south-east foot of Kilimanjaro, was a real contribution to geographical knowledge. With immense difficulty she and her companion descended the dense vegetation which covers the precipitous sides of the crater, and navigated the tiny lake on a raft, which was continually in danger from the swarms of crocodiles. Mrs. Bishop (Miss Isabella Bird) was anything but sensational. With perfect calmness and clearness she gave an account of an almost unexplored portion of the Bakhtiari country visited by her, and especially of its interesting inhabitants. Miss E. M. Clerke's paper on the aborigines of Western Australia was more suited to the Anthropological than the Geographical Section, and still more suited to a missionary meeting.

Mr. John Coles's paper on the art of observing showed how comparatively easy it is for any man of average intelligence, and even pupils in the higher classes of our schools, to acquire a knowledge of the use of the more common survey instruments. An excellent paper on the homology of continents was read by Dr. Hugh Robert Mill, who showed that in many respects there is a remarkable family likeness among the continents, arising from the fact that they have been subjected to essentially the same influences. Mr. Silva White, in his paper on the comparative value of African lands, attempted, by a statistical method, to indicate the lines of least resistance against the European domination in Africa. Mr. Miller Christy gave an elaborate and highly instructive paper on the absence of trees from prairies; his conclusion being that the main cause of the treelessness of American prairies has been forest fires. The paper was highly suggestive, showing, as it did, that if proper measures were taken even our great deserts might be made to blossom as the rose.

The greater part of one morning was devoted to a discussion on acclimatization, introduced in a valuable paper by Dr. Robert Felkin. The author showed that there are two schools of thought, the one regarding acclimatization as impossible, the other more sanguine and pronouncing it possible. Probably the truth will be found to be a mean between the two. In considering the subject, it is necessary to specify, first, the various nations who are to be acclimatized, and secondly, the places where they are to be located. As regards the first point, the national characteristics, habits, customs, and environment must be taken into account, and with respect to the second, the nature of the country, its climatology, its inhabitants, their mortality and endemic diseases must be brought under survey. The next point is to classify the various European nations, and it becomes evident that they can only become readily acclimatized in the temperate zone, where climatic and other conditions are approximately akin to their present habitat. In reference to Europeans becoming acclimatized in the tropics, what are those factors which prevent it, or which must be overcome before it is possible? They are as follows: heat, cold, damp, various endemic diseases, especially malaria, and those constitutional conditions induced by climate which either destroy the immigrants or diminish their fertility after one or two generations. Progress has been made during recent years in enabling persons to reside longer and to enjoy greater health in the tropics. What probability is there that science will accomplish still more in rendering acclimatization possible for Europeans in tropical countries? It must be said that both Dr. Felkin and those who followed him in the discussion occasionally lost sight of the real point at issue. The adaptation of a European to tropical conditions for a few years is one thing; the acclimatization of a race in a climate totally different from that which has been its inheritance is another. About the former there need be now no difficulty: what scanty experience we have leads to the conclusion that the latter is practically impossible. What we really want are experiments continued over three or four generations.

Colonel Holdich, of the Indian Survey, gave some valuable hints in his paper on the application of Indian geographical survey methods to Africa. An outline of the methods proposed may be summarized as—(1) The adoption of a rapid system of triangulation along the most important lines for first survey. (2) The extension of a graphic system of mapping from these lines by means chiefly of native labour. The most important lines for first survey are the international boundary lines. Until lately England has been peculiarly free from the necessity of demarcating or maintaining national boundaries. Even India offers but a comparatively short line for defence. The new partition of Africa largely increases her responsibilities in this respect, though there may be no immediate cause for action. There is, however, a great necessity for a topographical acquaintance with the boundaries adopted. Only a small portion of them apparently follow permanent natural features, the rest being defined by rivers, &c. It would appear, then, advantageous to commence triangulation along the boundary lines. This is, however, so far a national or international question, and consequently in these preliminary stages of survey State assistance might very well be expected, and Imperial resources drawn upon for carrying it out. (1) What are these resources? (2) What is the nature of surveys already existing in Africa? (3) What is the nature of the survey we ought to build up? Replying to (2) and (3), we find that if a continuous and comprehensive scheme is to be adopted, with unity of design for all the scattered districts of the African colonial system, nothing has been done as yet which would assist us in carrying out our scheme. This scheme should be largely borrowed from experiences in Asia. A consideration of it shows, in reply to (1), to what extent Imperial survey resources might be utilized during the processes of laying out the preliminary lines of triangulation. From this triangulation the extension of topography would thereafter probably depend on private enterprise. Then followed a short consideration of the general topographical processes as carried out by natives of India, of the value of such native labour, and of the possibility of raising survey establishments in Africa similar to those which have done such excellent work in Asia.

The subject of reform in our Ordnance Survey was again introduced this year in an elaborate paper by Mr. H. T. Crook, who was strongly supported by a number of speakers. Mr. Crook pointed out many defects in the large-scale maps. Some of them are notoriously behind date; they are issued in a most inconvenient form; they are far too expensive; they are difficult to obtain outside of London. The Committee of this Section sent a strongly-worded resolution to the Council of the Association, recommending, among other things, that the Directorship of the Survey, instead of being merely a staff appointment, should be made a permanent office. Unfortunately, the resolution submitted to the General Committee omitted this and other important points, so that in its final form it does not amount to much.

Mr. James Thomson's paper on photography applied to exploration contained suggestions of great practical value. He showed the value of the camera, not only in recording geographical features and types of people, but even as an adjunct to regular surveys.

The subject of geographical education was introduced in a short paper by Mr. J. Scott Keltie, who spoke of the results which had followed the action initiated by the Royal Geographical Society a few years ago. Advances have been made in many directions; Chairs have been established in Oxford and Cambridge; and a higher conception of geography and of its practical utility has begun to prevail. Happily, the attempt to obtain the Section's approval for the foundation of a local Geographical Society in Cardiff failed.

Among other papers worthy of mention were two by Colonel H. Tanner, of the Indian Survey—one on a new method of Bar-Subtense surveying, and a second on some of the principal tribes of the Himalayas.

MECHANICS AT THE BRITISH ASSOCIATION.

IN Section G, Mr. T. Forster Brown, an engineer well known in the locality in connection with mining industry, was the President. There was an average list of papers, but the discussions were not so full as is sometimes the case in this Section. As a consequence, the sittings were got through with more than ordinary speed; there being no meeting on the Saturday, and the whole business of the Section was completed by two o'clock on the Tuesday of the meeting. The President's address was given as usual on the Thursday, and referred to mechanical details in connection with mining. In character with the meeting it was brief. The usual vote of thanks having been moved and seconded, Prof. Osborne Reynolds proceeded to read the third Report of the Committee appointed to investigate the action of waves and currents on the beds and foreshores of estuaries by means of working models. It will be remembered that this Committee arose out of a paper read by Prof. Osborne Reynolds at the last Manchester meeting of the Association; and this, in turn, arose out of the investigations made upon a working model of the Mersey estuary in connection with the then proposed Manchester Ship Canal operations. The further investigations referred to in the last report have been conducted on the same system as previously described. The chief object of this series has been to obtain further information as to the final condition of equilibrium with long tidal rivers entering the head of a ▽-shaped estuary; to obtain more complete verification of the value of the criterion of similarity; to investigate the effect of tides in the generator diverging from simple harmonic tides; and to determine the comparative effect of tides varying from spring to neap. It would be impossible in this brief report of the proceedings of the Section to give an idea of the results at which the Committee arrived, or rather the results shown by the experiments, more especially without the aid of the diagrams by which the Report was illustrated.

The next business was the reading of a paper by Mr. G. Chatterton, in which a sewer was described that has lately been constructed to carry off the sewerage of a neighbouring district, and thus relieve the River Taff of some of its present foul burden. The sewer, no doubt, is a meritorious engineering work, but not one of magnitude or especial novelty. The most notable point is that the Taff has to be crossed seven times, and this is effected by means of inverted syphons which go below the river bed. The principle, of course, is not new. The chief interest was in the speech made by Mr. Baldwin Latham during the discussion, in the course of which the speaker exclaimed against the "faddists" who maintain that what is taken from the earth should be returned to the earth. Mr. Latham is of opinion that what is taken from the earth should be given to the sea. The ocean, he says, was given to the engineer as a receptacle of sewage—presumably among other functions. Moreover, Mr. Latham tells us that it is more profitable to put sewage in the sea than to keep it on the land. It encourages the growth of marine fauna; and it is, so Mr. Latham says, a well-known fact that where there is most sewage there are most fish. As there were no "faddists" present, Mr. Latham had it all his own way.

Mr. L. F. Vernon Harcourt's paper described the engineering operations carried on in the neighbouring River Usk and the harbour of Newport. This paper, again, did not bring forward any points of particular novelty. Mr. Vernon Harcourt is proceeding on the now fairly well recognized lines of increasing the tidal flow. Mr. Abernethy spoke in the discussion, and told the Section how he had once resigned his position in connection with the Swansea Harbour Board because it was proposed to canalize the river. The question might, we think, have been discussed with advantage—although, perhaps, not in connection with the rivers referred to—how far volume of ebb and flow, as compared with velocity, is the ruling factor.

Mr. W. Key, of Glasgow, described the system of ventilation and heating which he had introduced in the Victoria Infirmary, Glasgow. Here, again, we have no new theories enunciated, but the paper was none the less valuable on that account—perhaps more valuable. Mr. Key has taken recognized principles, selecting and arranging in a common-sense manner, and put them into practical shape. The consequence is, we hear, that the atmosphere in the Infirmary is as sweet as that outside—in fact, more so; for, whilst there may be fog in the street and mist on the hill-side, the wards are dry and clear. The circulation of air is by rotary fans driven from a gas-engine. A point upon which Mr. Key strongly insists is a screen down which water is constantly trickling, and which is automatically flushed at intervals. This has the effect of converting dust and other floating particles into mud. The air is heated over steam-pipes in the winter. Admission is 5 feet above ground, and eduction is from the floor-level, so that dust passes off, the air current assisting gravitation.

On the second day's sitting, Friday, August 21, the chief interest was absorbed by Sir Edward Reed's paper, in which he gave certain particulars of the Channel tubular railway, which he proposes some day to construct, supposing the Fates are propitious. If one may believe the eminent engineers who took part in the discussion, the Fates never will be propitious, for Sir Edward violates the first and cardinal rule of engineering enterprise in propounding a scheme that cannot pay. Sir Edward says his double tube, which is to be laid on the bottom of the sea—it is not a tunnel—will cost 12 to 14 millions. Sir Benjamin Baker says that Sir Edward must double his figures, and even then he will not have money enough. It has been stated on the highest authority that the Channel traffic would not pay interest on a million and a quarter spent on harbours; and, if this be the case, there would be a poor prospect for those who would subscribe money for even a Channel Tunnel, far more a tubular railway, and most of all a Channel Bridge, such as Messrs. Schneider and Hersent propose. Sir Edward's scheme is sufficiently heroic. He would construct two mammoth tubes, of steel plate and concrete, 20 feet in diameter. The tubes would be made in lengths, and when two lengths were completed they would be joined together in parallel, 50 feet apart, and floated out into the Channel to be attached to the completed length. The first part of the construction, near the shore, would not be difficult, but if ever Sir Edward gets out into deep water, say 200 feet, he will find troubles enough. All work is to be done above water. Thus the end of the completed part of the double tube will be kept afloat until a fresh length is joined on. Then that will be allowed to sink, and the last attached part will form the end of the completed part. In this way, so long as the work of construction is in progress, the part of the tubes last completed will slope up from the sea bottom to the surface, so that the next length may be attached. The scheme is splendid in its disregard of difficulties. It is worthy of the fervid genius of Jules Verne.

Prof. W. Robinson next read a paper on petroleum engines. It would appear that this description of motor is likely to come to the front, if one may judge from the fact that their manufacture is being taken up by some important engineering firms. Priestman Bros., of Hull, have been at work on the problem for the last year or two, and it is chiefly of the Priestman engine that Prof. Robinson speaks. Crossley Bros., of Manchester, who have made such a brilliant success with the Otto gas engine, have now taken up the subject, and are making an oil engine; whilst the big agricultural engineering firm, Hornsby's, of Grantham, have also turned their attention in this direction. There have also been efforts made by foreign engineers. A petroleum engine works generally on the same principle as a gas engine, but the chief trouble, we believe, hitherto has been to get over the clogging of parts. This supplies the chief feature in the Priestman design, in which there is a spray maker specially designed to get over this trouble. A jet of oil is first broken up by compressed air, and the spray is then further mixed with air, heated by the hot products of combustion. To cleanse the air it is drawn through cotton wool, which naturally has to be renewed from time to time. The proportions of air and oil vapour are arranged to give an explosive charge, and a regular explosion is obtained every cycle by means of an electric spark. The cylinders are water-jacketted. Messrs. Priestman have fitted a pair of their oil engines into a small launch, which is said to have answered well. Whether petroleum used explosively in an engine afloat will ever oust our tried but very imperfect servant steam—as the gas engine is superseding the steam engine in so many positions ashore—is a very open question. Certainly it is a great temptation to get rid of the heavy and bulky boiler, which takes up so much room in a boat, but much remains to be done before we can arrive at the more logical method of generating heat energy in the place where it has to be used. It may be that that terrible exhaustion of our coal-fields, about which we heard so much at the meeting of the Association, will be indefinitely postponed by the using of petroleum or other hydrocarbon as a source of motive power. But that is another story.

Mr. Beauchamp Tower described some improvements in detail which he has introduced in the design of that beautiful piece of mechanism by which he has secured to us, by means of gyroscopically-controlled hydraulic gear, a steady platform at sea; and Prof. A. C. Elliott read a paper on the transmission of power by compressed air. Dr. William Anderson described his revolving water purifier; and Mr. Faija gave a long account of many points in connection with Portland cement. These were all the papers read on Friday.

On Saturday there was no meeting in Section G, and Monday was, according to custom, devoted to electrical matters. Mr. W. H. Preece opened the proceedings with a long paper, or rather lecture, on the London and Paris telephone, in the course of which he was enthusiastic upon the success which had been obtained. He is sanguine that before long we shall be able to talk between London and Berlin. Of course, he improved the occasion by insisting on the necessity of metallic returns, a point upon which all will agree with him except shareholders in telephone companies. Naturally, also, Mr. Preece did not fail to hint how much better off the British public would have been had telephone exchange been left in the hands of the Post Office. No doubt, if all the telephones were now transferred to Mr. Preece's guidance, we should sooner have metallic returns, and Christian patience would be less exercised; but the question may arise whether we should have had any telephones at all now if Government monopoly had not been broken through. With Mr. Preece as the controlling factor, we should answer "Yes." But there are other sorts of Government officials than Mr. Preece.

Mr. Bennett's paper on the telephoning of great cities referred mostly to the arrangement of details of exchange.

Prof. G. Forbes read a long paper, in which he gave an account of recent progress in the use of electric motors. It was of an interesting nature, and dealt largely with the advance that has been made in America. We trust Mr. Forbes is better acquainted with Transatlantic electrical practice than he is with one branch, at least, of British practice; for when he said, as we understood him, that there are no electrical cranes in England, he was certainly wide of the mark.

Papers by Mr. N. Watts, on electric fire-damp indicators, and by J. A. Timmis, on electric lighting in trains, were also on the list.

On Tuesday, August 25, Section G held its last sitting, and there was a varied selection of papers. The first was a contribution by Mr. A. R. Bennett, in which he advocated a system of house-to-house parcels distribution, which would certainly be very convenient if it could be carried out. He proposes tunnels under the street with miniature electric railways. That would be a difficult thing to arrange in any of our cities, the space being so occupied by gas- and water-pipes, sewers, electric wires, hydraulic mains, and many other things, were the tunnels simply to be run straight away with only stations at distant points; but Mr. Bennett proposes to make this a house-to-house service, each subscriber having his own siding. The tube would be rectangular, with two lines of rails one above the other. By means of semaphores at the central station, worked electrically by the passage of the train, so that the operator can always tell where the train is, and by further electrical connection he is able to shunt the train into the subscriber's own siding. When one subscriber wants to send a parcel to another, he procures a truck, and despatches this through the tunnel to the central station, from whence the operator forwards it to the right address. There is even an arrangement for unloading automatically, and the truck can then be brought back by the operator without the intervention of the subscriber. The idea is fascinating, and we may say that it appears quite practicable; but it will not come yet. Some day, when we determine to pull down and rearrange London—as manufacturers throw aside obsolete but perfectly sound machinery to gain the economy of some newer designs—Mr. Bennett's electrical exchange may come in; and then the blessing it will be to the community will be incalculable. We can have a five minutes collection and delivery of letters; butcher-boys will no longer whistle at the side door, and the baker will cease to scribble on the gate-post.

Mr. W. Worby Beaumont next read a paper on internal and external work of evaporation. This is one of a series of monographs which the author has prepared on this subject, but the matter is too abstruse for us to deal with in this very brief account of the four days' meeting. Were we to attempt to abstract the paper, it might lead us into controversial matter.

Major R. de Villamil's paper on the action of screw-propellers was a praiseworthy effort to accomplish the apparently hopeless task of lifting the practice of designing the screw-propeller from the region of empiricism—where it has always dwelt—to the domain of pure science. We fear, however, in spite of it, that the marine engineer will still adhere to the ancient rule-of-thumb by which alone he is now guided. It is curious that the man who has done most to improve the design of the screw-propeller was essentially non-scientific. He made his chief discovery in an endeavour to do one thing, but produced the reverse result. When Griffith first used the spherical boss, he was trying to produce a retarding effect, but found, on trial, that he had added greatly to the efficiency of the screw.

Mr. Beaumont also read a paper on the screw-propeller. He described a method of reversing the direction of thrust by means of feathering-blades, on the well-known Bevis principle. The advantages claimed were that, as the engines and screw would be always running in one direction, there would be no momentum of moving parts to be overcome when it was desired to go from ahead to astern, or *vice versâ*, and therefore there would be less danger of breakage of the mechanism. The proposal was somewhat roughly handled in the discussion which followed, but we think that Mr. Beaumont fairly held his own in his reply. The most valid objection appeared to be that of Mr. Heard, who pointed out that the pressure on a given area of the blade was by no means constant throughout each revolution, and the disturbance would cause the joints of the mechanism to wear. For this reason there would be introduced an undesirable and even dangerous play on the pins after the apparatus had been in use some time.

A paper upon non-conducting coverings for steam-boilers having been read, the business of Section G was brought to a close with the usual votes of thanks.

ANTHROPOLOGY AT THE BRITISH ASSOCIATION.

THE proceedings began with the President's address, after which Prof. R. K. Douglas read a paper on the social and religious ideas of the Chinese as illustrated in the ideographic characters of the language. After a short introduction, showing that the Chinese ideographic characters are picture-writings, the author gave an account of the earliest or hieroglyphic form of the writing, the development of this resulting in the ideographic characters. The social habits of the people and their domestic life were illustrated by a number of ideograms descriptive of their household arrangements and relationships. The author traced in the written characters the ideas associated with men and women, their virtues and their failings; the notions connected with marriage; and the evidences of pastoral as well as of agricultural habits among the people. The paper concluded with references to the coinage of the country as described in the ideograms employed to represent its various forms.

The following papers were also read: on recent progress in the analysis of vowel-sounds, by Dr. R. J. Lloyd; family life of the Haidas (Queen Charlotte Islands), by the Rev. Charles Harrison; and the Report of the North-Western Tribes of Canada Committee. This last is again the work of Dr. Franz Boas in the interesting ethnological field of British Columbia. It consists of two parts, the first being devoted to the Bilqula, a people inhabiting a limited tract in the vicinity of Dean Inlet and Bentinck Arms, the second dealing with the physical characteristics of the tribes of the North-west coast region.

Prof. Max Müller then made some remarks on the work of Major J. W. Powell, Director of the U.S. Bureau of Ethnology. He said that he had just received the proof-sheets of a most important publication on the classification of the Indian languages spoken in America. It is a splendid piece of workmanship from Major Powell, the indefatigable Director of the American Bureau of Ethnology. The publications of that Bureau count amongst the most valuable contributions to anthropological science, and they reflect the highest credit, not only on Major Powell and his fellow-workers, but also on the American Government, which has sanctioned a very large outlay for the prosecution of these studies. There is no stint in the way these volumes are brought out, and most of the papers contained in them inspire the student with that confidence which can only be produced by honest, conscientious, and truly scholarlike work. Our American friends have perceived that it is a national duty to preserve as much as can still be preserved of the languages and thoughts of the indigenous races who were the earliest dwellers on American soil. They know that the study of what Prof. Max Müller ventured to call intellectual geology is quite as important as that of terrestrial geology, and that the study of the lower strata contains the key to a right understanding of the higher strata in the growth of the human mind. Coming generations will call us to account for having allowed the old world to vanish without trying to preserve its records. People who ask what can be the use of preserving the language of the Mohawks forget what we would give if some scholar at the time of Cato or Cæsar had written down, what many could then easily have done, a grammar of the Etruscan language. Some years ago the author had succeeded in persuading a Secretary of State for the Colonies that it was the duty of the English Government to publish a series of colonial records, containing trustworthy information on the languages, customs, laws, religions, and monuments of the races inhabiting the English colonies. Lord Granville saw that such an undertaking was a national duty, and that the necessary funds should be contributed by the various colonies. What a magnificent work this would have been! But while the American Government has pushed forward its work, Lord Granville's scheme expired in the pigeon-holes of the Colonial Office. America may well be proud of Major Powell, who would not allow the treasures collected by various scholars and Government officials to moulder and perish. He is a true enthusiast, not a man of mere impulse and good intentions, but a man of sustained effort in his work. He deserves the hearty thanks of the Association, and more especially of the Anthropological Section.

The whole of Friday morning was occupied by a paper by the Marquess of Bute, on the language of Teneriffe. The difficulties in the study of the language are due to the fact that the aboriginal words have been collected from *all* the islands without indicating their several origins, so that the Teneriffe words were not at first easily distinguished. Students hitherto have held three opinions as to this language. The first is that of Dr. Glas, who considered the language American (and the people African); the second, advanced by Sir Edmund Scory, classed the language and people as Berber; while the third holds that the Teneriffians were of Aryan origin.

Dr. Edward B. Tylor read a paper on the limits of savage religion. It has lately become clear by the inquiries of anthropologists that the world-famous Great Spirit of the North American Indians arose from the teachings of the Jesuit missionaries in Canada early in the seventeenth century. This and analogous names for a Supreme Deity, unknown previously to native belief, have since spread over North America, amalgamating with native doctrines and ceremonial rites into highly interesting but perplexing combinations. The mistaken attribution to barbaric races of theological beliefs really belonging to the cultured world, as well as the development among these races of new religious formations under cultured influence, are due to several causes, which it is the object of this paper to examine: (1) direct adoption from foreign teachers; (2) the exaggeration of genuine native deities of a lower order into a god or devil; (3) the conversion of native words, denoting a whole class of minor spiritual beings, such as ghosts or demons, into individual names, alleged to be those of a Supreme Good Deity or a rival Evil Deity.

Mr. H. Ling Roth read a paper on *couvade*, in which he gave an account of the distribution of this curious custom, and showed that the savage believes that there is some hidden link which binds the new-born child to its father, and he argued that the practice of *couvade* is to prevent the father bewitching his child.

In a paper by Mr. S. E. Peal, on the *morong* and other customs of the natives of Asam, the author shows that this institution of the *morong*, or club-house for the unmarried, is very widely distributed over the whole of the Indo-Pacific region, and he argues that it is, in fact, a relic of pre-marriage communism. Moreover, this custom being so often found associated with others of a distinctly non-Aryan character, such as juming, tattooing, blackening the teeth, building on piles, head-hunting, &c., has led him to suspect former racial affinity, even among such widely different types as Papuan and Mongol, Dravidian and Sawaiori.

A paper by the Rev. B. Danks, on the burial customs of New Britain, was read.

In a paper on the worship of meteorites, Prof. H. A. Newton, on Monday, gave a series of accounts of divine honours having been paid to meteoric stones in early times, and of myths and traditions pointing to such worship. Particular attention was directed to the indications of such worship that are found in Greek and Roman history and literature.

Dr. Garson read a paper on some human remains found in Yorkshire. He dealt principally with a round barrow in which skeletons with very long skulls had been found. These skulls were much longer and narrower than the heads of the existing inhabitants of this country, and corresponded with those of the Iberians. The average height of the persons whose skeletons were found in this barrow was a little over 5 feet 3 inches. The discovery of flint and the absence of iron implements showed that the burial took place before the use of metals. The Iberian people were short, had dark hair, straight noses, flat foreheads, and no ear-lobes. It was a race quite distinct from the Celtic type, which afterwards came in and drove them further westwards into forests and swamps.

A paper by Miss Buckland was read, on points of contact between Old World myths and customs and the Navajo myth entitled "The Mountain Chant." The author drew attention to the numerous points in which this myth reproduces customs and beliefs of the Old World. Among these were mentioned the singular prohibition of food in the abode of spirits, such as appears in the classical story of Persephone, but which is found slightly modified in the fairy folk-lore of Europe, in Aino and Japanese tales, and in New Zealand. Miss Buckland points out the great contrast between the bloodless Navajo rites and the sanguinary ceremonies of the ancient Mexicans, and the great dissimilarity in the forms of the Navajo and Mexican gods, as denoting an entirely different origin for the two religions, incompatible with the belief commonly entertained of the wholly indigenous character of American culture; and she urges that the Navajo rites point unmistakably to an Eastern origin.

A paper by the Rev. James Macdonald, on East Central African customs, was read. The customs dealt with ranged over the whole domestic and social life of the people.

The following papers were also read:—Prof. G. Hartwell Jones, barbaric Greece and Italy; J. E. Budgett Meakin, the Berbers of Morocco; Dr. J. S. Phené, a comparison of ancient Welsh customs, devices, and commerce with those of contemporary nations; W. M. Adams, the first sea-wanderings of the English race. The Report of the Prehistoric Inhabitants Committee, and the Report of the Elbolton Cave Committee, were also read.

On Tuesday, Dr. Garson read a paper on M. Bertillon's method of criminal anthropometry, in which he described the plan now adopted by the French police for the identification of criminals.

Dr. S. A. K. Strahan read a paper on instinctive criminality, its true character and rational treatment. The instinctive criminal belongs to a decaying race, and is only met with in families whose other members show signs of degradation; in fact, instinctive criminality is but one of the many known signs of family decay. Not only is criminality hereditary, but it is interchangeable with other degenerate conditions, such as idiocy, epilepsy, suicide, insanity, scrofula, &c., and it is a mere chance whether the insanity or drunkenness, say, of the parent, will appear as such in the child, or be transmuted in transmission to one or other of the above-mentioned degenerate conditions. Alcoholism is the most fruitful source of instinctive criminality, but insanity, epilepsy, and suicide are often transmuted to crime in passing to the children. Senility and immaturity of parents are also fruitful sources of crime in the enfeebled descendants, as is proved by the statistics of Marro, Korosi, and others. The present system of treatment has proved a disastrous failure; short periods of punishment can have no effect upon the instinctive criminal, either curative or deterrent. Everything points in the direction of prolonged or indefinite confinement in industrial penitentiaries. This system has been tried with success in America, and life-long detention has not been found by any means necessary.

Nicobar pottery, by E. H. Man. In this paper Mr. Man stated that the little island of Chowra has held for generations a monopoly of the manufacture; and the entire work of preparing the clay, as well as of moulding and firing the finished utensil, devolves on the females of the community. The inhabitants of the island appear to guard their art jealously, and the value of trade-marks is recognized. No vessels are made especially by the Nicobarese for funeral purposes, but cooking pots are among the personal and household requisites which are laid on a grave after an interment. They have no knowledge of any implement answering the purpose of a "potter's wheel."

The following communications were also received:—E. Seward, on the formation of a record of the prehistoric and ancient remains of Glamorganshire; Dr. J. S. Phené, on recent Hittite discoveries; Mrs. S. S. Allison, account of the Similkameen Indians of British Columbia; Report of the Anthropometric Laboratory Committee; Report of the Anthropological Notes and Queries Committee; and the Report of the Indian Committee.

SCIENTIFIC SERIALS.

THE *American Meteorological Journal* for September contains the concluding part of an article on mountain meteorology, by A. L. Rotch. The subjects specially treated of are wind and temperature in connection with atmospheric pressure, as observed chiefly at the Blue Hill Observatory. The wind velocity is found to be two-thirds greater there than at Boston, about 500 feet lower, but the difference changes for various hours of the day. At low levels the wind force generally increases from the early morning until the afternoon, but the conditions are reversed at higher levels. This fact was pointed out by Prof. Hellmann in 1875, when studying the Mount Washington observations, and the same fact has since been observed at Ben Nevis and other Observatories. The wind has also a vertical as well as a horizontal motion, which has amounted to seven miles an hour in a storm. The normal temperature at the summit of Blue Hill is 2° lower than at the base, giving a decrease of 1° for each 220 feet of ascent, but inversions frequently occur, when the temperature of the base is lower than at the summit. Instances of this are given, together with records obtained during balloon ascents.—The aspiration psychrometer and its use in balloons, by Dr. R. Assmann. Such an instrument was first used by Welsh in 1853, but it was not fully adapted to use in balloons. The apparatus invented and described by Dr. Assmann, which is intended to register the changes, which ordinary thermometers do not show quickly enough, is made by Fuess, of Berlin. The aspirator may be driven by a small electric motor, instead of by clockwork.—The Bergen Point tornado, by W. A. Eddy. The track was about nine miles south-west of New York City, on June 16 last. The tornado was preceded and followed by showers of large hailstones, and extended only for about two miles.—The hot winds of California, by Lieutenant J. P. Finley. The period during which these winds occur is from May to September; the thermometer has been known to reach 118° in the shade, and the winds generally occur during entire absence of clouds.—Altitude and hay fever, by Dr. W. J. Herdman. Special attention is drawn to the curative influence of mountain stations.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 14.—M. Duchartre in the chair.—Recent discussions on the subject of cyclones, by M. H. Faye.—A contribution to the botanical history of the truffle—*Kammé*, from Damas (*Terfezia Claveryi*), by M. A. Chatin. A description of a new species of truffle—the white truffle of the desert, known in Syria under the name *Kammé*. It has a wide range, the same species as this found near Damas having been also seen in the desert 400 miles south of Biskra. It forms an important article of food.—On the incandescence of platinum wires under water, by M. Paquelin. A mixture of hydrocarbon vapours and air is led over a specially arranged platinum apparatus, which becomes heated almost to its fusion point, and will then remain luminous if suddenly plunged into water.—Observations of the Comet Wolf, 1884 III., made by the *coudé* equatorial (0·36 m.) of the Lyons Observatory, by M. G. Le Cadet.—On the yeast of wine, by M. A. Rommier. Experiments made on the production of wines from vines of the same stock grown in different districts lead to the conclusion that the ferments producing the characteristic bouquet in wines of different districts, are peculiar to those districts and are not carried to new districts readily by the transplantation of the vines.—On the determinism of sexuality in *Hydatina senta*, by M. Maupas.

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THURSDAY, OCTOBER 1, 1891.

THE BACTERIOLOGICAL EXAMINATION OF WATER.

Manuel Pratique d'Analyse Bactériologique des Eaux. Par le Dr. Miquel. (Paris: Gauthier-Villars et Fils, 1891.)

THERE is probably no body of scientific men amongst whom national feeling and prejudice are so little under control as the workers in the domain of bacteriology. In perusing memoirs, text-books, dictionary-articles, and literature of every kind bearing upon this infant science, the reader must almost invariably take into consideration the language in which they are written, more especially whether German or French; and if the author belongs to neither of these rival nationalities, it is not unfrequently desirable to ascertain in which of the two camps he has been educated, for, unless this be made allowance for, a warped and often erroneous impression will be carried away.

The present work certainly forms no exception to this state of things; indeed, this phenomenon of party-spirit is regrettably prominent. Thus, in reading one of the first paragraphs, beginning with "Les premières statistiques relatives à la richesse bactérienne des eaux furent publiées par moi," and, indeed, throughout these pages we are reminded of the words of the deeply lamented *savant* who commenced his monumental work with "La chimie est une science française," and perhaps even more of the famous utterance, "L'état, c'est moi!"

Dr. Miquel's treatise, consisting of 194 pages, is divided into five chapters, dealing respectively with (1) the collection of samples, (2) the transport of the collected water, (3) the quantitative analysis, (4) the qualitative analysis, (5) the interpretation of the results obtained. On these subjects Dr. Miquel should be well qualified to write, because, as he informs us, it is only in his laboratory at Montsouris that the bacteriological examination of water has been carried on over a period of eleven years. Indeed, we know of no bacteriologist who has so entirely devoted his attention to the subject of micro-organisms in air and water as Dr. Miquel, whose name is so inseparably connected with "les organismes vivants de l'atmosphère." His energies have, however, apparently not been so successfully directed to the aquatic as to the aërial microbes, for we do not connect Dr. Miquel's name with any of the more important advances that have been made in our knowledge of the bacteria in water during the past ten years. The comparative sterility of Dr. Miquel's researches in this direction is perhaps partially to be accounted for through the extraordinarily cumbrous method of water-examination which he formerly exclusively employed, and which has placed him at a great disadvantage by the side of those investigators who at once availed themselves of Koch's methods, which Dr. Miquel, like many other French bacteriologists, has only adopted with reluctance, or almost under compulsion. The chief interest attaching to the bacteriological examination of water lies in its application to the hygiene of water-supply, inasmuch as it is all but certain that two at least of the most fatal zymotic diseases—cholera and typhoid—can be, and are, constantly propagated through the presence of specific micro-organisms in water, and indeed the majority of bacteriologists are agreed as to the particular forms responsible for these diseases. On this account it is conceived by many that the primary object of the bacteriological examination should be the search for such pathogenic microbes. This view is apparently endorsed by Dr. Miquel when he says, "Le but que doit poursuivre le micrographe dans les analyses bactériologiques de l'eau est sans contredit la découverte des organismes pathogènes"; although the logical conclusion to be drawn from the pages which follow, and in which he details the methods to be pursued in this quest, is that such an investigation is generally fraught with insuperable difficulties, and, for sanitary purposes, practically worthless. Thus, without wishing to detract from the importance of the discovery by Chantemesse, Widal, and others of the typhoid bacillus in certain waters which had been suspected of propagating this disease amongst their consumers, it is surely obvious that, even if this organism could be detected with unerring certainty in any water in which it was present, a search for this bacillus in the ordinary course of water examination would still have only a very subsidiary interest. Waters are surely not only to be condemned for drinking-purposes when they contain the germs of zymotic disease at the time of analysis, but in all cases when they are subject to contaminations which may at any time contain such germs. Sewage-contaminated waters must on this account be invariably proscribed, quite irrespectively of whether the sewage is, at the time that the water is submitted to examination, derived from healthy or from diseased persons. In the present state of our knowledge there can be no doubt that chemical analysis affords us in general a better, although a far from perfect, indication of sewage contamination than do the results of bacteriological examination. The real value of these bacteriological investigations, if judiciously applied, consists in their power of furnishing us with information as to the probable fate of dangerous organisms, should they gain access to drinking-water. It is by their means that we have learnt that many such organisms can preserve their vitality, nay, in some cases can actually undergo multiplication, in ordinary drinking-water; that they are destroyed by maintaining the water at the boiling-point for a short time; and that they are more or less perfectly removed by some processes of filtration and precipitation, whilst other processes of the same nature are worthless, or even worse.

These important results are of the greater value inasmuch as they have been obtained not only by experimenting with the few pathogenic organisms with which we are at present acquainted, but by studying the effect of these several processes on the complex mixtures of micro-organisms that are to be found in natural waters. The rapidity with which this knowledge has been acquired is due to the quantitative accuracy combined with facility of manipulation which characterize the method of gelatine-plate culture. It has been repeatedly urged against this method that it is incapable of revealing many well-known forms of bacteria which either do not grow in the gelatine-peptone medium at all, or at any rate not at those temperatures at which it still remains solid, and it is in this respect that Dr. Miquel claims superiority for his infinitely more laborious method of "ensemencements fractionnés" in bouillon. It is obvious that labour must be no consideration if any great scientific advantage is to be attained; but, on the other hand, the unnecessary complication of processes, without corresponding benefits, must invariably lead to the retardation of scientific progress. Now, it would certainly appear that the benefits obtained by Miquel's process are in no way commensurate with the additional labour which it entails. Thus, his process is also incapable of revealing all the microbes which may be present in water, and yields at best only a closer approximation to the total number than does the gelatine method. For the general purposes of the bacteriological examination of water, however, it is of very little consequence whether the method employed reveals, say, 30, 50, 70, or 90 per cent. of the total number of microbes present, all that is required being a result which will serve for comparison. Thus, supposing it is desired to ascertain the efficiency of some process of filtration, provided that the unfiltered and filtered waters respectively are submitted to the same method of examination, the comparative result will be the same whether 50 per cent. only or all the microbes present are in both cases enumerated. Thus putting this statement to the test of actual experiment, from the results of the gelatine-plate method of examination I reported to the Local Government Board in 1886 that the average reduction in the number of micro-organisms present in Thames water effected by the sand-filtration of the several London water companies amounted to—

98·6	per cent.	for the	Chelsea Company,
99·1	,,	,,	West Middlesex Company,
96·7	,,	,,	Southwark Company,
98·2	,,	,,	Grand Junction Company,
96·2	,,	,,	Lambeth Company,

whilst Dr. Miquel in 1890 gives as the effect of sand-filtration on the water of the River Loire a reduction of 99·3 per cent. in one case, and 99·4 per cent. in another case. A concordance more complete than this can certainly not be demanded. Similarly it can be shown that Dr. Miquel's method of water examination has not yielded any results of importance which had not already been arrived at before by other investigators using the more expeditious method of plate cultivation. It is indeed only for such differential experiments as that referred to above that the bacteriological examination of water, in the present state of our knowledge, is really of much value, for any judgment as to the purity or otherwise of a sample of water based upon the actual number of microbes found in a given volume of it, is liable to lead to the most serious errors, in consequence of the remarkable power which some bacteria possess of multiplying to an extraordinary extent in waters of the greatest organic purity; in fact, it is precisely in the purest waters that such multiplication is often most pronounced. It is the possibility of such multiplication taking place which renders it imperative that samples of water should be submitted to bacteriological examination within a few hours of their collection. In order to overcome this difficulty, which has hitherto debarred the examination of waters from distant sources, Dr. Miquel has the samples transmitted in a box surrounded with ice; to this there are manifold objections, for the low temperature thus secured by no means completely arrests the multiplication of some bacteria, whilst it causes the destruction of others. Dr. Georg Frank, of Berlin, on the other hand, seeks to overcome the difficulty by deputing to persons on the spot the task not only of collecting the samples, but also of preparing the plate-cultures; but, considering the nature of the instructions which he finds it necessary to give to the novices to whom this work may fall, the expedient does not appear very promising. The following is a verbatim extract from these instructions recently published in a German scientific journal of repute, which surely demands no comment:—

"The person commissioned with the collection of the sample takes off his coat, turns up his shirt-sleeves on both arms, fastening them so securely that they cannot fall down of themselves. Then he washes his hands and arms most carefully with soap and brush to above the elbow-joint. Special care must be bestowed upon the cleansing of the finger-nails, which must if necessary be treated with the nail-file. Finally, the person in question dries himself with a clean towel."

We take it that the value of results depending upon manipulations carried out by persons requiring these instructions would be such that it would be no loss if they were dispensed with altogether. Indeed, unless the bacteriological examination of water be invariably carried out by qualified persons, and by them employed only in cases where it is really capable of rendering service, it is certain to fall into that disrepute which has so frequently been drawn down upon the chemical examination of water through incompetent analysts. Indeed the bacteriological method has already seriously suffered in public estimation through the contradictions which have resulted from the attempts made in some quarters to classify waters according to the number of microbes revealed on cultivation. Such arbitrary standards have already done much mischief in the case of the chemical analysis of water; in the bacteriological examination they are still more reprehensible, and it is deeply to be regretted that Dr. Miquel, in this most recent work on the subject, should seek to perpetuate a system of standards which experience shows to be quite untenable.

The work concludes with some excellent recommendations as to the sterilization of water for drinking-purposes, a subject which cannot be too frequently brought into public notice, for, using Dr. Miquel's own words, "la vie d'un homme a bien sa valeur à côté du prix insignifiant auquel revient le litre d'eau purgée de germes qu'il peut consommer en vingt-quatre heures."

PERCY F. FRANKLAND.

EPIDEMIC INFLUENZA.

Epidemic Influenza: Notes on its Origin and Method of Spread. By Richard Sisley, M.D. (London: Longmans, Green, and Co., 1891.)

THE object of this brief treatise, which was prepared before the issue of the Report of the Local Government Board, is to prove the doctrine, widely held by physicians of eminence in the eighteenth century, that influenza is contagious, or, more strictly speaking, infectious, and therefore, in the opinion of the author, fit to be included among the diseases of which notification is locally compulsory. The book is somewhat peculiar in its arrangement, but in the essential qualities of impartiality and clearness leaves nothing to be desired. Many readers who do not require more than specimens of evidence, will thank Dr. Sisley for compressing the digest of "many thousands" of notes into such narrow compass; but other minds will require a chain of which every link is massive, to guide them to the point of view whence practical conclusions are palpable. If the manner of statement is somewhat bare, and examples rather scanty, in the exposition of a strong but disputed case, the facts brought forward bear none the less value in their neutral setting, and go far to justify the proposition with which he confronts us at the outset, derived from a study of the distribution of the disease and from its pathological character. Valuable assistance from Dr. Klein, Prof. Fleming, and many others, has enabled him to include in his pages some interesting matter relating to the microbic nature of the epidemic and its relation to a similar disease in animals. After all that has been conjectured on the latter point, it appears that evidence of any unusual prevalence of influenza among animals at the time is still wanting.

The original seat of influenza, which has been obscurely indicated in previous times as lying somewhere "in the East," has now been discerned in Mongolian and Chinese territory, for we have two independent accounts, each speaking of influenza as not uncommon in some parts of China. In Mongolia "it seldom proves fatal, but travellers are careful to avoid it, and no one would think of using the pot or ladle of a family suffering from this sickness." If the disease is sporadic and endemic in these countries, the population may be to some degree protected against epidemic outbreaks, for we have seen in Europe that the tendency to spread is much less marked in a second invasion occurring within one year, and least, on the whole, in those places where it was previously most severe.

The notes from Bokhara, translated in this volume, are of great importance, for they show how a wet spring had turned the neighbouring country into a perfect marsh, from which, when the hot weather set in, poisonous exhalations were given forth, and how the people, crowded together with horses, cattle, and sheep between high walls, distressed and weak with starvation and disease, were attacked much earlier than usual, in the first heat of summer, with malaria, and how this was quickly followed by an epidemic of influenza, reaching its height in July 1889. The extension of the disease westwards from Bokhara by the flight of convalescents to Russia, and eastwards by caravans to post-stations in Siberia, has been noticed in the official Report, and completes the evidence connecting the European epidemic with the miserable condition of an Asiatic town. Upon such a soil, influenza sprang into fatal activity, and acquired, as we may fairly infer, a particular virulence. In similar conditions, amid the filth, floods, and famines of Asiatic countries, cholera and other plagues of men and animals have been evolved and have set forth on their destructive march.

By reports from several medical officers, and by a number of charts showing the curve of prevalence of the disease in English and foreign cities, Dr. Sisley shows that we have no experience of any sudden prostration of a large population within a few days, such as was formerly supposed to occur; but that the rise is always gradual from a few cases to hundreds and thousands, the maximum usually occurring from one to two months after the first cases in the locality have been noted. Last century Dr. Haygarth had been fortunate in discovering the person who brought the infection to each place in his district. If equal pains had been taken in 1890, when the disease was on its way to us from Russia, the persons who conveyed it from country to country might, no doubt, have been identified. The author has not been able to find a single instance in which there was a sudden infection of a large number of people without the previous existence of cases of the disease; and wherever its course was studied with care, it was seen to spread in the same way as other infectious diseases. But the "atmospheric" doctrine, though previously disproved with regard to rabies, cholera, and pestilence in general, still finds a stronghold in consumption and influenza.

The classic examples of ships supposed to have been attacked on the ocean by wind-borne influenza, as well as those of towns supposed to have been prostrated "in a single day," really bear testimony to the insidious growth of the disease and to the necessity of early recognition. Neither in this volume nor in others on the same subject is the fact sufficiently dwelt upon, that the geographical distribution of this and of previous epidemics in successive weeks and months was wholly unlike what would have occurred if the germs had been largely spread, either by lower or by upper atmospheric currents.

The total exemption of lighthouse-keepers, deep-sea fishermen, and unvisited islands, is scarcely noticed by Dr. Sisley, but he considers the rarity of influenza among prisoners to have been due to their removal from sources of contagion, and relates a very interesting case of apparent infection of a man on his way home from a light-ship through contact with the crew of a fishing-boat, said to be in good health.

Dr. Sisley concludes that there is no convincing proof of transmission through unaffected persons, letters, &c.; but a series of cases each of considerable weight surely amounts to evidence strong enough to justify some precautions, such as would be taken with the organic dust from more serious diseases, *e.g.* scarlet fever and diphtheria, which are so transmissible. There is happily a great deal in common in the mode of spread of most zymotic diseases, and disinfection as usually practised could hardly be misapplied to influenza. The same may be said with regard to isolation, for no attack, however trivial in itself, is a matter of indifference to the public, if it may result in widespread illness, loss of work, and distress. A short retirement is desirable in the interest both of the patient and of the public. But Dr. Sisley can hardly desire that notification should take place on exactly the same lines as that of other diseases, for local authorities would with reason wince at the expense; and unless the notification were a national undertaking, no district would be adequately protected thereby from imported cases. Complete and national measures of notification and isolation, with the co-operation of local authorities, would be much more likely to be effectual. An expenditure of one-fiftieth of the cost of the recent epidemic would probably secure the country from any such infliction in future. But we must admit that without a somewhat strict supervision at ports of entry during the period of prevalence in other countries, and without provision for the segregation of slight or suspected cases during that period, mere notification would not be likely to put a stop to the spread of influenza. The early cases are worth taking a great deal of trouble to discover and isolate. When once many cases have occurred in a locality, the further progress of so protean a disease is difficult to arrest. The best chance of averting an epidemic must be sought in scrupulous care for early isolation, in tracing the movements of travellers from infected towns, and in the increased practice of ventilation in private houses and in public gatherings. Like typhus, influenza seems incapable of inflicting much damage except through the medium of close, confined, and impure air, and where measures of isolation and disinfection are used it seldom spreads. But the infectious character of influenza must be internationally recognized before protective regulations can achieve a full measure of success.

R. RUSSELL.

GENERAL CHEMICAL MINERALOGY.

Allgemeine Chemische Mineralogie. Von Dr. C. Doelter, O. Professor der Mineralogie an der K. K. Universität Graz. With 14 Figures in the Text. (Leipzig: W. Engelmann, 1890.)

MINERALOGY, at first purely descriptive, has been raised to the dignity of an experimental science by the application of the principles of chemistry and physics. The writer of a mineralogical text-book is thus met at the outset with the difficulty of deciding what amount of knowledge of chemistry and physics to assume in his reader. With regard to the chemical side at least, the rule appears to be to assume that he knows very little, and yet, somewhat inconsistently, to make the exposition of the atomic theory and the fundamental principles of chemistry so brief as to be of little service to one who has had no previous acquaintance with the subject.

The author of the present, in many respects useful and suggestive, book follows the same lines. The whole account of the fundamental chemical theories occupies about ten pages of the introduction. The same fault will be found in other parts of the book: *e.g.* it would be difficult to say to what class of reader a large portion of the chapter on chemical analysis would be useful. In his endeavour to introduce as many extracts as possible from the current literature of the subject, the author allows himself in many places to become somewhat sketchy. In spite of this, the book, with its wealth of information upon points which have not hitherto found a place in ordinary mineralogical text-books, will be found to give a very good idea of the present state of mineralogical science from a chemical point of view.

The arrangement of the book is in seven sections, viz. (1) introduction; (2) chemical crystallography; (3) chemical analysis of minerals; (4) synthesis of minerals; (5) metamorphism of minerals; (6) formation of minerals in nature; (7) chemical composition and constitution of minerals.

In the introduction, containing an account of the atomic theory and its consequences, one or two suggestive ideas will be found: *e.g.* the correspondence, pointed out by Tschermak, between the chemical law of multiple proportions and the crystallographic law of simple parameter ratios; and also the analogy between the law of constant proportion by weight and the fundamental crystallographic law of constancy of angle. The subject of chemical crystallography receives very full treatment. Here the reader is initiated into the mysteries of chemical and physical isomerism, polymorphism, enantiotropy, isomorphism, isodimorphism, isogonism, morphotropy, &c.; and if the perusal of this section, as well as of the last, on the constitution of minerals, shall leave him with a rather confused and unfavourable idea of the subject, the fault should perhaps be rather attributed to the present imperfect state of our knowledge than to the author. At present it is in most cases impossible to say whether bodies are polymeric, metameric, or chemical isomers.

As regards isomorphism, if the formation of mixed crystals is to remain the test, the original definition of Mitscherlich must be modified to suit the fact of the formation of mixed crystals from compounds of not precisely analogous chemical composition. Thus, according to modern views, isomorphism is in some degree to be deposed from its proud position as an infallible guide to chemical composition. The insidious nature of the attack upon this ancient stronghold of the faith may be judged by a comparison of one of the latest definitions of isomorphism with the original definition of Mitscherlich. According to the latter, isomorphism is the power which two or more compounds of *analogous* chemical composition possess of crystallizing in the same or similar crystalline forms, and of mixing in varying proportions to form homogeneous crystals. The latest definition is that bodies are isomorphous which, with *for the most part similar* chemical composition, possess the property of crystallizing in similar crystalline forms, and of forming mixed crystals which morphologically and physically graduate into each other. Such a change it is expected would lead to a considerable simplification in many of the formulæ which have been made unnecessarily complicated in order to comply with the requirements of Mitscherlich's definition.

The section on chemical analysis of minerals is one of the least satisfactory in the book. Short summaries of analytical methods can be of little service to any class of reader. Amongst matter which will not be generally found in the ordinary chemical text-book, this section contains some account of microchemical reactions, of the methods for the mechanical separation of minerals, so as to insure pure material for analysis, and directions for the course of analysis to be pursued in the case of the more important minerals.

The important subject of mineral synthesis receives more complete treatment than any other in the book. The section contains general accounts of the various methods for the artificial production of minerals by chemical reactions, fusion, sublimation, electrolysis, diffusion, &c., with detailed descriptions of the apparatus required.

The sections on the metamorphism of minerals, and on the formation of minerals in nature, will be found of great interest to the petrologist. Here are described the effects on minerals of heat, of gases at high temperatures, of fusion, of fused magmas, of water containing carbonic acid, &c. In the last section, dealing with the composition and constitution of minerals, the present imperfect state of our knowledge is brought prominently to light. The battle is still being fought between the so-called chemical, liquid, and crystal molecule; between constitutional and empirical formulæ. Mineralogists are beginning to understand that it is impracticable to attempt to use for complicated minerals principles which are only applicable to volatile organic compounds, and the idea is gaining ground that many minerals are molecular compounds only capable of existing in the solid state, the crystal molecule being built up of different chemical molecules.

The author intends to supplement the present work by another, entitled "Chemical Mineralogy," in which the composition, synthesis, &c., of each individual mineral will be treated more particularly. The present volume is intended as quite a general treatise on the subject of mineral chemistry; in fact, we cannot help thinking that in many parts the treatment is far too general, and that the book has been partially sacrificed for the sake of the volume that is to follow. The value of the book is increased by the lists of references to the literature which precede each section. G. T. P.

OUR BOOK SHELF.

Bush Friends in Tasmania: Native Flowers, Fruits, and Insects, drawn from Nature, with Prose Descriptions and Illustrations in Verse. By Louisa A. Meredith. Executed by Vincent Brooks, Day, and Son. (London and New York: Macmillan and Co., 1891.)

UPWARDS of thirty years ago Mrs. Meredith gave the world a volume containing admirable coloured figures of a selection from the many beautiful plants and insects that inhabit her island home, Tasmania; and now, in the evening of a long life, she has travelled to the old country to publish a second volume, which is to be the last. Her purpose achieved, she "hopes to return and end her days among her children in that pleasant colony," which has given a brighter home to so many of our kith and kin. Lovers of the beauties of Nature in this country will find much pleasure and instruction in this second volume from that talented lady's pen and pencil, and will be able thereby to form some conception of the totally different kind of vegetation from our own that clothes this remote southern island, as well as the great Australian country, for it is only a part of the same flora. To the colonists themselves the book will be even more attractive, as a means of becoming acquainted with the names and affinities of the beautiful objects with which they are surrounded. It will also, it is to be hoped, teach them to prize and preserve these rare and precious gifts. Like all true lovers of Nature, Mrs. Meredith deplores the wanton destruction of rare flowers near Hobart by thoughtless or greedy persons whose only aim seems to be quantity.

The botanical part of Mrs. Meredith's book is perfectly trustworthy, having been scrutinized by so eminent an authority as Sir Joseph Hooker; and Prof. Westwood furnished the names of the insects.

Some of the poems have a special interest in connection with the early history of the settlement of Tasmania. Notably an "Old Story" of 1834, which narrates the massacre by aborigines of a whole family—father, mother, and seven children.

The Elementary Geometry of Conics, with a Chapter on the Line Infinity. By C. Taylor, D.D. (Cambridge: Deighton, Bell, and Co., 1891.)

DR. TAYLOR'S "Geometry of Conics" is so well known, and has met with such acceptance—this is the seventh edition, revised—that we are not called upon to give a detailed account of it. Two additions, however, claim a brief notice. A new chapter (xii.) contains "a course for beginners," in which students who prefer to take the three conics separately have a selection of articles, from the text, indicated for a first reading. Further, a set of duplicate proofs is given in outline, the completion of which is left to the reader. The other novelty (chapter xi.) is "a new treatment of the hyperbola." This is the expansion of a paper which the author read before the Association for the Improvement of Geometrical Teaching, in January 1890, and of which the President (Prof. Minchin) is reported to have said: "One thing that struck him about the paper was, that Dr. Taylor arrived at points on the curve in a very much more rapid and simple way than any he had previously known of." The author remarks that it is in accordance with the historical order to draw the asymptotes before tracing the curve, for the hyperbola seems to have been discovered from its "equation" (A.I.G.T. Report, 1890, p. 12).

It is somewhat remarkable that Dr. Taylor does not give a proof of this equation. We append one. Taking his figure on p. 103, we draw the second asymptote. Now draw PM parallel to Cp, cutting the axis in K, and the second asymptote in M: then,

$$\begin{aligned}4\text{CM} \,.\, \text{MP} = 4\text{MK} \,.\, \text{MP} &= (\text{MP} + \text{MK})^2 - (\text{MP} - \text{MK})^2 \\ &= \text{C}p^2 - \text{KP}^2 = \lambda^2(p\text{N}^2 - \text{PN}^2) \text{ (where } \lambda \text{ is a constant)} \\ &= \lambda^2(\text{S}p^2 - \text{SP}^2) \\ &= \lambda^2(\text{S}p^2 - p\text{Y}^2) = \lambda^2 \,.\, \text{SY}^2 = \text{C}a^2 = a^2 + b^2.\end{aligned}$$

Again, let PQ be any chord meeting the asymptotes in p, q; and let Ql, Pm, parallel to Cp, Cq respectively, meet those lines in l, m. Then we have

$$\frac{\text{P}q}{\text{C}m} = \frac{\text{P}p}{pm} = \frac{pq}{\text{C}p} = \frac{\text{Q}q}{\text{Q}l},$$

$$\therefore \quad \frac{\text{P}q}{\text{Q}q} = \frac{\text{C}m}{\text{Q}l} = \frac{\text{C}l}{\text{P}m} = \frac{p\text{Q}}{\text{P}p};$$

hence

$$\text{P}p = \text{Q}q, \text{ and } \text{P}q = p\text{Q}.$$

Other properties occur to us, but the above are classic properties of the curve, and the wonder is that Dr. Taylor has not applied his new treatment to obtain them. There is no suggestion that they can be so obtained, either in the book or the original paper as printed in the A.I.G.T. Report. R. T.

Les Engrais Chimiques. Par Georges Ville. Septième Édition. (Paris: M. Engel, 1890.)

THIS is a new edition of the author's lectures on chemical manures, which were first published in 1868, and which have been translated into seven languages. An English edition, by Mr. Crookes, was published in 1879. The sixth French edition has been out of print for about ten years, and during that time the price of chemical manures has considerably declined, on an average about 40 per cent. On this account the author has introduced, at the end of the volume, a chapter containing new formulæ for mixed manures, based on considerations of market value and more complete knowledge of the requirements of crops. Thus, potassium chloride replaces potassium nitrate in the manure for leguminous plants, and in some cases a mixture of potassium chloride and ammonium sulphate replaces potassium nitrate; and a few other alterations are suggested in the treatment of various crops. Thomas's basic cinder is not mentioned as a source of phosphoric acid. The lectures themselves, and some controversial matter, are reprinted in their original form, and but little new matter is added.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Bird-Collections in the Oxford University Museum.

DURING a recent visit to Oxford I took the opportunity of examining the collection of birds in the University Museum, and beg leave to offer a few remarks upon its condition.

First, as regards the mounted specimens, there are three series belonging to this category:—

(1) The general series in the Central Court. This numbers about 1100 specimens, which are contained in twelve cases, placed in opposite rows of six each, but rather mixed up with mammals, shells, and other objects. The specimens are arranged according to Gray's "Genera," and in most cases correctly named. But many of them are in bad order and not well set up, and should be replaced by fresh examples. The whole series requires renovation and rearrangement, according to some modern system, and the orders and families should be designated by labels, and distinctly separated one from another.

(2) The collection of Arctic birds formed by Mr. J. Barrow, F.R.S., and presented to the Museum by that gentleman. This interesting collection, which has been well described by Mr. Harting in the *Ibis*, is placed in the gallery. It is well mounted and correctly named. But it is a question whether it is desirable to keep it apart from the general series.

(3) The British series, also placed in the gallery, which is in fair order, although it also requires revision and rearrangement according to some modern system. It ought not to be difficult to find some member of the British Ornithologists' Union to undertake this task, provided that the authorities will allow him a "free hand."

Besides the mounted specimens, there are, as I understand, about 4000 skins of birds, most of which are "put away" in boxes in various parts of the building. Of these, the only portion that I was able to see was the Bornean collection formed by Mr. Everett, and partly described by Dr. Bowdler Sharpe in the Zoological Society's Proceedings. These are placed in some drawers in the main hall. The other skins are stated to be "boxed up," and are kept partly in a room on the ground floor, and partly in some "upper chamber," to which no ready access is possible.

I venture to suggest that one of the side rooms in the Museum should be cleared of its contents, and devoted entirely to the bird-skins, and that they should be arranged there in cabinets, so as to be accessible to the ornithologist. It is hardly right for a great and rich University to accept collections from persons who, in the words of the late Prince Bonaparte put forward on a similar occasion, "croyant qu'ils travaillaient pour la science, non pas travaillés que pour les mites." I may add that any assistance that I can give in carrying out this reform will be most gladly rendered. P. L. SCLATER.

3 Hanover Square, London, W., September 4.

Variation and Natural Selection.

IN Prof. C. Lloyd Morgan's Presidential address to the Bristol Naturalists' Society, on "The Nature and Origin of Variations" (of which he has kindly sent me a reprint from the Society's Proceedings), there are one or two points on which there seems to me to be a slight misconception; and as the difficulties suggested have probably occurred to other naturalists, I wish to make a few observations in the hope of throwing a little light on this obscure subject.

After referring to the proofs of the variability of species in a state of nature which I have adduced in my "Darwinism" (to which proofs Prof. Lloyd Morgan has made some important additions in his recent work on "Animal Life and Intelligence") he remarks:—"We have been apt to suppose that a species is so nicely adjusted to its surrounding conditions that all variations from the type, unless of a very insignificant character, would be rapidly and inevitably weeded out. This, it is clear, is not true at any rate for some species." And a little further on, after discussing the question whether variations in all directions occur in equal proportions—an equality which does not appear to me to be at all necessary, or to have been ever suggested as occurring—he says: "And the candid biologist must, I think, admit that the evidence in Mr. Wallace's third chapter, while conclusive as to the occurrence of variations, gives on analysis little or no evidence of any selective agency at work."

The difficulties here stated appear to me to depend, chiefly, on not taking account of some important facts in nature. The first fact is, that the struggle for existence is intermittent in character, and only reaches a maximum at considerable intervals, which may be measured by tens of years or by centuries. The average number of the individuals of any species which reach maturity may be able to survive for some years in ordinary seasons or under ordinary attacks of enemies, but when exceptional periods of cold or drought or wet occur, with a corresponding scarcity of certain kinds of food, or greater persecution from certain enemies, then a rigid selection comes into play, and all those individuals which vary too far from the mean standard of efficiency are destroyed.

Another important consideration is that these epochs of severe struggle will not be all of a like nature, and thus only one particular kind of unbalanced or injurious variation may be eliminated by each of them. Hence it may be that for considerable periods almost all the individuals that reach maturity may be able to survive, even though they exhibit large variations in many directions from the central type of the species. During such quiescent periods, the chief elimination will be among the young and immature. Thus, with birds probably nine-tenths of the destruction occurs among the eggs and half-fledged young, or among those which have just escaped from parental care; while those which have survived to breeding age only suffer a slight destruction in ordinary years, and this may occur partly among the less experienced, partly among those which are old and somewhat feeble.

The severe elimination that occurs in the earlier stages may be thought to be accidental, but I doubt if it is really so except in a very small degree. The protection and concealment of the eggs and young in the nest will depend chiefly on the mental qualities or instincts of the parents, and these will have been always subject to a rigid selection owing to the fact that those with deficient instincts will leave fewer offspring to inherit their deficiency. And with young birds of the first year there will be an equally rigid selection of the incautious, and of those who are deficient in any of the sense-perceptions, or are less strong and active than their fellows.

The proof that there *is* a selective agency at work is, I think, to be found in the general stability of species during the period of human observation, notwithstanding the large amount of variability that has been proved to exist. If there were no selection constantly going on, why should it happen that the *kind* of variations that occur so frequently under domestication never maintain themselves in a state of nature? Examples of this class are white blackbirds or pigeons, black sheep, and unsymmetrically marked animals generally. These occur not unfrequently, as well as such sports as six-toed or stump-tailed cats, and they all persist and even increase under domestication, but never in a state of nature; and there seems no reason for this but that in the latter case they are quickly eliminated through the struggle for existence—that is, by natural selection.

One more point I will advert to is Prof. Lloyd Morgan's doubt, in opposition to Mr. Ball, "whether a thicker or thinner sole to the foot is a character of elimination value, whether it would determine survival or elimination, and make all the difference between passing or being plucked in life's great competitive examination." This seems to me to be a rather unfortunate objection, since, in constantly recurring circumstances during the life of a savage, this very character must be of vital importance. Whether on the war-path, or in pursuit of game, or when escaping from a human enemy or from a dangerous animal, the thickness of the sole, its insensibility to pain, and its resistance to wear and tear must have often determined life or death. A man who became sore-footed after a long day's tramp, or one whose thin sole was easily cut or torn by stones or stumps, could never compete with his thicker soled companions, other things being equal; and it seems to me that it would be difficult to choose a single physical character whose variations would be more clearly subject to the law of selection.

With the greater portion of Prof. Lloyd Morgan's very interesting address I am in perfect accord, and it is because his remarks and suggestions are usually so acute and so well founded that I have thought it advisable to point out where I think that his objections have a less stable foundation.

Alfred R. Wallace.

A Rare Phenomenon.

The rare phenomenon to which your two correspondents refer in their letters in your last issue (p. 494) was visible here at precisely the same time, and, viewed from Nottingham Forest, it presented a most interesting sight. It is curious that, as both the time and duration of the phenomenon coincide with its appearance here, its characteristics should be so dissimilar. It had more the appearance of a well-defined display of the aurora. Rays of light springing from the horizon penetrated high into the heavens, lasting about 10 or 15 seconds, and then disappeared, others taking their places. Its centre appeared to me to be almost due north, and, from notes made at the time, the beams or luminous rays reached an angle of about 50°, stars being visible through them. There was no arc visible of the character described by your correspondents, but vertical changing rays, several of which were distinctly orange-tinted.

Nottingham, September 26. Arthur Marshall.

Your columns record, from Ireland and Scotland, observations of the aurora to which I called attention last week. It was seen also in Warwickshire, the coruscations being so marked as to remind my informant of the search-light at the Naval Exhibition. Mr. E. B. Knobel informs me that, from 8 to 10 p.m. on the 11th, during which time the appearance was visible, active magnetic disturbances were noticed at the Royal Observatory, Greenwich, illustrating the close connection which has been established between auroral and magnetic phenomena.

W. Tuckwell.

It may be of interest to your readers to know that the "rare phenomenon" mentioned (p. 494) was seen by me from Ryde, I.W., on Friday, the 11th. A streak of light (at first thought to be a ray proceeding from a search-light), was visible near the Pleiades, at about 9.30, extending over an arc of about 45°, the width being probably about 1°. It gradually faded away, and at 10 no trace of it was left. F. C. Levander.

30 North Villas, Camden Square, N.W., September 28.

Instruments in Just Intonation.

As you have raised once more the question of justly intoned instruments, may I offer the following remarks? It does not seem likely that any arrangement for the organ would be practically adopted unless it permits as much freedom of modulation and of execution as that of equal temperament. To permit perfectly free modulation, with practically perfect intervals, nothing short of the cycle of fifty-three will suffice. Now to construct a key-board with fifty-three notes to the octave which can be played upon with the facility of a twelve-note key-board seems impossible. But the problem may be approached differently: as it is only necessary to use twelve notes at a time, the key-board might remain as it is, and only a mechanical device would be required to make these twelve keys correspond to the right twelve out of fifty-three pipes; if the services of an assistant be allowed (as is often necessary on large organs) the mechanical difficulties could easily be overcome. For example, arrange a number of studs—say 117, as suggested by Dr. Ellis—as a "duodenarium," and connected electrically to the fifty-three trackers; *i.e.* each tracker would be connected to two or three studs—$B^3\flat\flat$, $C^4\flat\flat\flat$, $A_3\sharp$ studs to tracker 46 for instance. Opposite these studs would be another set of 117 connected to the twelve keys, *e.g.* C, $B_2\sharp$, $B_3\sharp$, $D^2\flat\flat$, &c., all to the key C. Between the two sets of studs would be a frame carrying twelve contact pieces; the frame would then be moved along guides by the assistant, so that the twelve keys were electrically connected to the right duodene of studs, and hence could be made to open the right group of pipes.

Thus the only alteration in printing required would be to mark the duodene on the music. All the extra complication would be thrown on the mechanical arrangements, and the organist would be left in the same position as now. It seems to me that any more complicated key-board would fail in a large organ, through overburdening the organist.

Robt. A. Lehfeldt.

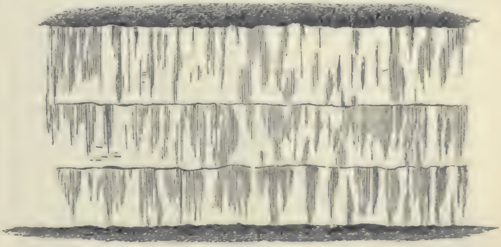
Firth College, Sheffield, September 14.

Unusual Frost Phenomenon.

The following is extracted from a letter dated Dubbo Creek, near Tumut, New South Wales, July 26, 1891:—

"I noticed the other day a strange effect caused by the late very hard frosts. It was a peculiar upheaval of the crust of the ground by a mass of innumerable threads of ice taking the form of spun glass or fine asbestos fibre. There were five layers of this ice-fibre, the uppermost bearing the raised earth-crust. Every night's frost was shown by its distinctive layer of fibres.

"As perhaps you may never have seen this form of ground frost, I append a rough sketch of its very singular appearance.



I have only shown three layers; there were five, but this may give you some idea of its appearance—quite a columnar basaltic appearance.

"Every morning here after a sharp frost, the whole of the ground, where not covered by grass or rubbish, is raised up thus. On the sides of the cuttings and banks of our claim, these ice-fibres may be seen projecting from the walls in bunches of snowy filaments, like spun glass. The sun, however, soon causes them to drop off, and they lie in heaps of some six inches in depth." A. H. White.

Richmond, Surrey.

The Destruction of Mosquitoes.

On two occasions, when proceeding northwards to Arctic Norway, I was much interested in observing the fact that the plague of mosquitoes, which is so intolerable there, especially prevails in latitudes beyond the northern range of the swallow.

This may possibly be a mere coincidence, but I think it is not—an opinion strongly supported by another and very broad fact, viz. that in a given district in our own country the gnats become more abundant immediately after the departure of the swallows, martins, &c. If this view is correct, the protection of these birds should be added to the devices named in your review of "Dragon-flies *v.* Mosquitoes." Such protection is very different from the indiscriminate sentimentalism about "small birds" which breaks out periodically at this season in the newspapers, and includes such feathered vermin as the thick-billed, seed-grubbing, pea-shelling, graminivorous sparrow among the objects of its tenderness. W. Mattieu Williams.

The Grange, Neasden, N.W.

A Tortoise inclosed in Ice.

DURING the last winter there was a good deal of correspondence in the columns of NATURE regarding the revivability of fish and insects that had been frozen hard. A similar phenomenon with regard to the tortoise having recently come under my notice, it may perhaps be interesting to some of your readers to have it put on record.

Some friends of mine have one of the small water-tortoises that are occasionally exposed for sale in the City. Last winter, this tortoise was inadvertently left in his small pond, the water of which froze completely into one block of ice, inclosing the tortoise. When the thaw came, the creature was found alive and flourishing. I especially endeavoured to ascertain whether the tortoise had been absolutely and completely *inclosed* in his icy casing, or whether he had been simply frozen into the ice, but partly inclosed and partly free. Unfortunately, however, in spite of cross-examining several of the family, I was unable to obtain a perfectly clear and definite statement on this point: one of my friends, however, declared that, if not completely encased, at any rate only the arch of the tortoise's back was free. This is, however, sufficiently indefinite to debar one from asserting that all access of air was denied to the tortoise; and that is the point on which my interest chiefly centred.

F. H. PERRY COSTE.

7 Fowkes Buildings, Great Tower Street, E.C.,
September 25.

The Soaring of Birds.

I HAVE read with much interest Mr. Peal's account of the soaring of vultures, pelicans, adjutants, &c., over the plain of Upper Assam (NATURE, May 21, p.56). Their manner of flight is identical with that of seagulls and harriers over the Canterbury Plains in New Zealand, which is about 150 miles long and 45 wide in its widest part. These birds begin to soar at a height of about 200 feet, and rise in slanting spirals to 2000 feet and under. The gulls are much the most numerous, and flocks of them may be seen soaring nearly every fine day in summer. Sometimes a number assemble, and after going round in circles for a short time, without rising, or rising very little, they come down, the condition of the air being apparently unfavourable for soaring. Whenever I have seen a flock finish an ascent, they all reached the same height, which is consistent with the supposition that they go as high as they can. They never remained at the limit of their ascent even for a short time, but separated, sailing away downward to great distances.

The explanation of soaring given by Mr. Peal can hardly be the true one. Bishop Courtenay has shown its inadequacy by proving that a bird in a uniform horizontal current is in no respect more able to support himself than in a calm. Though carefully looking for it, I have never been able to see the descent which Mr. Peal supposes to be made (he does not say that he has seen it) when the bird is going with the wind.

The soaring of birds shows plainly that the velocity of the wind over a flat country does not increase with the height in a perceptible degree up to great heights. If there were such an increase at anything like the rate near the ground, a bird soaring would be out of sight long before he could reach 1000 feet, but birds seem to drift horizontally at nearly the same speed during the whole of their ascent. The increase of the velocity of the wind with the height may be studied by observing the behaviour of smoke or steam carried along: near the ground the increase is easily seen, over 20 feet it is very small, over 50 seldom perceptible, a wreath of smoke over that height being carried along without any relative motion of the parts, or so little that it could be of no use in soaring.

In a description of the sailing flight of the albatross (NATURE, vol. xl., p. 9) I mentioned that when the wind is at right angles to the course of a steamer attended by a flock of albatrosses, some of them occasionally follow the vessel not far astern in undulating lines, rising against the wind and falling with it, and turning alternately right and left; also that seagulls do an imperfect imitation of this kind of flight over flat country, nearly touching the ground at each descent, as the albatrosses nearly touch the sea. The gulls are evidently unable to reach the height from which the previous descent was made without flapping their wings a few times during the second half of each ascent. Without doing this, they would soon come to the ground, though using the differential motion of the air, where it is at its maximum, to the greatest advantage possible. It seems, therefore, that soaring at great heights cannot be explained on the same principle as the sailing flight of the albatross, whose movements are confined to a comparatively thin stratum of air next the sea, in which the velocity of the wind increases rapidly with the height.

In Lyttelton Harbour, N.Z., which is surrounded by hills except at the entrance, the gulls soar by using the upward current on the slopes, rising in spirals in precisely the same manner as when soaring hundreds of feet above the plain. The motive power in the former kind of flight is evident, and perhaps throws light on that of the latter. Standing on a slope of about 20°, and about 100 feet above the sea, I saw a flock of gulls sitting on the water. A breeze sprang up, and the whole flock began to ascend over the slope. Being constantly among the shipping they are very tame, and several came within 12 feet of me. When moving against the wind their motion with respect to the earth was very slow, so that I had a good opportunity of seeing if there was any vibratory movement of the wings, but no movement of any kind was visible. The ascent of birds over a slope by means of the current flowing up it, and their descent in long inclines at a small angle with the horizontal, show that rapid motion through the air causes a great resistance in opposition to gravitation, which resistance has not yet, I believe, been accounted for quantitatively on mechanical principles.

The explanation of soaring at great heights which presents the fewest difficulties seems to me to be—that it is done by means of upward currents. This has been suggested by several observers, its main difficulty being the uncertainty that there are such currents of sufficient strength. I shall try to show that upward currents may be caused in two ways, but it would not be possible to give a direct proof that the currents so arising are strong enough. If, however, birds are seen to soar when one or other of these causes is present, there is a strong probability that they are true causes of soaring.

Everyone who has watched the working of a windmill must have seen that the force of the wind varies frequently, and sometimes rather suddenly. It is evident that there must be an ascent of air in front of a current moving faster than the average speed, and a descent of air behind it. As an example of this, a cold south-west wind was blowing, with showers of rain at intervals, accompanied, as often happens, by increased force of the wind. I saw a flock of gulls soaring in front of one of these squalls. There can, I think, be little doubt that there was an ascending current, of which the gulls took advantage.

Mr. W. Ferrel has shown ("Popular Treatise on the Winds") that if the rate of fall of temperature with increase of height be greater than the rate of dynamical cooling of an ascending current, the atmosphere is in an unstable state—that is, if by any cause a mass of air be started in an upward direction in such an atmosphere, the density of the ascending air is less than that of the surrounding still air, so that the former would be driven upwards, and an ascending current established, which would tend to rush up to the top of the atmosphere if the instability, consequent on the vertical decrease of temperature, should extend all the way up; but if the instability did not extend to the top, then, at its limit, the impelling force would cease, and friction would soon bring the ascending current to rest. Conversely, in an unstable atmosphere, if a mass of air be started downward, the density of the descending air is greater than that of the surrounding still air, and the descent tends to continue down to the ground. Mr. Ferrel says (p. 440):—"The unstable state in unsaturated air occurs mostly on very dry and sandy soils with little heat conductivity, when the weather is very warm, and the heat rays of the sun are unobstructed by any clouds above. The heat thus accumulates in the surface strata of the soil and the lower strata of the atmosphere, and thus is brought about the unstable state, at least up to a low altitude, even in clear dry weather." And in speaking of what may be called a multiple tornado (p. 412): "As the tornado originates in air in the unstable state, it often happens that there is about an equal tendency in the air of the lower stratum to burst up through those above at several places in the same vicinity at the same time."

This tendency of the lower strata to burst up in separate spots may exist where the instability is much less than that required to cause a tornado, as in the case of a plain strongly heated by the sun, and in the absence of any gyratory motion round the centre of an ascending current, there would be no whirlwind, only a quiet ascent of air, in a slanting direction if there were any wind. Such ascending currents may be of small area, not much larger than the circles described by birds when soaring. It seems possible that the object of describing circles may be to keep within the ascending current, though it is true they sometimes describe circles when the ascending current is up a slope and not limited to a small area. If a plain much heated by the sun border on the sea, ascending currents will soon start a sea-breeze, and the cold air from the sea will soon restore the stability of the atmosphere. In summer the sea-breeze blows over the Canterbury Plains four or five days a week, beginning between 8 a.m. and noon. When delayed till near noon, the soil and lower strata of air are much heated, and as the previous nights are cool, the conditions for causing the unstable state are present. I long ago remarked that the best time to look out for soaring birds is at the commencement of the sea-breeze when it is late. Soaring is much oftener seen here in summer than in winter, and is, I believe, more common, and the species of soaring birds more numerous, and the birds larger, in hot than in cold climates—that is, in climates where the unstable state of the atmosphere is oftenest caused by the sun's heat.

Mr. Peal says: "That there are no uprushes of air I have fairly good proof in the small tufts of cotton from the *Bombyx malabaricum* which cross the field of my telescope when examining the Noga Hills at ten, twenty, or thirty miles; these are always beautifully horizontal at elevations of from 200 to 2000 feet, coming from the plains and hills to the north-east of us." The presence of light bodies at great heights seems to show that there *are* upward currents: no doubt uprushes of air at a large angle with the horizontal, and of considerable area, might be detected by a careful observer from the movements of small floating bodies, but upward slanting currents of small area might easily escape observation.

It is obvious that upward currents over a plain, caused either by variations in the velocity of the wind or by the unstable state of the atmosphere, must be almost insensible near the ground, and could not attain their full strength under a considerable height. This accounts for the fact that over plains birds do not begin to soar at less than about 200 feet. If soaring were possible in a uniform horizontal current, they would save themselves the muscular effort of rising 200 feet and over by the active use of the wings, and would begin to soar immediately on leaving the ground, as they do in currents blowing up a slope.

I have often observed gulls with extended motionless wings following a steamer in the same relative position for several minutes. In every case it was clear that they used the current diverted upwards by the hull. Before the upward energy of this current is exhausted, a fast steamer has gone a good many yards, so that a bird is supported at some distance astern. Also an upward current of considerable strength would flow off the mizen sail of a ship sailing near the wind and leaning over.

Christchurch, N.Z. A. C. Baines.

Rain-making in Florida in the Fifties.

The article on "Rain-making in Texas" (Nature, p. 473) recalled to my memory a passage of Dr. Th. Reye's book ("Wirbelstürme, Tornados, &c.," Hanover, 1872), in which (at p. 12 and following) the author in question translates quotations from J. P. Espy's "Second and Third Report on Meteorology, 1851, auf Befehl des Senates der Union gedrukt" (Reye's note at his p. 235; quoting also fourth Report, 1857). The facts related were observed by the surveying officers George and Alexander Mackay. They (in Florida) had at their disposal great quantities of rushes (saw-grass), which they set in flame, and the huge conflagrations were invariably followed by rain.

September 22. G. P.

A Dog Story.

The following dog story may interest your readers.

As I went to the train one morning, I saw a brown retriever dog coming full speed with a letter in his mouth. He went straight to the mural letter box. The postman had just cleared the box, and was about 20 or 30 yards off when the dog arrived. Seeing him, the sagacious animal went after him, and had the letter transferred to the bag. He then walked home quietly.

Putney, September 23. John Bell.

SOME NOTES ON THE FRANKFORT INTERNATIONAL ELECTRICAL EXHIBITION.[1]

II.

A Page of Modern History.

Electric transmission of power to great distances bids fair in the near future to change the whole commerce of the world, and yet the history of its development is all comprised within the last fourteen years. In a long paper read in the early part of 1877 before the Institution of Civil Engineers, "On the Transmission of Power to a Distance," the author, Prof. Henry Robinson (now the engineer to various electrical companies), does not even suggest the possibility of employing electricity for this purpose. So that in the discussion Sir William Siemens remarked, "He might also refer to another method of transmitting power to a distance, which did not seem to have occurred to the author, perhaps because it was of recent date, viz. by electric conductors."

A week later, Sir W. Siemens, in his Presidential address to the Iron and Steel Institute, throws out the idea of utilizing the power wasted in the Falls of Niagara; and after referring to the use of high-pressure water mains and quick-working steel ropes for transmitting power over one or two miles, he says, "Time will probably reveal to us effectual means of carrying power to great distances, but I cannot refrain from alluding to one which is, in my opinion, worthy of consideration—namely, the electrical conductor." And he adds, "A copper rod three inches in diameter would be capable of transmitting 1000 horse-power at a distance of, say, thirty miles."

The use of the electric current for the transmission of power over considerable distances was, therefore, fully present in the mind of Sir William Siemens in 1877, but not apparently the employment of the high potential differences which are absolutely necessary to make such a transmission commercially possible. For a copper rod of three inches diameter, such as he speaks of, has a cross-section of nearly seven square inches, and could carry some 5000 or 6000 amperes without undue heating. Therefore, even when the problem of transmitting 1000 horse-power over thirty miles was in question, he did not contemplate, apparently, using a pressure of more than about 100 volts.

At the commencement of the following year, 1878, in his Presidential address to the Society of Telegraph Engineers, he refers to his previous statement, and adds, "Experiments have since been made with a view to ascertain the percentage of power that may be utilized at a distance." The result obtained, he says, is that "over 40 per cent. of power expended at the distant place may be recovered"; but Sir William adds, in reference to the 60 per cent. loss, "This amount of loss seems considerable, and would be still greater if the conductor through which the power were transmitted were of great length."

The length of the conductor employed in the above experiment is not given, but its approximate length, as well as what is understood by "great length," may be gathered from the context; for Sir William goes on to consider the problem "of distributing the power of a steam-engine of, say, 100 horse-power to twenty stations within a circle of a mile diameter"; and although the distance to which it is proposed to transmit the power is only one mile, he assumes that the loss is what was found in the above experiment, viz. 60 per cent. He further adds, "The size of the conductor necessary to convey the effect produced at each station need not exceed half an inch in external diameter." Clearly, then, as the power proposed to be transmitted by the half-inch conductor to each station one mile distant was only 5 horse, there was no idea of using a potential difference in the transmission higher than that maintained between the terminals of a lamp.

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[1] Continued from p. 497.

Two wrong notions misled people in those days—the one, that the maximum efficiency of a perfect electromotor could be only 50 per cent.; the other, quoting the remarks of Sir W. Siemens in the discussion of the paper read by Messrs. Higgs and Brittle at the Institution of Civil Engineers somewhat later in the same year 1878, "In order to get the best effect out of a dynamo-electric machine there should be an external resistance not exceeding the resistance of the wire in the machine. Hitherto it had been found not economical to increase the resistance in the machine to more than one ohm; otherwise there was a loss of current through the heating of the coil. If, therefore, there was a machine with one ohm resistance, there ought to be a conductor transmitting the power either to the light or the electromagnetic engine not exceeding one ohm." He then goes on to consider that as the conductor is lengthened its cross-section must be increased in proportion to keep the resistance constant at one ohm; and he arrives at a result quite new at the time, viz. that if the number of dynamos *in parallel* were increased in proportion to the length and cross-section of the line, "it was no dearer to transmit electromotive force to the greater than to the smaller distance."

Sir William Thomson grasps at once the novelty and importance of this idea, and renders it even more important by proposing to put all the dynamos *in series* at one end of the line, and all the lamps in series at the other. But it would still appear that even 40 per cent. efficiency for transmision over a considerable distance could only be attained when "there were a sufficient number of lamps" to make it necessary to use *many* dynamos in parallel in accordance with Siemens's proposal, or, *many* dynamos in series in accordance with Thomson's modification of Siemens's proposal.

In 1879, the electric transmission of power was still such a *terra incognita* that the largest firm of electrical engineers in Europe could not be induced to tender for transmitting power over ten miles in India.

At the British Association lecture in the autumn of 1879, Prof. Ayrton exposed the fallacy of assuming that 50 per cent. was the maximum efficiency theoretically obtainable with an electromotor. He further proposed that, instead of employing many dynamos at one end of the line and many lamps at the other, there should be used a single dynamo and a single motor, with much wire on each; that the high potential of the line necessary for economical transmission of power should be maintained by running both dynamo and motor much faster than hitherto; and that both dynamo and motor should be separately excited. Although not wholly free from the prevailing idea of that day—that electric transmission of power over long distances would only be commercially possible when a very large amount of power had to be transmitted—he says, after discussing the subject, "So now we may conclude that the most efficient way to transfer energy electrically is to use a generator producing a high electromotive force and a motor producing a return high electromotive force; and by so doing the waste of power in the transmission ought, I consider, to be able to be diminished with our best existing dynamo-electric machines to about 30 per cent."

This was perhaps the first time that it had been even suggested that the efficiency in electric transmission of power could be more than 50 per cent.

Further, the lecturer proposed to use in all cases this high E.M.F. motor, whether the received power were required for motive purposes, for light, or for electroplating; and, as experimentally shown in the lecture, to generate the current locally in the two latter cases by using the motor to drive a suitable dynamo, thus giving the first illustration of the employment of an electric transformer in the actual transmission of power to a distance.

Two years later, viz. in 1881, the old mistaken notion, that it was only 50 per cent. of the power given to a dynamo that could be returned by the motor, was again propounded during a discussion at the Society of Arts; and the Chairman, Sir W. Siemens, when correcting the speaker's error, added, "Experiments of undoubted accuracy had shown that you could obtain 60 or 70 per cent."

In this year two very important propositions were put forward—the one, by Sir W. Thomson, at the semi-centenary meeting of the British Association, that, in the electric transmission of power, the small current of high potential difference should be employed at the receiving end of the line to charge a large number of accumulators in series, the accumulators being subsequently discharged in parallel for supplying light or power to a town; the other, by MM. Deprez and Carpentier, to use one alternate current transformer at the sending end to raise the electric pressure, and another transformer at the receiving end to lower it down again, the arrangement being symbolically shown in Fig. 1.

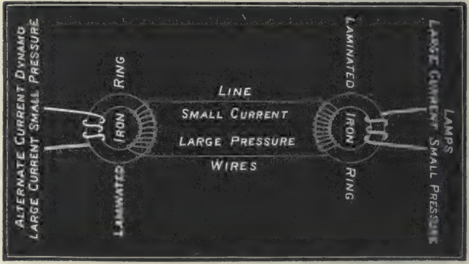

FIG. 1.—Deprez and Carpentier's Plan of Double Transformation.

The great advantage of this combination is, that the pressure along the line may be very high, and the line therefore composed of only thin wire, whereas the pressure between the leads from the generating dynamo at the transmitting end, as well as the pressure between the lamp mains at the receiving end of the line, may be as low as if the dynamo and lamps were close together.

In the experiments, however, made in the following year, 1882, to transmit power from Miesbach to Munich, along thirty-five miles of iron telegraph wire 0·18 inch in diameter, the current going by one wire and returning by another, M. Deprez did not employ his double transforming arrangement described above, probably because alternate current motors were then quite untried practically. But, instead, he used a direct current dynamo generating a potential difference of some 1500 volts, the current from which set in motion a direct current motor, wound to stand a similar high pressure, placed at the other end of the telegraph line.

The experiments were attended with various breakdowns of the dynamo, which was probably constructed on the usual string-and-glue fashion of those days; and finally, after repairs had been effected, the power given out by the motor at Munich was only a fraction of 1 horse, with a commercial efficiency of about one-third.

It was, therefore, decided to repeat the experiments the next year, 1883, with machines constructed more solidly, and for the convenience of the jury the dynamo and motor were placed close together in the workshops of the Northern Railway near Paris, one terminal of each being connected by a short wire, and the other terminals by a telegraph wire 0·157 inch thick going from Paris to Bourget and back again, a distance of 18,133 yards. The power used in driving the dynamo was towards the end of this second set of experiments about 10½ horse, and the power given out by the motor about 3⅓ horse, the potential difference at the dynamo terminals being some 1850 volts.

The arrangement of the machines was very bitterly criticized: some pronounced the result a great success; others that the whole thing was a fraud, that the power did not go from the dynamo at Paris to Bourget and back again, but that, owing to leakage from one of the telegraph lines to the other, the actual distance over which the power was transmitted was far less than the distance stated.

The next experiments were made with the same machines rewound and improved in insulation. They were now employed to transmit power over 8¾ miles, from Vizille to Grenoble, a pair of silicium bronze wires 0·079 inch in diameter being used to connect the dynamo and motor. A difference of potential of about 3000 volts was employed, and 7 horse-power was given off by the motor with a commercial efficiency of 62 per cent.

This experiment of transmitting power from Vizille to Grenoble in 1883 was distinctly successful, and constituted a great advance on anything in electric transmission that had been attempted before. It is interesting, for example, to compare it with the transmission from Hirschau to Munich by Mr. Schuckert in 1882, and which was regarded as very striking at the time it was carried out.

Transmission of Power.

	1882. Hirschau to Munich.	1883. Vizille to Grenoble.
Distance in miles ...	3½	8¾
Diameter of conducting wire in inches ...	0·18	0·079
Horse-power delivered by electromotor ...	5·8	7
Commercial efficiency of the transmission...	36	62
Potential difference at terminals of dynamo in volts	700	3000

Comparing, then, the Vizille transmission of 1883 with the Hirschau transmission of 1882, we see that the distance was twice as great, the cross-section of the wire less than one-quarter, the power somewhat greater, and the efficiency nearly twice as great; this great improvement being effected by using a pressure of 3000 instead of 700 volts.

But with 3000 volts the limit of constructing the commutator of an ordinary direct current dynamo or motor is reached—a fact which was not appreciated by M. Deprez. For when it was decided somewhat later to try and transmit 200 horse-power through 35 miles of copper wire 0·2 inch in diameter, stretched on telegraph poles between Creil and Paris, by using a pressure of 6000 or more volts, the same system of *direct* current dynamo and motor, that had been employed by M. Deprez in his previous transmissions, was resorted to. The result was that the 200 horse-power had to be reduced to 100, and the dynamo and motor were burnt up time after time.

Eventually, after the expenditure of a very large sum of money, spent in several rewindings of the machines, &c., M. Deprez succeeded in 1886 in obtaining from the shaft of the motor at Paris 52 horse-power, this being 45 per cent. of the power spent in driving the dynamo at Creil. The power delivered at Paris was distributed by coupling a low potential difference dynamo to this motor, and using the current developed by this dynamo for driving various smaller motors, so that the power actually delivered to the pumps, &c., was somewhat less than the 52 horse stated above.

In the use of a dynamo and motor each with a high resistance armature and a low resistance field magnet, the fields being produced by separate excitation, and in the employment of a motor-dynamo for utilizing the received power, M. Deprez expressed his approval of the very plan proposed by Profs. Ayrton and Perry in 1879 for "sending by even quite a fine wire a small current," and so obtaining "an economic arrangement for the transmission of power."

This experiment, although very costly, had considerable interest, in showing that as much as 52 horse-power could be actually delivered at the end of thirty-five miles of copper wire 0·2 inch thick, and that a pressure of 6000 volts could be practically employed with a lead covered insulated conductor. But probably the most important lesson learned from it was, that when the distance over which power had to be transmitted was so great that a pressure of 6000 volts became necessary to obtain economy in the conducting wire, an alternating and not a direct current ought to be used.

While these various experiments of M. Deprez with direct currents were being carried out, the transmission of power by means of alternating currents had been progressing in the face of considerable opposition. The exhibition at the Aquarium, Westminster, in the spring of 1883, will probably be chiefly remembered from its being there that Messrs. Gaulard and Gibbs showed what they called a "secondary generator," which was simply an improved form of Ruhmkorff induction coil, without the ordinary vibrating make and break. A current from an alternating dynamo was sent round one of the coils, and to the terminals of the other were attached lamps, the brightness of which could be varied by pulling out the iron core of the induction coil more or less, as is done with medical coils to alter the strength of the shocking current.

Nobody thought much of the "secondary generator"; it seemed to have no very special use; the iron core felt very hot, so that there would be a new waste of power introduced into electric lighting by the use of secondary generators. Besides, the electricians saw that Messrs. Gaulard and Gibbs were employing methods and apparatus for measuring the power which must give totally erroneous results when used with alternating currents; and so, forgetful of the fact that invention is frequently quite ignorant of the language of the text-book, they decided that there was nothing in it.

But Messrs. Gaulard and Gibbs believed in their secondary generator, whatever electricians and the technical press might say; they put them at the Notting Hill Gate, Edgware Road, Gower Street, King's Cross, and Aldgate stations of the Metropolitan Railway, joined the fine wire coils of all the generators *in series* with one another, and sent a small alternating current through the whole circuit from a dynamo placed at Edgware Road. Lamps of different kinds attached to the thick wire coils of each of the generators at the five railway stations burned steadily and brightly; an alternate current motor, even, which was put at one of the stations, revolved rapidly: but what a great waste of power there must be in all this unnecessary transformation, said the learned.

Well, in the spring of the next year, 1884, Dr. J. Hopkinson tested the efficiency of these secondary generators on the Metropolitan Railway, and, to the surprise of nearly everyone, it came out close on 90 per cent.

In the autumn of the same year, in connection with the Exhibition at Turin, power was transmitted to Lanzo, twenty-five miles away, by means of a bare overhead wire rather less than one-quarter of an inch in thickness, and, by means of Gaulard and Gibbs's secondary generators, the power was distributed at Lanzo and elsewhere along the route, for lighting incandescent and arc lamps. The jury reported that the efficiency of the transformers was 89 per cent., the whole distribution strikingly successful, and a prize of 10,000 francs was awarded to Messrs. Gaulard and Gibbs by the Italian Government.

No electromotors, however, appear to have been driven by the transmitted power, for, even in 1884, alternating current electromotors were still comparatively untried.

Tests of a secondary generator were next undertaken in 1885 by Prof. Galileo Ferraris, of Turin, who found the efficiency at full load to be no less than 97 per cent.,—a value even higher than that previously published. This investigation is the more memorable, in that it led Prof. Ferraris to take up the mathematical and experimental investigation of alternating currents, resulting in the discovery and construction of the self-starting alternate current motor in 1885, and to extensions of considerable practical importance in our knowledge of the action of secondary generators, now called *transformers*. And so one of the chief lions this year at the Frankfort Exhibition was Prof. Ferraris. W. E. A.

(*To be continued.*)

THE GIRAFFE AND ITS ALLIES.

Although coming within that well-defined group of ruminants known as the Pecora, the Giraffe (the sole existing representative of the genus *Giraffa*) stands markedly alone among the mammals of the present epoch; although, on the whole, its nearest living relations appear to be the deer (*Cervidæ*). Moreover, not only is the giraffe now isolated from all other ruminants in respect of its structure, but it is also exclusively confined to that part of the African continent which constitutes the Ethiopian region of distributionists. When, however, we turn to the records of past epochs of the earth's history, we find that both the structural and distributional isolation of the giraffe are but features of the present condition of things. Thus, in regard to its distribution, we find that in the Pliocene epoch giraffes were abundant in Greece, Persia, India, and China; and we may therefore fairly assume that they were once spread over the greater part of the Palæarctic and Oriental regions. Then, again, with regard to their allies, the researches of palæontologists have been gradually bringing to light remains of several large extinct ruminants from various regions, which are more or less nearly related to the giraffe, but whose affinities appear to be so complex and so difficult to decipher, that not only do they remove the stigma of isolation from that animal, but even render it well-nigh impossible to give a definition of the group of more or less giraffe-like animals, by which it may be distinguished on the one hand from the deer (*Cervidæ*), and on the other from the antelopes (*Bovidæ*). Since an interesting account of a new extinct Giraffoid from the Pliocene deposits of Maragha in Persia has been recently given by Messrs. Rodler and Weithofer in the *Denkschriften* of the Vienna Academy, the present time is a suitable one to offer a brief *résumé* of the present state of our knowledge of this group of animals, and the different views which have been entertained as to the affinities of some of its members.

Among the chief structural peculiarities of the giraffe, the most noticeable is its great height, which is mainly produced by the excessive length of the neck and limbs. The fore-limbs are, moreover, longer than the hind ones, as is well shown by the circumstance that the radius, or main bone of the fore-leg, is longer than the tibia in the hind-leg; whereas, in other living ruminants the reverse condition obtains. The skull is more like that of the deer than of any other existing ruminants, this being shown by its general contour, and also by the presence of the large unossified space below the eye, which completely separates the lachrymal from the nasal bone; a condition but very rarely met with in the *Bovidæ*, although found in the skull of the water-buck. Then, again, the skull resembles that of the deer in the great elongation of the portion situated behind the eyes, *i.e.* the parietal region. The bony processes arising from the skull between the occiput and the eyes, and clothed in the living animal with skin, are not strictly comparable either with the antlers of the deer or the horn cores of the antelopes; in the young condition they are separate from the bones of the skull, with which, however, they unite as age advances. The whole of the frontal and nasal region is much swollen and inflated by the development of air-cells between the inner and outer layers of bone; and at the junction of the frontal and nasal bones there is a large oval hillock-like protuberance in the middle line, which is sometimes termed a third horn. This excessive inflation of the region of the face makes the appearance of this part of the skull very different from that of the deer, in which it is much flattened. The grinding or molar teeth of the giraffe are remarkable for the peculiar roughness of their external coating of enamel, and also for their broad and low crowns, which in the upper jaw lack the internal additional column occuring in those of most deer and many antelopes. These teeth are, however, more like those of the deer than those of other ruminants, although they can be distinguished at a glance from all others except the larger ones of the under-mentioned fossil forms.

Since a good deal depends on the similarity between the structure of the molar teeth of the giraffe and those of the extinct ruminants in question, it may be well to observe that the characters of the molar teeth among all the ruminants are of great importance in classification. Thus, these teeth in all the deer, although varying to a certain extent in the relative height of their crowns, present the same general structure, those of the upper jaw being comparatively short and broad, with a large internal additional column. Then, again, in the *Bovidæ* we may notice that each of the several groups into which the antelopes are divided, as well as the goats and sheep and the oxen, are severally distinguished by the characters of their molar teeth, and that, although the teeth of one group may approximate more or less closely to that of another, we do not find any instances where one member of a group possesses teeth of a totally different type from those of the other representatives of the same group. These facts strongly indicate that, when we meet with fossil ruminants having molar teeth of the very peculiar type met with in the giraffe, we shall be justified in considering that there must be a certain amount of relationship between the owners of such teeth.

Another marked peculiarity of the giraffe is that the humerus has a double groove for the biceps muscle, instead of the single one found in ordinary ruminants. In regard to its soft parts, the giraffe resembles the deer in the usual absence of the gall-bladder, although its reproductive organs are constructed more on the Bovine type.

With these preliminary remarks on some of the structural peculiarities of the giraffe, we may proceed to the consideration of its fossil allies. The genus which probably comes nearest to the giraffe is the imperfectly known *Vishnutherium*, founded upon part of a lower jaw from the Pliocene of Burma, but to which have been referred some upper molars and bones from the corresponding beds of the Punjab. This animal must have been considerably larger than the giraffe, and the upper molars are remarkable for the great flatness of the outer surfaces of their external columns, in which respect they come nearer to the corresponding teeth of the elk than do those of any other members of the group. The posterior cannon-bone, or metatarsus, assigned to this genus, although relatively much shorter than that of the giraffe, is more elongated and giraffe-like than the corresponding bone of any other fossil genus in which this part of the skeleton has been described. The cervical vertebræ are also more elongated and giraffe-like than those of any of the under-mentioned genera. It will of course be immaterial if these bones prove to belong to a genus distinct from *Vishnutherium*; their interest lying in the circumstance that they indicate the existence of an animal to a great extent intermediate between the giraffe and the following genus.

The genus *Helladotherium* was established upon the remains of a large giraffe-like ruminant from the Pikermi beds of Greece, to which a skull from the Indian Siwaliks, which had been previously regarded as referable to the female of *Sivatherium*, proved to belong. The Helladothere, of which the entire skeleton is known, was a hornless animal, of larger size than the giraffe, but with much shorter and stouter neck and limbs. The skull approximates in many respects to that of the giraffe, having the same long parietal region, but with a minor development of cells in the frontals, and the important difference that there is no unossified space below the eye. The limbs agree with those of the giraffe in the great relative length of the anterior pair, as is shown by the radius being considerably longer than the tibia. That the Helladothere was not the female of the Sivathere seems to be evident from the absence in the Pikermi beds of the antler-like cranial appendages of the latter, which are comparatively common in the Indian Siwaliks. The intimate affinity existing between the Helladothere and the giraffe has been admitted by all who have written on the subject.

The animal recently described by Messrs. Rodler and Weithofer from the Persian Pliocene, for which the hybrid name *Alcicephalus* has been proposed, tends to connect the Helladothere with the deer, and more especially the elk. Thus, in the first place, the front and hind limbs are approximately equal, the length of the radius and ulna being nearly the same. Then, again, from the total absence of air-cells in the frontal region of the skull, the middle of the face is nearly flat, and the orbits have their frontal borders in the plane of the face, instead of considerably below it, as in the Helladothere, and still more so in the giraffe. There is, however, no unossified space in front of the eye; although the whole contour of the skull is strikingly elk-like.

The conclusion to be drawn from these hornless forms appears to be that they serve to connect the giraffe with less aberrant ruminants, and more especially the *Cervidæ*, and also that the unossified vacuity in the skull of the giraffe is probably an acquired feature, since it is absent both in the extinct giraffoid genera, and in the earliest deer, like the Miocene *Amphitragulus*. Both giraffes and deer may, therefore, probably have had a common ancestor more or less closely allied to the lower Miocene genus *Gelocus*.

Leaving now these hornless forms, as to the affinities of which there has been no dispute, we have to turn our attention to another group provided with cranial appendages of very curious and still imperfectly understood structure, in regard to whose relationship exceedingly different views have been entertained. This group, so far as we know at present, seems to be confined to the Pliocene of India and Persia, being represented in the former area by the gigantic *Sivatherium*, *Bramatherium*, and *Hydaspitherium*, and in the latter by the much smaller *Urmiatherium*. In all these animals the skull is characterized by the extreme shortness of the parietal region, and the position of the horns or antlers immediately over the occiput; the elevated facial profile thus produced being in very striking contrast to the straight one of the deer. In *Bramatherium* and *Hydaspitherium* the cranial appendages rise from a massive common base, and the latter genus is distinguished from all the others by the presence of an unossified space below the eye, corresponding to that of the giraffe. Their molar teeth are very similar to those of the Helladothere. In the Sivathere, on the other hand, there is one pair of large branching and palmated cranial appendages rising from separate bases immediately above the occiput; and in addition to these a pair of much smaller conical ones placed immediately over the orbits. In general appearance the large palmated appendages are more like the antlers of the elk than those of any other existing ruminants; but the absence of a "burr" at their base indicates that they were not deciduous, while the deep arterial grooves on their surface suggest that they were clothed either with skin or with a horny substance. The molar teeth conform to those of the giraffe—and to a less degree the deer—having the same rugose enamel; but the ridges on the outer surfaces of those of the upper jaw are more developed than in the other extinct genera. A peculiarly giraffe-like and cervine feature in these upper teeth is the extension of the anterior extremity of the anterior crescent far towards the outer side of the crown. Lastly, the humerus of the Sivathere resembles that of the giraffe in the presence of a double groove for the biceps muscle; while the form of the terminal bones of the feet is almost identical in the two animals. In the small Persian *Urmiatherium*, which is known only by the hinder portion of the skull, it appears that the cranial appendages consisted of a pair of unbranched, somewhat compressed, and upright processes rising immediately above the occiput.

With regard to the affinities of this group, it has been argued that the shortness of the parietal region of the skull, and the position of the cranial appendages immediately above the occiput, indicate affinity with certain African antelopes, such as the Sassabi and its kindred (*Alcelaphus*). In that group of antelopes it is, however, perfectly clear that the features in question are acquired ones; the allied Blessbok scarcely possessing them in any degree. Again, the straightness of the cranial axis in the skull of Waller's gazelle (*Gazella walleri*) shows that the arching of this axis, which is so characteristic of most antelopes, is likewise a feature specially acquired among that group of animals. Moreover, apart from this evidence, no one who thinks for a moment on the subject can believe that the Sassabi, with its narrow sheep-like molars and true horns, and the Sivathere, with its broad giraffe-like molars and cranial appendages, which are neither true horns nor true antlers, can be anything approaching to first cousins; and yet if they are not so, it is perfectly evident that the similarity in the structure of their skulls must have been independently acquired. It is therefore abundantly clear that no arguments based on these resemblances will hold water; the true explanation probably being that the superficial similarity of their skulls is solely connected with the support of cranial appendages having a similar position in both groups.

It follows from this that, if a type of skull with a short parietal region, a curved basal axis, and horns placed immediately over the occiput, has been independently developed among the antelopes from a type of skull with a long parietal region, a straight basal axis, and horns placed over the orbits, there is no conceivable reason why a similar line of development should not have taken place among giraffe-like animals. Taking, therefore, into consideration that the Sivathere and its allies have molar teeth like those of the giraffe, that their cranial appendages could be derived from those of the latter by special modification and development better than from those of any other group, that their humerus has a double bicipital groove, that the terminal phalangeals of their feet are giraffe-like, and that the proportions of their limbs are only a step beyond those obtaining in the admittedly giraffoid Helladothere, the evidence in favour of regarding these animals as greatly modified Giraffoids is so strong as to be almost a certainty. Indeed, it appears most probable that we ought to regard the Sivathere and its allies as holding a somewhat analogous position among the Giraffoids to that occupied among the antelopes by the Sassabi and its cousins.

The writer has purposely refrained from making any reference to the large unossified suborbital vacuity in the skull of the Hydaspithere, as reasons have already been given for regarding that feature as an acquired one. If, however, that view be incorrect, the presence of this vacuity at once stultifies the statement that the Sivathere can have no kinship with the giraffe and the deer, on account of the absence of a similar vacuity; and its presence, so far as it goes, is also another argument against the Sassabi theory.

The last representative of the Giraffoid animals that we have to mention is the recently discovered *Samotherium*, from the Pliocene of Samos, a figure of the skull of which appeared in NATURE, illustrating an article on the extinct mammals of those deposits. In this animal, the elongated form and straight profile characteristic of the skull of the Giraffe are retained; and the teeth are almost indistinguishable from those of the latter. There is, however, no development of air-cells in the bones of the frontal region, so that the upper border of the orbit is approximated to the plane of the face; and the cranial appendages take the form of upright compressed processes rising immediately over the orbits. These appendages, which appear to have been inseparable from the bones of the forehead, are, indeed, very similar, both in form and position, to the horn-cores of certain extinct antelopes, but we are, of course, unacquainted with the nature of their covering. If, however, as seems to be undoubtedly the case, the Samothere is a Giraffoid, it would seem that we must here again regard this superficial resemblance to the antelopes as one independently acquired.

Finally, if the views expressed above are anywhere near the truth, it would appear that, in the Pliocene epoch, Giraffoid animals played a very important *rôle* among the ruminants, and that they have undergone modifications and developments fully as marked as those which we observe among the antelopes at the present day. Whether the circumstance that none of them, except the giraffe (which is obviously an animal incapable of further modification), appears to have obtained an entrance into Africa has been the chief reason why only a single representative of the group has survived to our own times may be a fair subject of conjecture, since after the Pliocene epoch both India and Europe seem to have been unsuited to the maintenance of many forms of large Artiodactyle Ungulates, as is proved by the disappearance from those regions of the hippopotamus, the giraffe, and a number of antelopes of African type. R. L.

PHOTOGRAPHIC MAGNITUDES OF STARS.

THE character of the image of a star photographed on a sensitized film; the relation between the intensity of the light photographed and the blackened disk produced; the influence of the time of exposure on the image—are questions now receiving much attention. For this reason, Dr. Scheiner's contribution to the subject, embracing, as it does, the latest results of the Potsdam Observatory, is especially welcome; but these results will not be accepted without great reserve, contravening, as they do, a theory, or at least an assertion, that has been very generally accepted, viz. that increasing the intensity of light is exactly equivalent to increasing the time of photographic exposure. A consequence of such a law would be that an additional magnitude would be impressed on the film by increasing the time of exposure two and a half times the length.

Such a law cannot be rigorously exact, and its stoutest supporters have been careful to confine its application "within limits." But Dr. Scheiner's contention is that, owing to the complex character of the disk produced on the film, such a principle is a very unsafe guide, either as a rule for the determination of the feeblest magnitude impressed on the negative, or as offering a satisfactory explanation of the growth of the diameter or area.

In the first place, there is evidence of want of uniformity of actinic action throughout the whole extent of the stellar disk. A mean intensity (i) may be assumed at a certain distance (r) from the centre of the image, where the intensity is I. This centre will not be a geometrical point, but, owing to atmospheric and other disturbances, will occupy a small area of radius (ρ). The intensity (i) at distance (r) will depend materially on the increase of the area (ρ), which may be represented by $\psi(\rho)$. Consequently, the simplest expression for $i = \mathrm{I}\psi(\rho)e^{ar}$, where a is the coefficient of absorption of the sensitive film. On comparing two stellar disks, formed on the same emulsion, and treated by the same developer, this expression becomes

$$\frac{t_0}{t_1} = \frac{\mathrm{I}_1\,\psi(\rho_1)}{\mathrm{I}_0\,\psi(\rho_0)}e^{a(r_1-r_0)},$$

and, if the disks be on the same plate, $\rho_1 = \rho_0$ and $t_1 = t_0$, so that the formula can be simplified to

$$a(r_0-r_1) = \log\frac{\mathrm{I}_1}{\mathrm{I}_0}. = \frac{0{\cdot}4}{\text{mod.}}(m_1 - m_0)$$

In order to derive the relation between diameters and exposure, put $\mathrm{I}_0 = \mathrm{I}_1$, and then

$$\log\frac{t_0}{t_1} = a(r_1 - r_0).$$

It is not likely that such an expression has any other value than to serve as a convenient formula for interpolation. The variable character of a under different conditions, but always depending on the time of exposure, is shown by the following table:—

Exposure. m. s.	Instrument.	a.	Instrument.	a.
1 0	Reflector	4·99	5-in. refractor	4·12
2 0	,,	4·57	,,	5·09
4 0	,,	4·67	,,	5·47
8 0	,,	4·89	,,	5·89
16 0	,,	5·39	,,	7·51
0 24	13-in. refractor	3·18	13-in. refractor	2·67
1 0	,,	3·16	,,	2·20
2 30	,,	3·33	,,	2·48
6 15	,,	3·33	,,	3·00
15 38	,,	4·48	,,	—

Another well-known formula in which magnitude is made to depend on diameter is $m = a - b \log \mathrm{D}$, and in this case b is shown, notwithstanding Dr. Charlier's results to the contrary, to be a function of the time of exposure. The results are as follows:—

Time of exposure. h. m.	b Charlier.	Time of exposure. m. s.	b Scheiner.
0 13	6·719	0 24	5·17
1 30	6·779	1 0	6·35
2 0	6·683	2 30	7·06
3 0	6·814	6 15	8·08

The disagreement is conspicuous, but the explanation offered by Dr. Scheiner is scarcely satisfactory. He would ascribe the constancy in the value of b, found by Dr. Charlier, to the fact that in his experiments there is always a large absolute value of the time coefficient. It will, however, be observed that the ratio between Dr. Charlier's extreme exposures is not greatly different from that which obtains in Dr. Scheiner's experiments.

If it be admitted that the product of intensity by the time is *not* a constant quantity, it becomes a matter of great practical importance to determine what is gained on a photographic plate by prolonged exposure. This question forms the real investigation of Dr. Scheiner's two papers, and though some of his results may be questioned, yet the general issue is so grave and disquieting that it may not be utterly ignored. Passing over the details of his method of examination, and the precautions taken to insure accurate results, for which the reputation of the Potsdam Observatory is a sufficient guarantee, Dr. Scheiner presents the following table, in which is exhibited the faintest magnitude which, under certain varied circumstances, can be detected on a photographic plate:—

Time of exposure.		Faintest magnitude.			
m.	s.	Plate I.	Plate II.	Plate III.	Plate IV.
0	24	9·0	6·4	7·7	8·2
1	0	9·4	7·25	8·3	8·75
2	30	9·9	7·7	8·55	9·3
6	15	10·6	8·45	9·3	9·65
15	38	—	8·85	9·7	—

It will be noticed that while each successive exposure is 2·5 that of the preceding, the corresponding gain in light is considerably less than one magnitude. From each of the four plates the gain is as follows:—

	Gain in mag.
Plate I.	0·53
,, II.	0·61
,, III.	0·50
,, IV.	0·48

The mean is 0·53—that is to say, instead of one magnitude being gained by continued exposure through each successive interval, the actual gain is only half a magnitude. The exception that might be taken to these experiments is, that the detection of the feeblest stars on a plate is a matter of doubt and great practical difficulty. Dr. Scheiner has, however, availed himself of a second test by counting the stars on a plate after various exposures. With this view two plates were taken of the region round ϵ Orionis, one with an exposure of one hour, the other with eight hours' exposure. Therefore, if 2·5 times the exposure produced stars a magnitude fainter, there ought to be a gain of more than two magnitudes on the second plate, and it may be assumed that the number of stars impressed would follow the known law. On the one-hour plate were found 1174 stars, on the eight-hour 5689. There ought to have been on the long-exposed plate over 10,000 stars, so that roughly speaking only one-half of the stars given by the law were photographed. Further, Argelander has catalogued within this area 125 stars, and therefore it might have been anticipated from the law of increase that some 10,000 stars would have been visible on the one-hour plate.

This margin is too great to be readily explained away. Of course, there is the same difficulty in perceiving the minute dots that represent the faintest stars as in the former case, and further, it is possible that the law of average increase of the number of stars did not hold in this particular part of the sky. It is not to be expected that a law, which applies with more or less accuracy *on the average* to the whole of the sky, is necessarily fulfilled on any small portion, such as the ten-thousandth part. If the stars are not in the heavens, they cannot be photographed. Evidently, it would be unlikely that on every thousandth part of *that* plate would be found the thousandth part of the total number of stars impressed.

But allowing for errors of exaggeration and observation, the result is very interesting, and not a little alarming as implying that photography is not so powerful an engine as was at first anticipated, and that, to accomplish the full hope of all that was expected of it, longer exposure and consequently a greater expenditure of time will be needed. Dr. Scheiner gives a little table, which shows that if a star of the 9·5 mag. be registered in 24 seconds, then in 190 minutes a star of the 16·5 mag. will be photographed, supposing a whole magnitude to be gained by successively multiplying the exposure by 2·5. But if the gain be only 0·5 in this interval, then the faintest star impressed will be only 13·0 mag., even after this long exposure. If 0·6 of a mag. be the rate of increase, then the 13·6 mag. will be seen; if 0·7, then 14·4 mag. The truth will probably be found near this latter limit.

NOTES.

THE second International Folk Lore Congress meets at the rooms of the Society of Antiquaries this afternoon, when an address will be delivered by Mr. Andrew Lang, the President. Three subjects are to be considered—folk tales, mythology, and institutions and customs. To each of these subjects a day will be devoted. The proceedings will be brought to an end on Wednesday morning next.

THE Iron and Steel Institute will meet at the Woolwich Arsenal on Tuesday next. The members are to be conducted over the manufacturing departments at the Arsenal, and will see quick-firing and machine guns in practice. On the following day the Institute will conclude its meeting at the Institution of Civil Engineers.

THE third biennial session of the International Statistical Congress was opened at Vienna, on Monday, by Baron Gautsch, the Austrian Minister of Public Instruction. An address was delivered by Sir Rawson Rawson, the President.

THE seventeenth Annual Congress of the Sanitary Association of Scotland was held in Edinburgh last week. Dr. Farquharson, M.P., President of the Congress, delivered an address "On a Model Hygienic State, or a Glance at the Sanitation of the Future." In the course of his remarks he urged the necessity for more organized attention being given in Parliament to hygienic matters, and advocated the appointment of a Minister of Public Health.

THE Harveian Oration will be delivered at the Royal College of Physicians, by Dr. W. H. Dickinson, at the Royal College of Physicians, on Monday, October 19, at 4 o'clock.

WE referred last week to the death of Prof. W. Ferrel. He was born on January 29, 1817, and since the foundation of the *American Meteorological Journal* he was a frequent contributor to that paper, from which we take most of the following details of his life. During his boyhood he was kept rather closely at work on his father's farm, and with the first money he earned, he bought a copy of Park's "Arithmetic." Having also a liking for astronomical studies, he used to draw a number of diagrams upon the doors of his father's farm, describing circles with the prongs of a pitchfork. In 1839, he entered one of the Colleges in Pennsylvania, and graduated at Bethany College in 1844. In 1857, he became an assistant in the office of the "American Ephemeris and Nautical Almanac," and subsequently entered the U.S. Coast Survey and the Signal Office, from which last he retired in 1886. He was elected a member of the National Academy of Sciences in 1868. Ferrel is described as an extremely diffident man, and he never once sought position; every official position that he occupied having been offered to him. His first paper bearing directly on meteorology was published in 1856, with reference to the deflective effects of the earth's rotation upon the motions of the atmosphere; and this paper, which has done much towards establishing meteorology on a scientific basis, was subsequently revised and reprinted as one of the professional papers of the Signal Service, under the title "Motions of Fluids and Solids on the Earth's Surface." In this treatise he proposed a complete analytical investigation of the general motions of the fluids surrounding the earth. These papers received considerable attention and discussion soon after publication, especially in France; in America and England they were overlooked until recent years, but they are now recognized as fundamental propositions in the study of meteorology. He also wrote various articles on the tides, which are of equal significance with those on the motions of the atmosphere, and he constructed a "maxima and minima tide-predicting machine," which is now in use at the Coast Survey Office in Washington. The last of his numerous works upon meteorology was a "Popular Treatise on the Winds," published in 1889, and reviewed at length in our columns (vol. xli. p. 124). In this work he has explained at length, and with great clearness, many points which in his other writings have been too mathematical to allow of their being generally understood.

We have already recorded with regret that Miss E. A. Ormerod has considered it desirable to resign her post of Consulting Entomologist of the Royal Agricultural Society, which she has occupied for about nine years, having been appointed in 1882. We understand that her reasons for resignation are partly on account of health, as in wet and cold weather she cannot take the requisite journeys to attend Committees without risk; partly on account of claims made of power of Council to direct her to render service in reporting elsewhere, and claims also made as to use of information in her possession beyond what the terms of her engagement granted. These claims, we understand, have been withdrawn, but Miss Ormerod considers she can work more efficiently when freed from the anxieties and possible ties which public office necessarily brings with it. Miss Ormerod's agricultural entomological work, as shown by her annual reports, has now been going on steadily for at least fourteen years, having been begun several years before she was elected to the staff of the Royal Agricultural Society; and this she purposes to continue precisely as before in all respects, whether as regards replies to inquiries, or publication by herself of observations in the form of yearly reports.

In an article on Hooker's "Icones Plantarum," in our last issue (p. 498) we attributed the plates of the earlier volumes to Sir William Hooker. Sir Joseph Hooker informs us that they are all the work of Mr. W. H. Fitch.

A valuable report, by Mr. A. E. Shipley, on an orange disease in Cyprus, caused by a scale insect, is published in the September number of the *Kew Bulletin*. The disease appears to have been noticed in Cyprus for the last six or eight years. The particular insect to which it is due is *Aspidiotus aurantii*, Maskell, a member of the sub-family *Diaspinæ*, which, with some others, compose the family *Coccidæ*. Mr. Shipley gives an account of the life-history of this insect, and then describes the various methods of dealing with it. The most successful of these methods is the gas treatment, a full description of which, by Mr. Coquillett, is quoted by Mr. Shipley from Bulletin No. 23 of the U.S. Department of Agriculture, Division of Entomology. We may note that Mr. Shipley is anxious to obtain examples of *Coccidæ* which infest plants, and examples of nematode worms parasitic in plants, with the affected parts of their respective hosts.

The *Kew Bulletin* for September, besides Mr. Shipley's report on orange scale in Cyprus, contains sections on the rediscovery of gutta-percha trees at Singapore, on a new process for recovering some portion of the gutta-percha which is left in the bark of the trees after collection by the ordinary native method, on the fodder plant Tagasaste, and on Kangra buckwheat.

The *Oesterreichische Botanische Zeitung* for September contains a report of Dr. A. v. Degen's botanical excursion to the island of Samothrace, and of Dr. R. F. Solla's to Southern Istria.

The fourth number of the first volume of *Contributions from the U.S. National Herbarium*, published under the auspices of the Department of Agriculture at Washington, consists of a description, by Mr. J. N. Rose, of the plants collected by Dr. E. Palmer in 1890 in Western Mexico and Arizona. Forty-five new species are described, and several of these are illustrated by plates. Most of the new species obtained were from the neighbourhood of Alamos, a mining town of about 10,000 inhabitants, situated 180 miles south-east from Guaymas, at an altitude of about 1275 feet, where there are both a dry spring and a rainy autumn flora, very different from one another. Dr. Palmer has again started for a year's exploration of Western Mexico.

Some valuable and interesting notes on the fertilization of South African and Madagascar flowering plants, by Mr. G. F. Scott Elliot, appear in *Annals of Botany* (vol. v., No. xix., August 1891), and have also been issued separately. They represent much work done during a two years' botanical trip. While travelling, Mr. Elliot found it impossible to make as thorough and complete observations as are really required for a proper comprehension of all the adaptations of a flower to insect visitors; but he tried to collect every insect which he saw visiting flowers, and brought home with him a numbered collection. Most of the forms secured by him had not previously been studied in their native haunts.

The Transactions of the Liverpool Biological Society for 1891 contain an important paper by Mr. G. Murray on the Distribution of Marine Algæ in space and in time. The author compares the algal flora of three widely separated regions—the Arctic Sea, the West Indian region, and Australia; and shows in a table how many genera and species are common to any two of the regions. The number of known species of seaweeds is given as 259 in the Arctic Sea, 788 in the West Indies, and 1132 in Australia. Only twelve species are common to all three regions, and of these four belong to the Ulveæ.

A great Mining Exhibition is to be opened at Johannesburg next July, and exhibits from all parts of the world are invited.

The administration report of the Marine Survey of India for the official year 1890–91, by Captain R. F. Hoskyn, has been published. For some time notices had been received from several vessels, which seemed to indicate that the shoals lying off the eastern coast between Ennore and Pulicat were extending seaward. In the early part of 1890, therefore, the *Investigator* proceeded to this neighbourhood, and made a survey of the coast between these two places, carrying the soundings out to the 100-fathom line. The result showed that no material change had taken place in the size or position of the shoals from the time of the previous survey. The work of the season ended on May 7, when the *Investigator* arrived at Bombay. In October last a new season's operations began, and at the time when the report was written (March 9, 1891) the survey of the eastern coast of Hindustan had been completed to lat. 16° 50′ N.

The report of Dr. A. Alcock, surgeon-naturalist on board the *Investigator*, is one of great interest. It is given as an appendix to that of Captain Hoskyn. We have already referred to Dr. Alcock's account of the general results of his deep-sea work. It may be noted that on November 3, 1890, the deepest haul ever made in Indian seas—1,997 fathoms—was successfully carried out in lat. 9° 34′ N., long. 85° 43′ 15″ E., the bottom being Globigerina ooze with pieces of water-worn pumice, and the bottom-temperature being 35° F. About 2200 fathoms of wire were veered. The following was the entire take:—There were three species of siliceous sponges and numerous detached spicules of *Hyalonema*; a large sea-anemone of a salmon-pink colour, with bright red tentacles; a mutilated specimen of the Brisingoid *Freyella benthophila*, Sladen, a fine new species of *Hyphalaster*, and a small, probably new, species of *Marsipaster* with the nidamental pouches widely open and full of ova; two species of Ophiurids, one of which is *Ophiomastus*; three species of Holothurians including *Echinosoma*; numerous specimens of a long-stalked Ascidian; two specimens of a very large species of *Amphipod*, a blind Crangonid, three species of macrurous Crustaceans, and a small *Scapellum*; a small Lamellibranch; and a number of empty annelid tubes, some of which were constructed of Foraminifera shells, while others consisted of agglutinated silky (siliceous) threads.

Mr. W. L. Dallas, assistant meteorological reporter to the Government of India, has written a valuable paper on the meteorology and climatology of Northern Afghanistan, the facts having been collected by officers connected with the Afghan Delimitation Commission. Taking the whole of the record into consideration, Mr. Dallas thinks it may safely be maintained that in the great majority of cases the disturbed weather which appears over North-Western India during the winter and spring months is the result of disturbances, which either effect simultaneously the whole region comprising Afghanistan, Baluchistan, and North-Western India, or which have appeared first over Afghanistan and secondly over India, and that these disturbances have seldom originated in India itself or are confined to India.

We have received from the Meteorological Council their Quarterly Weather Report for July to December 1880, and Monthly Weather Report for May to December 1887. The Quarterly Reports, which commenced with the year 1869, contain, in addition to the monthly and five-daily means of the observations made at the seven observatories, plates of the continuous curves of the self-recording instruments, which have been etched at the Office, and are perhaps the most complete and perfect series of meteorological curves hitherto published, and also a condensed account of the most important meteorological changes of the period. The Quarterly Reports are now discontinued, and the publication of a Monthly Weather Report was undertaken in 1884 in substitution for the Quarterly Report, while the hourly observations and means have been published in a separate volume. This Report contains the results of observations made at a considerable number of stations, together with a chronicle of the weather, and charts showing the average conditions of the various elements. Both the Quarterly and Monthly Weather Reports also contain a number of elaborate discussions of various allied subjects. The Monthly Reports in the form hitherto issued have been modified; and instead of appearing as a separate work, a Monthly Summary of the Weather, on a more concise plan, has been added to the Weekly Weather Report, commencing with the year 1888. With the exception of the years 1881–83 we have therefore a continuous and valuable record of the weather—in addition to such as is afforded by the Daily and Weekly Reports—since 1869, and we believe it is the intention of the Council to connect the gap between the Quarterly and Monthly Reports at an early date, by a discussion of the weather for that period. We shall refer in a future number to the publications which deal with the observations and results at the Stations of the Second Order, which are more particularly of a climatological character, without discussions of current weather.

The Park Commissioners of Boston, U.S., have set apart three parcels of land for the establishment, by the Boston Society of Natural History, of zoological gardens and aquaria. It is essential that 200,000 dollars should be raised before any attempt can be made to realize the scheme as a whole, but if a third of the amount were subscribed, one of the two proposed aquaria might at once be instituted. An appeal has been made by the Society to the people of Boston for the necessary funds, and it will be strange if it does not meet with a ready and liberal response. The Society is sanguine enough to think that every public-spirited citizen will see in the scheme "an addition to the forces which increase the intelligence of the voter, and thereby tend to make Boston a more desirable place of residence."

Students of the Ice Age will read with interest a paper by Mr. N. S. Shaler on the antiquity of the last glacial period, submitted to the Boston Society of Natural History, and printed in the latest instalment of the Society's Proceedings. Mr. Shaler differs decidedly from those geologists who suppose that the end of the glacial period is probably not very remote from our own day. One of the strongest of his arguments is derived from the distribution of the vegetation which in America has regained possession, by migration, of the glaciated district. We must conceive, he points out, that as the ice retreated and gradually disappeared from the surface a considerable time elapsed before existing forests attained their organization. He assumes as certain that the black walnut and the pignut hickory, between Western Minnesota and the Atlantic coast, have advanced, on the average, a distance of 400 miles north of the ancient ice front to which their ancestors were driven by the presence of the glacial sheet. For several reasons he believes that the northward progress of these forms must have been due mainly, not to the action of streams or tornadoes, but to the natural spread of the seed from the extremities of boughs, and to the carriage of the seed by rodents. But allowing for every conceivable method of transportation, he argues that a period of ten or even twenty thousand years is wholly inadequate to account for the present distribution of these large-seeded trees. If they occurred only sporadically in the northernmost part of the field they occupy, their implantation might be regarded as due to chance action. The fact, however, that they extend from the Atlantic to Minnesota indicates that the advance was accomplished by causes of a general and continuous nature.

"Water-birds that live in the Woods" formed the subject of an interesting paper read lately by Mr. G. B. Sennett before the Linnæan Society of New York. About a dozen species were dealt with, the most interesting of them perhaps being the tree ducks (*Dendrocygna autumnalis et fulva*). The former is found in the heaviest timber along the Rio Grande of Texas, at Lomita, and as this river furnishes no sort of food, it adapts itself to circumstances and feeds upon seeds or grain. These ducks will alight upon a stalk of growing corn with the ease of a blackbird, and are quite at home among the lofty trees where they make their nests. They do not resort to the river, which is so cold and muddy, from the melting snows of the mountains whence it flows, that all vegetable and animal life save the gar-pike is wanting. No ducks of any kind are found upon it. A flock of cormorants, about four miles long and one mile and a half wide, was once seen by Mr. Sennett in Minnesota.

Sparrows do not seem to lose in New Zealand any of the audacity for which they are famous in Europe. In a paper read some time ago before the New Zealand Institute, and now printed in the Transactions, Mr. T. W. Kirk gives an example of what he calls their "daring and cool impudence." Between Featherston and Martinborough he heard one day a most unusual noise, as though all the small birds in the country had joined in one grand quarrel. Looking up, he saw a large hawk (*C. gouldi*—a carrion-feeder) being buffeted by a flock of sparrows. They kept dashing at him in scores, and from all points at once. The unfortunate hawk was quite powerless; indeed, he seemed to have no heart left, for he did not attempt to retaliate, and his defence was of the feeblest. At last, approaching some scrub, he made a rush indicative of a forlorn hope, gained the shelter, and there remained. Mr. Kirk watched for fully half an hour, but he did not reappear. The sparrows congregated in groups about the bushes, keeping up a constant chattering and noise, evidently on the look-out for the enemy, and congratulating themselves upon having secured a victory.

If we may judge from the Report of the Department of Agriculture, Victoria, for the year 1889–90, the farmers of that colony are likely to benefit largely by the work of the agricultural authorities. The Department is efficiently organized, and has a thoroughly scientific conception of the nature of its duties. Mr. D. McAlpine, who has been appointed consulting vegetable pathologist, presents the following summary of the tasks undertaken by his particular section: (1) special investigations concerning the rust of wheat, oats, barley, and other cereals, and, connected with that, the question of rust on various grasses—native and imported; (2) investigations of the life-histories of the various fungus pests, and a knowledge of the best time to cope with them; (3) reports upon diseased specimens sent in from different parts of the colony, and the best known remedies for the palliation or prevention of such diseases; (4) collection of specimens of the various diseases due to fungi, and the subsequent formation of a museum for educational purposes; (5) delivery of lectures in different centres on the fungus pests most prevalent there; (6) preparation of illustrated handbooks, describing the nature of the various diseases and the remedies to be employed where possible; (7) testing various fungicides and the best methods of applying them; (8) visiting different districts in order to find out prevailing and injurious fungi; (9) contributing periodic reports to the official Bulletin of the Department.

IN the Proceedings of the Bath Natural History and Antiquarian Field Club (vol. vii. No. 2), Mr. J. F. Mostyn Clarke gives an account of the geological formations exposed in the cuttings of the Bridgwater Railway, the construction of which opened up a continuous line of excavation through the heart of the Polden Hills. Mr. Clarke had charge of the construction of the railway until near the completion of the earthwork, so that he had excellent opportunities for making careful observations. Geologists may be glad to have his description of the strata when the slopes of the cuttings are overgrown.

MESSRS. LONGMANS, GREEN, AND CO. have published the sixth edition of "An Elementary Treatise on the Integral Calculus," by Dr. Benjamin Williamson, F.R.S. In this edition the work has been revised and enlarged.

MESSRS. MITSCHER AND RÖSTELL, 61*a* Jägerstrasse, Berlin, have issued an important list of books which they have for sale. The works relate to the various departments of botany.

TWO communications upon the volatile carbonyl compounds of platinum, from Dr. Pullinger, of Tübingen, and Drs. Mylius and Foerster, of Charlottenburg, appear in the last number of the *Berichte*. Since the preparation of the remarkable carbonyl compounds of nickel and iron by Messrs. Mond, Langer, and Quincke, these platinum compounds, discovered by Schutzenberger in the year 1868, have become more interesting, and the two papers now before us add considerably to our knowledge of them. They are compounds containing platinum, chlorine, and carbon monoxide, and Schutzenberger assigned to them the formulæ $PtCl_2CO$, $PtCl_2 . 2CO$, and $2PtCl_2 . 3CO$ respectively. He obtained them by heating spongy platinum to a temperature of 250° C. in a stream first of chlorine and afterwards of carbon monoxide. The volatile, readily fusible, and crystalline sublimate obtained contained a mixture of the three, and he effected a separation by extraction with carbon tetrachloride, in which the three compounds are differently soluble. They are well defined by their melting-points, which are 194°, 142°, and 130° C. respectively. They are decomposed by water with separation of platinum, formation of hydrochloric acid, and evolution of carbon dioxide, and also, in case of the second and third compounds, of carbon monoxide. The most stable of these compounds and the best investigated is the simpler one, $COPtCl_2$. It appears to possess a distinctly basic character, so that it is able to combine with hydrochloric acid to form a compound, $COPtCl_2 . HCl$; this compound is formed in solution when the crystals are dissolved in concentrated hydrochloric acid. The two other compounds are decomposed by hydrochloric acid, losing carbon monoxide and forming the hydrochloride of the first compound. On evaporation of the hydrochloric solution, the first compound is left in needle-shaped crystals. When phosgene gas, $COCl_2$, is passed over the crystals, drops of liquid are formed, which consist of a solution of the compound in liquefied carbonyl chloride. In addition to these compounds, the bromide and iodide corresponding to the compound $COPtCl_2$ have been prepared. When the hydrochloric acid solution of the latter is evaporated on a water-bath in a stream of hydrobromic acid gas, and the resulting compound extracted with benzene, the filtered solution deposits, on cooling, orange-red needles of the bromide, $COPtBr_2$. The bromide has likewise been obtained by Dr. Pullinger, by passing carbon monoxide over heated platinous bromide. Similarly, the iodide has been prepared by evaporating crystals of the chloride with excess of hydriodic acid solution, and treating the residue with warm benzene. The crystals of the iodide, $COPtI_2$, which separate from the benzene solution on cooling, are deep red in colour, with a violet surface reflection. The chloride, bromide, and iodide exhibit a beautifully graduated difference of properties. Thus the chloride is yellow, the bromide orange, and the iodide red in colour. The melting-points are 194°, 181°, and 140° respectively. The chloride is readily, the bromide difficultly, and the iodide not at all volatile. The chloride is strongly hygroscopic, the bromide less so, and the iodide permanent. In addition to these compounds, another has been obtained by Dr. Pullinger, of the composition $PtCl_2 . 2COCl_2$, in the form of non-volatile yellow crystals, readily soluble in water, from which it recrystallizes unchanged. It appears to be the most stable of all these platinum compounds, but is only obtained in very small quantity.

OUR ASTRONOMICAL COLUMN.

INFLUENCE OF ABERRATION UPON OBSERVATIONS OF SOLAR PROMINENCES.—Some recent observations of the development and movement of solar prominences have led M. Fizeau to consider the influence that the aberration of light may exercise upon them. A note relative to such an inquiry is contained in *Comptes rendus* for September 7. It is well known that, in consequence of aberration, the longitude of the sun, and therefore of the prominences, is diminished by the amount of the constant, $20''\cdot445$—an apparent displacement depending upon the earth's orbital velocity. And it results from this that if a prominence is developed in the neighbourhood of the ecliptic, and the luminous matter of which it is composed has a velocity of translation equal to the velocity of the earth in its orbit, its position will suffer a displacement of $20''\cdot445$, which may be added to the effect due to the earth's motion, or otherwise, according to the direction of propagation, and thus give rise to corresponding variations in distances from the edge of the sun. As a matter of fact, however, the velocities of prominences are not uniform, and do not commonly attain the required value; nevertheless it seems that the high velocities which have been determined must give rise to apparent movements which depend upon the laws of aberration, and which ought to be taken into account in precise measurements.

Another point touched upon in the communication to which reference has been made is the physical nature of prominences. The simplest hypothesis is that they represent clouds of incandescent hydrogen and other metallic vapours; but M. Fizeau favours the idea that their visibility is the result of the passage of electrical discharges through gaseous material.

NEW ASTEROIDS.—The 317th asteroid was discovered by Charlois on September 8, and the 318th on September 11.

SOME OF THE POSSIBILITIES OF ECONOMIC BOTANY.[1]

OUR Association demands of its President, on his retirement from office, some account of matters connected with the department of science in which he is engaged.

The subject which I have selected for the valedictory address deals with certain industrial, commercial, and economic questions: nevertheless it lies wholly within the domain of botany. I invite you to examine with me some of the possibilities of economic botany.

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[1] Abstract of the Presidential address delivered before the American Association for the Advancement of Science, at Washington, August 1891, by George Lincoln Goodale, M.D., LL.D., Fisher Professor of Natural History, Harvard University, Cambridge, Mass., U.S.A.

Of course, when treating a topic which is so largely speculative as this, it is difficult and unwise to draw a hard and fast line between possibilities and probabilities. Nowadays possibilities are so often realized rapidly that they become accomplished facts before we are aware.

In asking what are the possibilities that other plants than those we now use may be utilized we enter upon a many-sided inquiry. Speculation is rife as to the coming man. May we not ask what plants the coming man will use?

There is an enormous disproportion between the total number of species of plants known to botanical science and the number of those which are employed by man.

The species of flowering plants already described and named are about one hundred and seven thousand. Acquisitions from unexplored or imperfectly explored regions may increase the aggregate perhaps one-tenth, so that we are within very safe limits in taking the number of existing species to be somewhat above one hundred and ten thousand.

Now if we should make a comprehensive list of all the flowering plants which are cultivated on what we may call a fairly large scale at the present day, placing therein all food and forage plants, all those which are grown for timber and cabinet woods, for fibres and cordage, for tanning materials, dyes, resins, rubber, gums, oils, perfumes, and medicines, we could bring together barely three hundred species. If we should add to this short catalogue all the species, which, without cultivation, can be used by man, we should find it considerably lengthened. A great many products of the classes just referred to are derived in commerce from wild plants, but exactly how much their addition would extend the list, it is impossible in the present state of knowledge to determine. Every enumeration of this character is likely to contain errors from two sources: first, it would be sure to contain some species which have outlived their real usefulness; and, secondly, owing to the chaotic condition of the literature of the subject, omissions would occur.

But after all proper exclusions and additions have been made, the total number of species of flowering plants utilized to any considerable extent by man in his civilized state does not exceed, in fact it does not quite reach, one per cent.

The disproportion between the plants which are known and those which are used becomes much greater when we take into account the species of flowerless plants also. Of the five hundred ferns and their allies we employ for other than decorative purposes only five; the mosses and liverworts, roughly estimated at five hundred species, have only four which are directly used by man. There are comparatively few Algæ, Fungi, or Lichens which have extended use.

Therefore, when we take the flowering and flowerless together, the percentage of utilized plants falls far below the estimate made for the flowering alone.

Such a ratio between the number of species known and the number used justifies the inquiry which I have proposed for discussion at this time—namely, can the short list of useful plants be increased to advantage? If so, how?

This is a practical question; it is likewise a very old one. In one form or another, by one people or another, it has been asked from early times. In the dawn of civilization, mankind inherited from savage ancestors certain plants, which had been found amenable to simple cultivation, and the products of these plants supplemented the spoils of the chase and of the sea. The question which we ask now was asked then. Wild plants were examined for new uses; primitive agriculture and horticulture extended their bounds in answer to this inquiry. Age after age has added slowly and cautiously to the list of cultivable and utilizable plants, but the aggregate additions have been, as we have seen, comparatively slight.

The question has thus no charm of novelty, but it is as practical to-day as in early ages. In fact, at the present time, in view of all the appliances at the command of modern science and under the strong light cast by recent biological and technological research, the inquiry which we propose assumes great importance. One phase of it is being attentively and systematically regarded in the great experiment stations, another phase is being studied in the laboratories of chemistry and pharmacy, while still another presents itself in the museums of economic botany.

Our question may be put in other words, which are even more practical. What present likelihood is there that our tables may, one of these days, have other vegetables, fruits, and cereals, than those which we use now? What chance is there that new fibres may supplement or even replace those which we spin and weave, that woven fabrics may take on new vegetable colours, that flowers and leaves may yield new perfumes and flavours? What probability is there that new remedial agents may be found among plants neglected or now wholly unknown? The answer which I shall attempt is not in the nature of a prophecy; it can claim no rank higher than that of a reasonable conjecture.

At the outset it must be said that synthetic chemistry has made and is making some exceedingly short cuts across this field of research, giving us artificial dyes, odours, flavours, and medicinal substances, of such excellence that it sometimes seems as if before long the old-fashioned chemical processes in the plant itself would play only a subordinate part. But although there is no telling where the triumphs of chemical synthesis will end, it is not probable that it will ever interfere essentially with certain classes of economic plants. It is impossible to conceive of a synthetic fibre or a synthetic fruit. Chemistry gives us fruit-ethers and fruit-acids, and after a while may provide us with a true artificial sugar and amorphous starch; but artificial fruits worth the eating or artificial fibres worth the spinning are not coming in our day.

Despite the extraordinary achievements of synthetic chemistry, the world must be content to accept, for a long time to come, the results of the intelligent labour of the cultivator of the soil and the explorer of the forest. Improvement of the good plants we now utilize, and the discovery of new ones, must remain the care of large numbers of diligent students and assiduous workmen. So that, in fact, our question resolves itself into this: Can these practical investigators hope to make any substantial advance?

It seems clear that, except in modern times, useful plants have been selected almost wholly by chance, and it may well be said that a selection by accident is no selection at all. Nowadays, the new selections are based on analogy. One of the most striking illustrations of the modern method is afforded by the utilization of bamboo fibre for electric lamps.

Some of the classes of useful plants must be passed by without present discussion; others alluded to slightly, while still other groups fairly representative of selection and improvement will be more fully described. In this latter class would naturally come, of course, the food-plants known as

I. THE CEREALS.

Let us look first at these.

The species of grasses which yield these seed-like fruits, or as we might call them for our purpose seeds, are numerous; twenty of them are cultivated largely in the Old World, but only six of them are likely to be very familiar to you—namely, wheat, rice, barley, oats, rye, and maize. The last of these is of American origin, despite doubts which have been cast upon it. It was not known in the Old World until after the discovery of the New. It has probably been very long in cultivation. The others all belong to the Old World. Wheat and barley have been cultivated from the earliest times; according to De Candolle, the chief authority in these matters, about four thousand years. Later came rye and oats, both of which have been known in cultivation for at least two thousand years. Even the shorter of these periods gives time enough for wide variation, and as is to be expected there are numerous varieties of them all. For instance, Vilmorin, in 1880, figured sixty-six varieties of wheat with plainly distinguishable characters.

If the Chinese records are to be trusted, rice has been cultivated for a period much longer than that assigned by our history and traditions to the other cereals, and the varieties are correspondingly numerous. It is said that in Japan above three hundred varieties are grown on irrigated lands, and more than one hundred on uplands.

With the possible exception of rice, not one of the species of cereals is certainly known in the wild state.

It is out of our power to predict how much time would elapse before satisfactory substitutes for our cereals could be found. In the improvement of the grains of grasses other than those which have been very long under cultivation, experiments have been few, scattered, and indecisive. Therefore we are as badly off for time-ratios as are the geologists and archæologists in their statements of elapsed periods. It is impossible for us to ignore the fact that there appear to be occasions in the life of a species when it seems to be peculiarly susceptible to the influences of surroundings. A species, like a carefully laden ship, represents a balancing of forces within and without. Disturbance may come through variation from within, as from a shifting of the cargo, or in some cases from without. We may suppose both forces to be active in producing variation, a change in the internal condition rendering the plant more susceptible to any change in its surroundings. Under the influence of any marked disturbance, a state of unstable equilibrium may be brought about, at which times the species as such is easily acted upon by very slight agencies.

One of the most marked of these derangements is a consequent of cross-breeding within the extreme limits of varieties. The resultant forms in such cases can persist only by close breeding or by propagation from buds or the equivalents of buds. Disturbances like these arise unexpectedly in the ordinary course of nature, giving us sports of various kinds. These critical periods, however, are not unwelcome, since skilful cultivators can take advantage of them. In this very field much has been accomplished. An attentive study of the sagacious work done by Thomas Andrew Knight shows to what extent this can be done. But we must confess that it would be absolutely impossible to predict with certainty how long or how short would be the time before new cereals or acceptable equivalents for them would be provided. Upheld by the confidence which I have in the intelligence, ingenuity, and energy of our experiment stations, I may say that the time would not probably exceed that of two generations of our race, or half a century.

In now laying aside our hypothetical illustration, I venture to ask why it is that our experiment stations, and other institutions dealing with plants and their improvement, do not undertake investigations like those which I have sketched? Why are not some of the grasses other than our present cereals studied with reference to their adoption as food grains? One of these species will naturally suggest itself to you all—namely, the wild rice of the lakes. Observations have shown that, were it not for the difficulty of harvesting these grains, which fall too easily when they are ripe, they might be utilized. But attentive search might find or educe some variety of *Zizania*, with a more persistent grain and a better yield. There are two of our seashore grasses which have excellent grains, but are of small yield. Why are not these, or better ones which might be suggested by observation, taken in hand?

The reason is plain. We are all content to move along in lines of least resistance, and are disinclined to make a fresh start. It is merely leaving well enough alone, and so far as the cereals are concerned it is indeed well enough. The generous grains of modern varieties of wheat and barley compared with the well-preserved charred vestiges found in Greece by Schliemann, and in the lake-dwellings, are satisfactory in every respect. Improvements, however, are making in many directions; and in the cereals we now have, we possess far better and more satisfactory material for further improvement both in quality and as regards range of distribution than we could reasonably hope to have from other grasses.

From the cereals we may turn to the interesting groups of plants comprised under the general term

II. VEGETABLES.

Under this term it will be convenient for us to include all plants which are employed for culinary purposes, or for table use, such as salads and relishes.

The potato and sweet potato, the pumpkin and squash, the red or capsicum peppers, and the tomato, are of American origin.

All the others are, most probably, natives of the Old World. Only one plant coming in this class has been derived from Southern Australasia—namely, New Zealand spinach (*Tetragonia*).

Among the vegetables and salad-plants longest in cultivation we may enumerate the following: turnip, onion, cabbage, purslane, the large bean (Faba), chick-pea, lentil, and one species of pea (garden-pea). To these an antiquity of at least 4000 years is ascribed.

Next to these, in point of age, come the radish, carrot, beet, garlic, garden-cress, and celery, lettuce, asparagus, and the leek. Three or four leguminous seeds are to be placed in the same category, as are also the black peppers.

Of more recent introduction the most prominent are: the parsnip, oyster-plant, parsley, artichoke, endive, and spinach.

From these lists I have purposely omitted a few which belong exclusively to the tropics, such as certain yams.

The number of varieties of these vegetables is astounding. It is, of course, impossible to discriminate between closely allied varieties which have been introduced by gardeners and seedsmen under different names, but which are essentially identical, and we must therefore have recourse to a conservative authority, Vilmorin, from whose work a few examples have been selected. The varieties which he accepts are sufficiently well distinguished to admit of description, and in most instances of delineation, without any danger of confusion. The potato has, he says, innumerable varieties, of which he accepts forty as easily distinguishable and worthy of a place in a general list; but he adds also a list, comprising, of course, synonyms, of thirty-two French, twenty-six English, nineteen American, and eighteen German varieties. The following numbers speak for themselves, all being selected in the same careful manner as those of the potato: celery more than twenty; carrot more than thirty; beet, radish, and potato, more than forty; lettuce and onion more than fifty; turnip more than seventy; cabbage, kidney-bean, and garden pea, more than one hundred.

The amount of horticultural work which these numbers represent is enormous. Each variety established as a race (that is, a variety which comes true to seed) has been evolved by the same sort of patient care and waiting which we have seen is necessary in the case of cereals, but the time of waiting has not been as a general thing so long.

In the case of the cabbage there are important morphological changes like those to which Prof. Bailey has called attention in the case of the tomato. Suppose we are strolling along the beach at some of the seaside resorts of France, and should fall in with this coarse cruciferous plant, with its sprawling leaves and strong odour. Would there be anything in its appearance to lead us to search for its hidden merit as a food-plant? What could we see in it which would give it a preference over a score of other plants at our feet? Again, suppose we are journeying in the high lands of Peru, and should meet with a strong-smelling plant of the nightshade family, bearing a small irregular fruit, of sub-acid taste and of peculiar flavour. We will further imagine that the peculiar taste strikes our fancy, and we conceive that the plant has possibilities as a source of food. We should be led by our knowledge of the potato, probably a native of the same region, to think that this allied plant might be safely transferred to a northern climate, but would there be promise of enough future usefulness, in such a case as this, to warrant our carrying the plant north as an article of food? Suppose, further, we should ascertain that the fruit in question was relished not only by the natives of its home, but that it had found favour among the tribes of South Mexico and Central America, and had been cultivated by them until it had attained a large size; should we be strengthened in our venture? Let us go one step further still. Suppose that, having decided upon the introduction of the plant, and having urged everybody to try it, we should find it discarded as a fruit, but taking a place in gardens as a curiosity under an absurd name, or as a basis for preserves and pickles; should we not look upon our experiment in the introduction of this new plant as a failure? This is not a hypothetical case.

The tomato, the plant in question, was cultivated in Europe as long ago as 1554; it was known in Virginia in 1781 and in the Northern States in 1785; but it found its way into favour slowly, even in this land of its origin. A credible witness states that in Salem it was almost impossible to induce people to eat or even taste of the fruit. And yet, as you are well aware, its present cultivation on an enormous scale in Europe and this country is scarcely sufficient to meet the increasing demand.

Before asking specifically in what direction we shall look for new vegetables, I must be pardoned for calling attention, in passing, to a very few of the many which are already in limited use in Europe and this country, but which merit a wider employment. Cardon, or cardoon; celeriac, or turnip-rooted celery; fetticus, or corn-salad; martynia; salsify; sea-kale; and numerous small salads, are examples of neglected treasures of the vegetable garden.

The following, which are even less known, may be mentioned as fairly promising;—

(1) *Arracacia esculenta*, called Arracacha, belonging to the parsley family. It is extensively cultivated in some of the northern States of South America. The stems are swollen near

the base, and produce tuberous enlargements filled with an excellent starch. Although the plant is of comparatively easy cultivation, efforts to introduce it into Europe have not been successful, but it is said to have found favour in both the Indies, and may prove useful in our Southern States.

(2) *Ullucus* or Ollucus, another tuberous-rooted plant from nearly the same region, but belonging to the beet or spinach family. It has produced tubers of good size in England, but they are too waxy in consistence to dispute the place of the better tubers of the potato. The plant is worth investigating for our hot dry lands.

(3) A tuber-bearing relative of our common hedge-nettle, or *Stachys*, is now cultivated on a large scale at Crosnes, in France, for the Paris market. Its name in Paris is taken from the locality where it is now grown for use. Although its native country is Japan, it is called by some seedsmen Chinese artichoke. At the present stage of cultivation, the tubers are small and are rather hard to keep, but it is thought "that both of these defects can be overcome or evaded." Experiments indicate that we have in this species a valuable addition to our vegetables.

We must next look at certain other neglected possibilities.

Dr. Edward Palmer, whose energy as a collector and acuteness as an observer are known to you all, has brought together very interesting facts relative to the food-plants of our North American aborigines. Among the plants described by him there are a few which merit careful investigation. Against all of them, however, there lie the objections mentioned before, namely:—

(1) The long time required for their improvement, and

(2) The difficulty of making them acceptable to the community, involving

(3) The risk of total and mortifying failure.

In 1854 the late Prof. Gray called attention to the remarkable relations which exist between the plants of Japan and those of our eastern coast. You will remember that he not only proved that the plants of the two regions had a common origin, but also emphasized the fact that many species of the two countries are almost identical. It is to that country which has yielded us so many useful and beautiful plants that we turn for new vegetables to supplement our present food resources. One of these plants—namely, *Stachys*—has already been mentioned as promising. There are others which are worth examination and perhaps acquisition.

One of the most convenient places for a preliminary examination of the vegetables of Japan is at the railroad stations on the longer lines—for instance, that running from Tokio to Kobe. For native consumption there are prepared luncheon boxes of two or three stories, provided with the simple and yet embarrassing chopsticks. It is worth the shock it causes one's nerves to invest in these boxes and try the vegetable contents. The bits of fish, flesh, and fowl which one finds therein can be easily separated and discarded, upon which there will remain a few delicacies. The pervading odour of the box is that of aromatic vinegar. The generous portion of boiled rice is of excellent quality, with every grain well softened and distinct, and this without anything else would suffice for a tolerable meal. In the boxes which have fallen under my observation there were sundry boiled roots, shoots, and seeds which were not recognizable by me in their cooked form. Prof. Georgeson, formerly of Japan, has kindly identified some of these for me, but he says, "There are doubtless many others used occasionally."

One may find sliced lotus roots, roots of large burdock, lily bulbs, shoots of ginger, pickled green plums, beans of many sorts, boiled chestnuts, nuts of the gingko tree, pickled greens of various kinds, dried cucumbers, and several kinds of seaweeds. Some of the leaves and roots are cooked in much the same manner as beet-roots and beet-leaves are by us, and the general effect is not unappetizing. The boiled shoots are suggestive of only the tougher ends of asparagus. On the whole, I do not look back on Japanese railway luncheons with any longing which would compel me to advocate the indiscriminate introduction of the constituent vegetables here.

But when the same vegetables are served in native inns, under more favourable culinary conditions, without the flavour of vinegar and of the pine wood of the luncheon boxes, they appear to be worthy of a trial in our horticulture, and I therefore deal with one or two in greater detail.

Prof. Georgeson, whose advantages for acquiring a knowledge of the useful plants of Japan have been unusually good, has placed me under great obligations by communicating certain facts regarding some of the more promising plants of Japan which are not now used here. It should be said that several of these plants have already attracted the notice of the Agricultural Department in this country.

The soy bean (*Glycine hispida*). This species is known here to some extent, but we do not have the early and best varieties. These beans replace meat in the diet of the common people.

Mucuna (*Mucuna capitata*) and dolichos (*Dolichos cultratus*) are pole beans possessing merit.

Dioscorea. There are several varieties with palatable roots. Years ago one of these was spoken of by the late Dr. Gray as possessing "excellent roots, if one could only dig them."

Colocasia antiquorum has tuberous roots, which are nutritious.

Conophallus Konjak has a large bulbous root, which is sliced, dried, and beaten to a powder. It is an ingredient in cakes.

Aralia cordata is cultivated for the shoots, and used as we use asparagus.

Œnanthe stolonifera and *Cryptotænia canadensis* are palatable salad plants, the former being used also as greens.

III. FRUITS.

Botanically speaking, the cereal grains of which we have spoken are true fruits—that is to say, are ripened ovaries, but for all practical purposes they may be regarded as seeds. The fruits of which mention is now to be made are those commonly spoken of in our markets as fruits.

First of all, attention must be called to the extraordinary changes in the commercial relations of fruits by two direct causes—

(1) The canning industry, and

(2) Swift transportation by steamers and railroads.

The effects of these two agencies are too well known to require more than this passing mention. By them the fruits of the best fruit-growing countries are carried to distant lands in quantities which surprise all who see the statistics for the first time. The ratio of increase is very startling. Take, for instance, the figures given by Mr. D. Morris, at the time of the great Colonial and Indian Exhibition in London. Compare double decades of years—

	£
1845	886,888
1865	3,185,984
1885	7,587,523

In the Colonial Exhibition at London, in 1886, fruits from the remote colonies were exhibited under conditions which proved that, before long, it may be possible to place such delicacies as the cherimoyer, the sweet-cup, sweet-sop, rambutan, mango, and mangosteen, at even our most northern seaports. Furthermore, it seems to me likely that, with an increase in our knowledge with regard to the microbes which produce decay, we may be able to protect the delicate fruits from injury for any reasonable period. Methods which will supplement refrigeration are sure to come in the very near future, so that even in a country so vast as our own, the most perishable fruits will be transported through its length and breadth without harm.

The canning industry and swift transportation are likely to diminish zeal in searching for new fruits, since, as we have seen in the case of the cereals, we are prone to move in lines of least resistance, and leave well enough alone.

To what extent are our present fruits likely to be improved? Even those who have watched the improvement in the quality of some of our fruits, like oranges, can hardly realize how great has been the improvement within historic times in the character of certain pears, apples, and so on.

The term historic is used advisedly, for there are pre-historic fruits which might serve as a point of departure in the consideration of the question. In the ruins of the lake-dwellings in Switzerland, charred apples have been found, which are in some cases plainly of small size, hardly equalling ordinary crab apples. But, as Dr. Sturtevant has shown, in certain directions there has been no marked change of type—the change is in quality.

In comparing the earlier descriptions of fruits with modern accounts, it is well to remember that the high standards by which fruits are now judged are of recent establishment. Fruits which would once have been esteemed excellent would to-day be passed by as unworthy of regard.

It seems probable that the list of seedless fruits will be materially lengthened, provided our experimental horticulturists make use of the material at their command. The common fruits which have very few or no seeds are the banana, pineapple, and certain oranges. Others mentioned by Mr. Darwin as well known are the bread-fruit, pomegranate, arazole or Neapolitan medlar, and date-palms. In commenting upon these fruits, Mr. Darwin says that most horticulturists "look at the great size and anomalous development of the fruit as the cause, and sterility as the result," but he holds the opposite view as more probable—that is, that the sterility, coming about gradually, leaves free for other growth the abundant supply of building material which the forming seed would otherwise have. He admits, however, that "there is an antagonism between the two forms of reproduction, by seeds and by buds, when either is carried to an extreme degree, which is independent of any incipient sterility."

Most plant-hybrids are relatively infertile, but by no means wholly sterile. With this sterility there is generally augmented vegetative vigour, as shown by Nägeli. Partial or complete sterility, and corresponding luxuriance of root, stem, leaves, and flower may come about in other obscure ways, and such cases are familiar to botanists. Now, it seems highly probable that, either by hybridizing directed to this special end, or by careful selection of forms indicating this tendency to the correlated changes, we may succeed in obtaining important additions to our seedless or nearly seedless plants. Whether the ultimate profit would be large enough to pay for the time and labour involved is a question which we need not enter into; there appears to me no reasonable doubt that such efforts would be successful. There is no reason in the nature of things why we should not have strawberries without the so-called seeds; blackberries and raspberries, with only delicious pulp; and large grapes as free from seeds as the small ones which we call "currants," but which are really grapes from Corinth.

These, and the coreless apples and pears of the future, the stoneless cherries and plums, like the common fruits beforementioned, must be propagated by bud-division, and be open to the tendency to diminished strength said to be the consequence of continued bud-propagation. But this bridge need not be crossed until we come to it. Bananas have been perpetuated in this way for many centuries, and pineapples since the discovery of America, so that the borrowed trouble alluded to is not threatening.

It is absolutely necessary to recollect that, in most cases, variations are slight. Dr. Masters and Mr. Darwin have called attention to this, and have adduced many illustrations, all of which show the necessity of extreme patience and caution. The general student curious in such matters can have hardly any task more instructive than the detection of the variations in such common plants as the blueberry, the wild cherry, or the like. It is an excellent preparation for a practical study of the variations in our wild fruits suitable for selection.

It was held by the late Dr. Gray that the variations in nature by which species have been evolved were led along useful lines —a view which Mr. Darwin regretted he could not entertain. However this may be, all acknowledge that, by the hand of the cultivator, variations can be led along useful lines; and, furthermore, the hand which selects must uphold them in their unequal strife. In other words, it is one thing to select a variety, and another to assist it in maintaining its hold upon existence. Without the constant help of the cultivator who selects the useful variety, there comes a reversion to the ordinary specific type which is fitted to cope with its surroundings.

I think you can agree with me that the prospect for new fruits and for improvements in our established favourites is fairly good.

IV. Timbers and Cabinet Woods.

Can we look for new timbers and cabinet woods? Comparatively few of those in common use are of recent introduction. Attempts have been made to bring into great prominence some of the excellent trees of India and Australia which furnish wood of much beauty and timber of the best quality. A large proportion of all the timbers of the South Seas are characterized by remarkable firmness of texture and high specific gravity. The same is noticed in many of the woods of the Indies. A few of the heavier and denser sorts, like Jarrah, of West Australia, and Sabicu of the Caribbean Islands, have met with deserved favour in England, but the cost of transportation militates against them. It is a fair question whether, in certain parts of our country, these trees, and others which can be utilized for veneers, may not be cultivated to advantage. Attention should be again called to the fact that many plants succeed far better in localities which are remote from their origin, but where they find conditions substantially like those which they have left. This fact, to which we must again refer in detail with regard to certain other classes of plants, may have some bearing upon the introduction of new timber trees. Certain drawbacks exist with regard to the timber of some of the more rapidly growing hard-wood trees which have prevented their taking a high place in the scale of values in mechanical engineering.

One of the most useful soft-wooded trees in the world is the Kauri. It is restricted in its range to a comparatively small area in the North Island of New Zealand. It is now being cut down with a recklessness which is as prodigal and shameful as that which has marked our own treatment of forests here. It should be said, however, that this destruction is under protest; in spite of which it would seem to be a question of only a few years when the great Kauri groves of New Zealand will be a thing of the past. Our energetic Forest Department has on its hands problems just like this which perplexes one of the new lands of the South. The task in both cases is double: to preserve the old treasures and to bring in new.

There is no department of economic botany more promising in immediate results than that of arboriculture.

V. Vegetable Fibres.

The vegetable fibres known to commerce are either plant hairs, of which we take cotton as the type, or filaments of bast-tissue, represented by flax. No new plant hairs have been suggested which can compete in any way for spinning with those yielded by the species of Gossypium, or cotton, but experiments more or less systematic and thorough are being carried on with regard to the improvement of the varieties of the species. Plant hairs for the stuffing of cushions and pillows need not be referred to in connection with this subject.

Countless sorts of plants have been suggested as sources of good bast-fibres for spinning and for cordage, and many of these make capital substitutes for those already in the factories. But the questions of cheapness of production, and of subsequent preparation for use, have thus far militated against success. There may be much difference between the profits promised by a laboratory experiment and those resulting from the same process conducted on a commercial scale. The existence of such differences has been the rock on which many enterprises seeking to introduce new fibres have been wrecked.

In dismissing this portion of our subject it may be said that a process for separating fine fibres from undesirable structural elements and from resin-like substances which accompany them is a great desideratum. If this were supplied, many new species would assume great prominence at once.

VI. Tanning Materials.

What new tanning materials can be confidently sought for? In his "Useful Native Plants of Australia," Mr. Maiden describes over thirty species of "wattles" or acacias, and about half as many eucalypts, which have been examined for the amount of tanning material contained in the bark. In all, eighty-seven Australian species have been under examination. Besides this, much has been done looking in the same direction at the suggestion and under the direction of Baron von Mueller, of Victoria. This serves to indicate how great is the interest in this subject, and how wide is the field in our own country for the introduction of new tanning plants.

It seems highly probable, however, that artificial tanning substances will at no distant day replace the crude matters now employed.

VII. Resins, &c.

Resins, oils, gums, and medicines from the vegetable kingdom would next engage our attention if they did not seem rather too technical for this occasion, and to possess an interest on the whole somewhat too limited. But an allied substance may serve to represent this class of products and indicate the drift of present research.

India Rubber.—Under this term are included numerous substances which possess a physical and chemical resemblance to each other. An Indian Ficus, the early source of supply, soon became inadequate to furnish the quantity used in the arts even when the manipulation of rubber was almost unknown. Later supplies came from *Hevea* of Brazil, generally known as Para rubber, and from *Castilloa*, sometimes called Central American rubber, and from *Manihot Glaziovii*, Ceara rubber. Not only are these plants now successfully cultivated in experimental gardens in the tropics, but many other rubber-yielding species have been added to the list. The Landolphias are among the most promising of the whole: these are the African rubbers. Now in addition to these, which are the chief source of supply, we have *Willughbeia*, from the Malayan Peninsula, *Leuconotis*, *Chilocarpus*, *Alstonia*, *Forsteronia*, and a species of a genus formerly known as *Urostigma*, but now united with *Ficus*. These names, which have little significance as they are here pronounced in passing, are given now merely to impress upon our minds the fact that the sources of a single commercial article may be exceedingly diverse. Under these circumstances search is being made not only for the best varieties of these species but for new species as well.

There are few excursions in the tropics which possess greater interest to a botanist who cares for the industrial aspects of plants than the walks through the Gardens at Buitenzorg in Java and at Singapore. At both these stations the experimental gardens lie at some distance from the great Gardens which the tourist is expected to visit, but the exertion well repays him for all discomfort. Under the almost vertical rays of the sun, are here gathered the rubber-yielding plants from different countries, all growing under conditions favourable for decisions as to their relative value. At Buitenzorg a well-equipped laboratory stands ready to answer practical questions as to quality and composition of their products, and year by year the search extends.

I mention this, not as an isolated example of what is being accomplished in commercial botany, but as a fair illustration of the thoroughness with which the problems are being attacked. It should be further stated that at the Garden in question assiduous students of the subject are eagerly welcomed, and are provided with all needed appliances for carrying on technical, chemical, and pharmaceutical investigations. Therefore I am justified in saying that there is every reason for believing that in the very near future new sources of our most important products will be opened up, and new areas placed under successful cultivation.

At this point, attention must be called to a very modest and convenient hand-book on the "Commercial Botany of the Nineteenth Century," by Mr. Jackson, of the Botanical Museum attached to the Royal Gardens, Kew, which not only embodies a great amount of well-arranged information relative to the new useful plants, but is, at the same time, a record of the existing state of things in all these departments of activity.

VIII. FRAGRANT PLANTS.

Another illustration of our subject might be drawn from a class of plants which repays close study from a biological point of view—namely, those which yield perfumes.

In speaking of the future of our fragrant plants we must distinguish between those of commercial value and those of purely horticultural interest. The former will be less and less cultivated in proportion as synthetic chemistry by its manufacture of perfumes replaces the natural by the artificial products; for example, coumarin, vanillin, nerolin, heliotropin, and even oil of wintergreen.

When, however, one has seen that the aromatic plants of Australia are almost free from attacks of insects and fungi, and has learned to look on the impregnating substances in some cases as protective against predatory insects and small foes of all kinds, and in others as fungicidal, he is tempted to ask whether all the substances of marked odour which we find in certain groups of plants may not play a similar *rôle*.

It is a fact of great interest to the surgeon that in many plants there is associated with the fragrant principle a marked antiseptic or fungicidal quality; conspicuous examples of this are afforded by species of *Eucalyptus*, yielding eucalyptol, *Styrax*, yielding styrone, *Thymus*, yielding thymol. It is interesting to note, too, that some of these most modern antiseptics were important constituents in the balsamic vulneraries of the earliest surgery.

Florists' plants and the floral fashions of the future constitute an engaging subject, which we can touch only lightly. It is reasonably clear that while the old favourite species will hold their ground in the guise of improved varieties, the new introductions will come in the shape of plants with flowering branches which retain their blossoms for a somewhat long period, and especially those in which the flowers precede the leaves. In short, the next real fashion in our gardens is probably to be the flowering shrub and flowering tree, like those which are such favourites in the country from which the Western world has gladly taken the gift of the chrysanthemum.

Twice each year, of late, a reception has been held by the Emperor and Empress of Japan. The receptions are in autumn and in the spring. That in the autumn, popularly known as the Emperor's reception, has for its floral decorations the myriad forms of the national flower, the chrysanthemum; that which is given in spring, the Empress's reception, comes when the cherry blossoms are at their best. One has little idea of the wealth of beauty in masses of flowering shrubs and trees, until he has seen the floral displays in the Imperial Gardens and the Temple grounds in Tokio.

CONCLUSION.

Lack of time renders it impossible to deal with the questions which attach themselves to our main question, especially as to the limits of effect which cultivation may produce. We cannot touch the problem of inheritance of acquired peculiarities, or the manner in which cultivation predisposes the plant to innumerable modifications. Two of these modifications may be mentioned in passing, because they serve to exemplify the practical character of our subject.

Cultivation brings about in plants very curious morphological changes. For example, in the case of a well-known vegetable the number of metamorphosed type-leaves forming the ovary is two, and yet under cultivation the number increases irregularly until the full number of units in the type of the flower is reached. Prof. Bailey, of Cornell, has called attention to some further interesting changes in the tomato, but the one mentioned suffices to illustrate the direction of variation which plants under cultivation are apt to take. Monstrosities are very apt to occur in cultivated plants, and under certain conditions may be perpetuated in succeeding generations, thus widening the field from which utilizable plants may be taken.

Another case of change produced by cultivation is likewise as yet wholly unexplained, although much studied—namely, the mutual interaction of scion and stock in grafting, budding, and the like. It is probable that a further investigation of this subject may yet throw light on new possibilities in plants.

We have now arrived at the most practical question of all, namely—

In what way can the range of commercial botany be extended? In what manner, or by what means, can the introduction of new species be hastened?

It is possible that some of you are aware of the great amount of uncoordinated work which has been done and is now in hand in the direction of bringing in new plants.

The competition between the importers of new plants is so great both in the Old World and the New that a very large proportion of the species which would naturally commend themselves for the use of florists, for the adornment of greenhouses, or for commerical ends, have been at one time or another brought before the public or are being accumulated in stock. The same is true, although to a less extent, with regard to useful vegetables and fruit. Hardly one of those which we can suggest as desirable for trial has not already been investigated in Europe or this country, and reported on. The pages of our chemical, pharmaceutical, medical, horticultural, agricultural and trade journals, especially those of high grade, contain a wealth of material of this character.

But what is needed is this: that the promising plants should be systematically investigated under exhaustive conditions. It is not enough that an enthusiast here, or an amateur there, should give a plant a trial under imperfectly understood conditions, and then report success or failure. The work should be thorough, and every question answered categorically, so that we might be placed in possession of all the facts relative to the object experimented upon. But such an undertaking requires the co-operation of many different agencies. I shall venture to mention some of these.

In the first place, Botanic Gardens amply endowed for research. The Arnold Arboretum, the Shaw Garden, and the Washington Experimental Garden, are American illustrations of what is needed for this purpose. University gardens have their place in instruction, but cannot wisely undertake this kind of work.

In the second place, Museums and Laboratories of Economic Botany. Much good work in this direction has been done in this country by the National Museum and by the department in charge of the investigation of new plants. We need institutions like those at Kew in England, and at Buitenzorg in Java, which keep in close touch with all the world. The founding of an establishment on a scale of magnitude commensurate with the greatness and needs of our country is an undertaking which waits for some one of our wealthy men.

In the third place, Experiment Stations. These may, within the proper limits of their sphere of action, extend the study of plants beyond the established varieties to the species, and beyond the species to equivalent species in other genera. It is a matter of regret that so much of the energy displayed in these stations in this country, and we may say abroad, has not been more economically directed.

Great economy of energy must result from the recent change by which co-ordination of action is assured. The influence which the stations must exert on the welfare of our country and the development of its resources is incalculable.

In the last place, but by no means least, the co-operation of all who are interested in scientific matters, through their observation of isolated and associated phenomena connected with plants of supposed utility, and by the cultivation of such plants by private individuals, unconnected with any State, Governmental, or academic institutions.

By these agencies, wisely directed and energetically employed, the domains of commercial and industrial botany will be enlarged. To some of the possible results in these domains, I have endeavoured to call your attention.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

PROF. BONNEY will begin a course of about sixty lectures on geology at University College, London, on Tuesday, October 6, at noon, and a course of about eighteen lectures on geology for engineering students, on Monday, October 12, at 2 p.m. A class for students preparing for the B.Sc. degree in the University of London will meet on October 6 at 2 p.m.

THE prizes to the students at the medical school of St. Thomas's Hospital will be distributed to-day by Sir G. M. Humphry, F.R.S.

LECTURES will be delivered in Gresham College, Basinghall Street, E.C., on October 6, 7, 8, and 9, by Dr. E. Symes Thompson, Gresham Professor of Medicine, on influenza and its results.

SEVERAL series of lectures for which the Salop County Council has made arrangements have been begun. They are on chemistry, botany, geology, agricultural chemistry, management of stock, insect pests and crop diseases, mechanics, and principles of agriculture, and are being given in various parts of the county. Most of them are being delivered in connection with the Oxford University Extension Scheme.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 21.—M. Duchartre in the chair.—Admiral Mouchez made some remarks on the second volume of the Paris Observatory Star Catalogue, presented to the Academy. The Catalogue contains stars between the right ascensions 6h. and 12h., and about 500,000 observations made at Paris during the last fifty years have been utilized in its construction.—On the colour sensations excited in one eye by coloured light which illuminates the retina of the other, by M. A. Chauveau. From the experiments described it appears that the excitation of one retina by coloured light influences, not only the optic nerves of this retina, but also those of the opposite side, so that the latter are able to awaken the sensation of the colour employed whilst the excited retina only sees the complementary colour. Thus, if a white surface be observed for a short time through a bit of coloured glass, using only one eye, and screening the other, when the glass is taken away the white ground appears to be tinted with a colour complementary to that of the glass. This is an old experiment, but the point is that if the first eye be closed and the screened eye opened the white surface appears to be tinted with the same colour as the glass.—Observations of the asteroid discovered by Charlois on August 28, made with the *coudé* equatorial of Algiers Observatory, by M. F. Sy. Observations for position were made on August 31 and September 7.—Observations of Wolf's comet (1884 *e* III.) made with the *coudé* equatorial (0·36m. aperture) of Lyons Observatory, by M. G. Le Cadet. Observations for position were made on September 9, 10, 11, and 12.—On the partial eclipse of Jupiter's first satellite by the shadow of the second, by M. J. J. Landerer. This phenomenon occurred on August 14.—The metamorphoses of *Acridium peregrinum*, Oliv., by M. Charles Brongniart. The author has specially observed that locusts undergo various colour changes at different stages of their existence.—On the grafting of underground portions of plants, by M. Lucien Daniel.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Mechanics for Beginners; Part 1, Dynamics and Statics: Rev. J. B. Lock (Macmillan).—Manual of the Science of Religion: Prof. P. D. C. de la Saussaye; translated by B. S. Colyer-Fergusson (Longmans).—Solutions: Prof. Ostwald; translated by M. M. P. Muir (Longmans).—Principles and Practice of Plumbing: S. S. Hellyer (Bell).—Lunar Radiant Heat: O. Boeddicker (Williams and Norgate).—The Universal Atlas, Parts 1 to 6 (Cassell).—Mayhew's Illustrated Horse Doctor, revised and improved: J. I. Lupton (Griffith).—Foods for the Fat, 3rd edition: Dr. Yorke-Davies (Chatto).—On the Adjustment and Testing of Telescopic Objectives: T. Cooke and Sons (York, Johnson).—Die geographische Verbreitung der Säugetiere: Dr. A Nehring (Berlin, Pormetter).—De Klimaten der Voorwereld en de Geschiedenis der Zon: E. Dubois (Batavia, Ernst).—Economic Journal, No. 3 (Macmillan).—Journal of the Asiatic Society of Bengal, Vol. lix., Part 2, Nos. 4 and 5; Vol. lix., Part 2, Supplement No. 2; Vol. lx., Part 2, No. 1 (Calcutta).—Journal of Physiology, vol. xii., No. 4 (Cambridge).—Calendar of the University College of Wales, Aberystwyth, 1891–92 (Manchester, Cornish).—Psychology: F. S. Granger (Methuen).—Studies in Jewish Statistics: J. Jacobs (Nutt).—Diphtheria: Dr. R. Thorne Thorne (Macmillan).—Experiments in Aërodynamics: S. P. Langley (Washington).—The Story of the Heavens, 18th Edition: Sir R. S. Ball (Cassell).—Deutsche Seewarte—Indischer Ozean, Ein Atlas (Hamburg, Friederichsen).—Arithmetical Exercises in Chemistry: Dr. L. Dobbin (Edinburgh, Thin).—La Transcaucasie et la Péninsule D'Apchéron: C. S. Gulbenkian (Paris, Hachette).—Ueber die Finnländischen Rapakiwigesteine: J. J. Sederholm (Wien, Hölder).—Studien über Archäische Eruptivgesteine aus dem Südwestlichen Finnland: J. J. Sederholm (Wien, Hölder).—The Eocene and Oligocene Beds of the Paris Basin: Harris and Burrows (Stanford).—Versuch über die Erdgeschichtliche Entwickelung: Dr. G. Pfeffer (Hamburg, Friederichsen).

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THURSDAY, OCTOBER 8, 1891.

THE ICE AGE IN NORTH AMERICA.

The Ice Age in North America, and its Bearings upon the Antiquity of Man. By G. Frederick Wright, D.D., &c. With an Appendix on "The Probable Cause of Glaciation," by Warren Upham, F.G.S.A. With many new Maps and Illustrations. (London: Kegan Paul, Trench, Trübner, and Co., Limited, 1890.)

SWITZERLAND has been called the playground of Europe. The glacial epoch occupies a similar position in geology. Here the student, wearied with the precision of palæontology or of mineralogy, may revel in dreams of omnipotent glaciers, wrap himself in ice sheets, throw mental somersaults, swallow self-contradictory arguments, and be as blind to unpleasant facts as was Nelson at Copenhagen, when he put the telescope to his useless eye, and "spoke disrespectfully" of the signal of recall. To any sarcastic historian of the progress of geology, the literature of ice and its effects will be a boon, since it is so rich in unsound inductions and unstable hypotheses.

Dr. Wright's book, however, is, on the whole, a favourable exception to this general rule. Passages, no doubt, may be found here and there, to which exception might be taken—notably to his remarks on the subject of cirques, in which he regards with favour opinions which are hard to reconcile with expressions in other parts of the book, and rest largely on an erroneous statement—namely, that cirques "are confined to glaciated regions," and "as a rule . . . occupy positions where glaciers first appear." Still, in general his conclusions are supported by facts, very clearly and carefully described, so that we feel, even if occasionally not quite convinced, that his view is worthy of careful and respectful consideration.

But in the matter of ice the subject is long, and our space is brief. It will be better to abstain from criticism of details and give a short outline of those parts of Dr. Wright's book which will be of most interest to readers on this side of the Atlantic. As he states in his preface, his work deals not only with the Ice Age in North America, but also with the whole subject of the Glacial Period. So in its earlier part a considerable space is allotted to glaciers in general and their characteristics; in its later, to the effects of the Glacial Period in other parts of the world, its cause, its date, and its relation to the history of man. These, however, we shall pass over, and confine ourselves to the section dealing with glacial action on the North American continent.

After a sketch of the existing glaciers on the Pacific coast, Dr. Wright gives the results of a study of the Muir glacier in latitude 58° 50′, by the side of which a small party, of which he was a member, camped out for a month. This glacier is about a mile wide where it comes down to the sea, terminating in ice cliffs 300 feet, and sometimes a little above 400 feet in height. The rise inland is gradual—perhaps about 100 feet per mile—and the main body of the glacier occupies a vast amphitheatre, with diameters ranging from 30 to 40 miles. From a number of observations it appeared that the stream of ice entered the inlet, where the cross section was about five million square feet (5000 feet wide by 1000 deep), at an average rate of 40 feet a day (70 feet in the centre and 10 near the margin). It was, however, evident that this glacier, for some time past, had been retreating; indeed, fresh striations and *débris* could be traced to more than 2500 feet above its present surface. Dr. Wright also found below the end of the ice the dead stumps of a forest of cedar trees, erect, and rooted in a clayey soil, but buried beneath glacial gravel. Probably this was deposited by streams, flowing from the advancing ice, which afterwards overrode the mass.

Dr. Wright estimates the amount of sediment which is now being washed down from the basin of the Muir Glacier as equal to nearly one-third of an inch per annum over the total area (1200 square miles) which it occupies. In regard to the vexed question of the excavatory powers of glaciers, Dr. Wright expresses himself, as a rule, cautiously, ascribing to them the formation of true rock-basins under favourable circumstances, but laying stress upon the fact that, in the lower part of their course, where they are beginning to spread out over the lowlands, they can pass, as in the case mentioned above, over quite incoherent materials, without disturbing them. It also seems to follow from his remarks that he regards glaciers as agents of abrasion rather than of erosion, in which we have no doubt he is correct. As another indication of his general caution and candour, we may note that he is careful to point out that striated stones and rock surfaces do not always prove the former presence of a glacier, and may not even have been produced by the action of ice.

A large part of the book is devoted, as a matter of course, to a description of the glaciated area in North America. The boundary of this, as Dr. Wright explains, is sometimes distinctly marked by a terminal moraine, at others it is less definite, being only vaguely indicated by scattered *débris.* But in his opinion—and here he expresses the opinion of the majority of American geologists—there was a time when a large part of Northern America east of the Rocky Mountains was buried beneath a mass of ice. There is, indeed, a driftless area in Wisconsin, which may have formed a kind of *jardin* on a gigantic scale, in this huge *mer de glace*, but, speaking generally, the whole region of the great lakes was covered by an ice-sheet which came down to the sea at Long Island and traversed the northern part of Pennsylvania; thence its irregular frontal margin can be traced to the south-west, until, in the valley of the Mississippi, it reaches almost as far south as the 37th parallel of latitude. Of the various indications of this vanished ice-sheet, the smoothed and striated surfaces of rock, the moraines and boulder clays, the "kames" and "drumlins," Dr. Wright gives careful descriptions and illustrations, usually taken from photographs, so that the evidence is presented as clearly as is possible to the reader. To the last-named phenomena—the "kames" and "drumlins"—and some curious hollows which he calls "kettle-holes," Dr. Wright devotes much attention. The first he regards as indicative of lines of drainage in the closing stage of the Ice Age; the second, as early terminal moraines, modified in shape by the subsequent passage of the ice over them, and so anterior in date to the kames. The kettle-holes occur among morainic deposits, and are thus explained:—As the ice is retreating, a mass of it may be insulated; as this melts, the superincumbent material tends to slip towards the edges, and thus to form a ring of *débris*, by which, after the ice has disappeared, a hollow is inclosed. Dr. Wright also adopts the opinion, maintained by Prof. Claypole, the late Prof. H. C. Lewis, and others, that one effect of the advance of this great mass of ice was to obstruct the flow of all rivers which take a northerly course, and thus to convert their valleys into lakes.

But into a discussion of this interesting question, and of the cause of the glacial epoch, to which a considerable space is devoted, we must not now enter. We must also pass over the questions relating to the date of the glacial epoch and its relation to the first appearance of men, merely stating that Dr. Wright inclines to regard the latter as pre-glacial, but the former as less remote than is generally supposed. It must suffice to say that he appears to be a careful observer, and generally a cautious reasoner, though slightly too prone to quote the remarks of others without due criticism; so that, on the whole, his book presents us with a good summary of the results of investigations into the glacial geology of North America, and will be valuable for purposes of reference on this side of the Atlantic.

T. G. Bonney.

THE TOTAL REFLECTOMETER AND THE REFRACTOMETER FOR CHEMISTS.

Das Totalreflectometer und das Refractometer für Chemiker, ihre Verwendung in der Krystalloptik und zur Untersuchung der Lichtbrechung von Flüssigkeiten. Von Dr. C. Pulfrich, Privatdocenten an der Universität Bonn, und Assistenten des physikalischen Instituts. With 4 Lithographic Plates and 45 Figures in the Text. (Leipzig: W. Engelmann, 1890.)

THIS book contains an exhaustive account of one of the latest devices in physical optics for investigating the refractive power of uniaxial and biaxial crystals. The idea of making use of the principle of total reflection for this purpose is not new. Wollaston, at the beginning of the century, brought forward a method in which the crystal plate under examination was attached to a glass prism; but, owing to the experimental difficulties involved in this process, it met with little practical application. The instrument constructed by Kohlrausch in 1878, in which the crystal plate was immersed in a strongly refractive liquid, was a distinct advance, and has been much used. Within the last ten years, also, Wollaston's apparatus has been considerably improved by Fussner and Liebisch. Both these instruments, however, have still many inconveniences, and it is the claim of the author that the method which he has devised, and which forms the subject of the present work, is free from these.

To give some idea of this method, without entering into practical details, it will be sufficient to state that it consists essentially in the replacement of the prism of the Wollaston instrument by a glass cylinder, to the upper plane surface of which the crystal plate is attached. The cylinder can be rotated about its long axis, so that the refractive phenomena in all azimuths can be observed. This is the distinguishing feature which forms the chief advantage of the new method. Thus, by illuminating the crystal plate from the side at grazing incidence, and slowly rotating the cylinder, the whole extent of the limiting curves of total reflection comes under observation. By a special method of illumination from all sides the limiting curves may be received on a screen beneath the cylinder and made visible to a number of observers; *e.g.* in the case of a uniaxial crystal the appearance on the screen will be the sectional curves of the wave-surface, a circle and an ellipse corresponding to the ordinary and extraordinary rays.

The method was first suggested by the author four years ago. The object of the present work is to give a complete account of the series of measurements and observations which have been made with the instrument since that time with a view to testing its usefulness and trustworthiness. After some preliminary observations on the theoretical principles involved in the method of total reflection, the author gives a detailed description of the construction of the new instrument and the methods of observation by which it is possible in a single crystal section to ascertain the position of the axes of elasticity, to measure the optic axial angle for different colours, and to determine the principal refractive indices. Of special interest is the section on the appearances in the direction of the optic axes of biaxial crystals. Observations made on a plate of asparagine, cut parallel to the optic axial plane, showed distinctly the effects due to the internal and external conical refraction, thus supplementing Lloyd's experiments in demonstrating the general correctness of the Fresnel wave-surface. The last section of the book deals with the refraction of liquids, and contains a description of the refractometer for chemists, which is a simplified form of the total reflectometer, in which a prism replaces the cylinder. Altogether, a perusal of the work leaves the impression that the invention of this ingenious and yet comparatively simple method for investigating the refractive power of doubly refractive media marks a decided advance in physical science; and the author appears to have quite substantiated his claim to have made the total reflection method, which has long been recognized as theoretically the most promising, also a thoroughly practical one.

G. T. P.

A WEATHER RECORD OF THE FOURTEENTH CENTURY.

Consideraciones temperiei pro 7 annis, per Magistrem Wilhelmum Merle, socium domus de Merton. Reproduced and Translated under the supervision of G. J. Symons, F.R.S. (London: Edward Stanford, 1891.)

IN January 1337, barely forty-five years after the death of Roger Bacon, and ten years after the accession of King Edward the Third, William Merle, a Fellow of Merton College, and Rector of Driby, in Lincolnshire, commenced a journal of the current weather as experienced partly at his rectory "in Lyndesay, near the north-east coast," and partly at Oxford. This journal he continued month by month for seven years, or up to three years before his death, the notices of the last four years being considerably amplified over the earlier entries; and the original manuscript, still preserved in the Bodleian Library, has now, thanks to the initiation of Mr.

G. J. Symons, been reproduced in *facsimile* by photography, translated from the monkish Latin of the original text by Miss Parker, and published in a handsome small folio volume, of which one hundred copies have been printed. It is probably, as stated on the title-page, the earliest known weather journal in the world.

The manuscript consists of nine and a half pages of abbreviated Latin, written on vellum in a distinct and easily decipherable text, and is apparently in excellent preservation. It is bound up with a number of other manuscript treatises (one of which is also by Merle) dealing with weather prognostication, astrological lore, and other subjects which, according to the scientific views of the day, were nearly related branches of knowledge. Some of these treatises were collected, and some written by, William Reed, who was Bishop of Chichester from 1369 to 1386, and who bequeathed them to scholars of Merton, "being of his kin." Subsequently, the volume passed into the possession of Sir Kenelm Digby, who, in 1634, presented it, together with other manuscripts, to the Bodleian Library. It is interesting and not uninstructive to note how modest a figure is cut, in this scientific record of the fourteenth century, by the few pages of original observation amid the mass of speculative writings in which they are buried; and how in the nineteenth century they alone retain all their pristine value, and are resuscitated with all the honours of *facsimile* reproduction, while the learned treatises on the conjunctions of the planets, the lunar mansions, and rules for prognosticating the weather, are left undisturbed in the musty dignity in which they have reposed for more than five centuries.

As already remarked, Merle's entries are at first very brief, the notice of each month's weather seldom exceeding two lines of the manuscript. Thus for January 1337 we find:—

> "In January there was warmth with moderate dryness, and in the previous winter [or the previous part of the same winter?] there had not been any considerable cold or humidity, but more dryness and warmth."

Gradually, however, the notes expand, and it is not a little interesting to trace how by degrees the journalist's growing interest in his probably novel undertaking leads him to record more and more in detail the facts that present themselves to his daily observation. Thus from a brief general summary of the characteristic weather of the month, as illustrated in the above quotation, at the end of the year he proceeds to record the character of each week, and towards the end of the third year (1339) he begins to notice the weather of a few special days. From the beginning of 1340 greater amplification is indulged in: the monthly notes often expand to six or eight lines, and in the final year of the record (1343) sometimes to from ten to fourteen lines. In illustration of these more detailed entries, the notice for July 1343 may be quoted:—

> "July.—Considerable heat on the first five days, and it was great on the 3rd and 4th. On the 4th, two or three hours before sunset, heavy thunder began with more vivid lightning than I think I had ever seen, which lasted until midnight, with heavy rain. 5th, light thunder about sunset. On the 6th day and throughout the second week it was gloomy, and there was a slight fog occasionally. 12th, light rain; 14th, gloomy; 15th, and three following days, considerable heat; 19th, rain which penetrated a good deal; 20th, light rain; 22nd, rain; 25th, heavy rain, with heavy thunder in the night, and also in the morning of the following day. All the remainder was rainy, with fog, and rain in small drops, and it was gloomy the whole time. 28th in the night, and 29th in the morning, thunder, with heavy rain. There was lightning with the last two thunderstorms."

For the last four years, indeed, Merle's notes are sufficiently ample to allow of a fair estimate of the weather of those years in comparison with that of the present day, and perhaps some such comparison may be instituted by those who have at command the ample registers of our own time for the same part of Lincolnshire. Seeing how great have been the changes wrought in the character of the surface of the country, by the clearing of forests, drainage, and the extension of agriculture, such a comparison may possibly furnish matter of great interest.

The fourteenth century is sadly memorable for the disastrous famines and pestilences that then desolated England, and above all for the "Black Death," which half depopulated the realm, and was nowhere more fatal than in East Anglia. But this last did not make its first appearance until the end of 1348, about a year after Merle's death, and nearly five years after the conclusion of his journal, which ends abruptly with January 1344; and although a severe famine is recorded in 1335, and another in 1353, it does not appear that any of the years included in his register was especially disastrous. The famine of 1335 is said to have been due to excessive rain, and we may perhaps hazard the surmise that the recent memory of this visitation was the stimulus that induced Merle to record these interesting notes, which good fortune has preserved for us through five and a half centuries.

H. F. B.

OUR BOOK SHELF.

The South Italian Volcanoes. Being the Account of an Excursion to them made by English and other Geologists in 1889, under the auspices of the Geologists' Association of London, with Papers on the Different Localities by Messrs. Johnston-Lavis, Platania, Sambon, Zezi, and Madame Antonia Lavis; including the Bibliography of the Volcanic Districts, and Sixteen Plates. Edited by H. J. Johnston-Lavis, M.D., F.G.S., &c. Pp. 342. (Naples: F. Furchheim, 1891.)

IN this useful volume, Dr. Johnston-Lavis has issued reprints of his report on the Italian excursion made by the members of the Geologists' Association under his direction, and of his abridged sketch of the geology of Vesuvius and Monte Somma, already noticed in this journal. These reprints are accompanied by several interesting original papers—namely, one on the thermo-mineral and gas springs of Sujo, near Roccamonfina, by Dr. Johnston-Lavis himself; one on the geology of Acireale, by Signor G. Platania; another entitled "Notes on the Eolian Islands and on Pumice-stone," by Dr. L. Sambon; and lastly a chapter on "The Travertine and Acque Albule in the neighbourhood of Tivoli," by Signor Pietro Zezi. These various memoirs occupy 88 pages of the volume, the remainder being devoted to a very useful bibliography of Italian vulcanology, compiled by Dr. Johnston-Lavis and Madame Antonia F. Lavis.

Not the least valuable portion of the work is the series of beautiful photographs taken by Dr. Johnston-Lavis from well-selected points of view, and admirably reproduced as small quarto plates. These plates are striking illustrations of what can be accomplished by instantaneous photography as an aid to vulcanological study. Among them are very instructive views of explosive outbursts from the craters of Stromboli and Vulcano. In the case of the small explosions from the first-mentioned volcano, the ejected fragments are seen in the midst of the steam-clouds; and in the case of the more violent eruptions from Vulcano several phases in the same outburst have been caught at intervals of a few seconds. Those who already know this very interesting district will be glad to have their recollections revived by these admirable plates; and those who have never had the pleasure of visiting the South Italian volcanoes may obtain from these remarkable photographs a much better idea of the localities than any descriptions or drawings can possibly give.

Buried Cities and Bible Countries. By George St. Clair, F.G.S. (London: Kegan Paul, Trench, Trübner, and Co., 1891.)

Everyone knows that recent archæological research has brought to light a vast number of facts which are directly or indirectly connected with ancient Hebrew history. The object of the author of the present work is to set forth the more important of these facts, and to explain their significance. He deals with the results of exploration in Egypt, Palestine, and Mesopotamia; and he has a chapter on Jerusalem, with regard to the topography of which he has been led to conclusions different from those of other writers. The book has been prepared for the benefit of persons "who have no time to follow the course of exploration, and no taste for technical details"; and readers of this class will find in it much that will be to them both new and interesting. The value of the text is increased by good maps, plans, and other illustrations.

Food, Physiology, &c. By William Durham, F.R.S.E. (London and Edinburgh: A. and C. Black, 1891.)

This is the third volume of a series by Mr. Durham, entitled "Science in Plain Language." The author does not pretend to say anything new, but he has brought together, and arranged clearly, a mass of facts which will no doubt be of interest, and may be of practical service, to many readers who have neither time nor inclination for the study of more elaborate treatises. He begins with the consideration of solid and liquid foods, then gives some account of the constituents of food, and finally sketches the structure and functions of the bodily organs.

Blackie's Science Readers. (London: Blackie and Son, 1891.)

The aim of this series is to arouse the interest of children in the common objects of the natural world, and to give them some insight into the processes by which articles of ordinary use are produced. The idea is excellent, and has been very successfully worked out. The series consists of five little volumes, the first two of which present some "lessons on common objects." From the third volume the reader will learn something about the simple principles of classification; about substances used in arts and manufactures; about phenomena of earth and atmosphere; and about matter in three states—solids, liquids, and gases. The fourth and fifth volumes—by the Rev. Theodore Wood—deal with animal and plant life. The facts set forth have been carefully selected, and they are presented in a bright, easy, natural style which cannot fail to make them at once intelligible and attractive. Good teachers will find the series of real service in helping them to foster in the minds of their pupils a love of accurate observation and independent reasoning.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

Comparative Palatability.

With the view of supplementing the experiments carried out last year by Mr. F. Finn and myself (Nature, vol. xlii. pp. 571, 572), I have been feeding, during August and September, specimens of the common frog and toad.

Among Hymenoptera, *Bombi* are readily taken by frogs. I have records of *B. lapidarius* (drones and workers), *terrestris* (queens and workers), and *muscorum* (drones and workers). On one occasion only a freely-feeding frog refused to attack for the second time a large queen of *terrestris*, which had stung its mouth. Many of the insects were, however, thus taken at the second attempt. The common wasp was eaten eagerly by frogs and toads. I was again unfortunate in not taking any *Chrysididæ*. *Sirex gigas* was attacked both by a frog, for which it seemed too large, and by a toad, under whose lip it appeared to insert its ovipositor. Neither animal ventured to seize it again—certainly for an hour or so. I was then obliged to abandon the observation. I could get no large ichneumons.

Of Lepidoptera, *Vanessa urticæ* was taken by frogs and toads, and *V. io* by a frog. Three or four specimens of *Pieris rapæ* and *napi* would be taken in succession by a frog, which also ate *P. brassicæ*. The insects' flutterings did not seem to matter: more than once they were taken on the wing. A toad once took *P. rapæ*. I was surprised to see a frog seize a dead specimen of this butterfly, which had been lying for several hours in the inclosure. It was partially swallowed, but rejected after some seconds—having unfortunately been taken together with some cedar needles. *Plusia gamma* was eaten eagerly by both frogs and toads. Hairy caterpillars (*e.g.* of *Orgyia antiqua* and *Spilosoma* sp.) were taken by a frog. Smooth green larvæ were eaten greedily.

Of Diptera, *Eristalis tenax* was eagerly seized by frogs and toads. A red-tailed, long-winged fly was eaten by a frog.

Blatta orientalis was taken without hesitation; as were, of course, earthworms.

Of three frogs under observation, only one was of much working value. This specimen (a male) became in a fortnight so tame as to attempt to take the handle of the butterfly-net with which I placed the insects, &c., in the inclosure. This fact recalls Mr. E. B. Poulton's observation, that his tree-frogs seized the end of the forceps with which food was given them.

It is, perhaps, worthy of notice that the larvæ of the blow-fly, though eaten eagerly by toads, are frequently passed whole from the body; and would, therefore, seem to be with difficulty digested.

Want of time has prevented my experimenting, as I had wished to do, with *Salamandra maculosa*. Mr. F. Finn offered a specimen to ducks, which will eat the small newt, and found that though more than one bird observed it, and one even ran towards it, it was not touched. The observation extended over more than an hour.

E. B. Titchener.

Mote House, Mote Road, Maidstone, September 25.

Alum Solution.

Dans le no. 1141 de votre excellente Revue, M. Napier Draper demande pour quelle raison la solution d'alun a été universellement adoptée pour l'absorption des radiations de grande longueur d'onde. Ce n'est point pour répondre à cette question que je vous écris, car, pas plus que votre correspondant, je ne connais d'expériences directes suffisamment exactes desquelles il résulterait que la solution d'alun absorbe plus que l'eau pure. Je hasarderai, cependant, une explication: l'eau est un des liquides transparents les plus absorbants; l'alun occupe un rang analogue parmi les solides; en dehors de toute vérification, si l'absorption sélective de chacun de ces corps s'exerce sur une partie différente du spectre, on peut supposer que leur mélange exerce une absorption plus complète que chacun des corps pris isolément.

A cette occasion, je prendrai la liberté de relever une erreur que l'on a fréquemment commise dans ces derniers temps au sujet de l'absorption des radiations infra-rouges par l'eau. On a coutume de définir le rendement d'un foyer de lumière par le rapport de l'énergie située dans la partie visible du spectre à l'énergie totale rayonnée par le foyer. Sans insister sur ce que cette définition a de défectueux (je traiterai prochainement cette question dans la *Revue générale des Sciences*), je rappellerai qu'on mesure d'ordinaire le rendement en recevant successivement sur un radiomètre quelconque (pile de Melloni, bolomètre, radiomicromètre de Boys) la radiation totale du foyer, et la radiation qui a traversé une certaine épaisseur d'eau; on admet que les radiations obscures ont été retenues, et on fait le quotient de ces deux quantités. Aucun physicien, je suppose, ne croit que l'absorption par l'eau commence à l'endroit précis où cesse la vision, et devient immédiatement totale, mais on pense en général que le résultat ainsi obtenu est assez approché.

Or nous pouvons déterminer directement le rendement photogénique d'une source en mesurant la superficie des courbes d'énergie rayonnante visible et invisible. En partant des nombres de M. Langley, on trouve ainsi, pour le rendement d'une lampe à gaz une valeur comprise entre 1 et 2 pour cent. D'autre part, les recherches de M. Knut Ångström ont montré que l'absorption par l'eau est presque nulle pour $\lambda = 1\mu$, et n'est totale qu'à partir de $\lambda = 2\mu$ environ. Une couche épaisse d'eau laisse passer près de 10 pour cent de l'énergie rayonnante invisible. La méthode ordinaire donnerait donc, pour le rendement d'une lampe à gaz, 11 à 12 pour cent, c'est à dire une quantité six fois trop forte.

Je ne quitterai pas ce sujet sans faire remarquer le singulier usage en vertu duquel la puissance de la radiation solaire est rapportée à la *minute*, tandis que toutes les puissances possibles—cheval, *horse-power*, watt, ainsi que toutes les radiations—sont exprimées par rapport à la seconde. Il serait temps de faire disparaître cette anomalie.

Ch. Ed. Guillaume.

Pavillon de Breteuil, Sèvres, France,
25 septembre, 1891.

Weather Cycles.

With reference to this most interesting question, may I be allowed to call attention to the following figures? Having had to consult Dr. Rutty's "Natural History of Dublin," 1772, vol. ii., I casually found on p. 353 of that volume, in his remarkable detailed registry of the weather in Dublin for a long series of years, the following remark: "It has been remarked that the following years were memorable for great frosts in England, viz. 1638, 1661, 1684, 1708, 1716, 1739." Now the intervals between these dates are 23, 23, 24, 8, 23. He further remarks, on p. 368:—"It is to be observed that whereas since the great frost of 1739, until the latter end of the present summer, 1744, we had generally an unusual prevalence of dry weather, in autumn our usual wet weather returned." It may be remarked that the interval of 23 years is about double the sun-spot period, and furthermore that the years mentioned by Rutty correspond roughly with years of sun-spot minima or maxima as given in Wolf's Catalogue, mentioned by Guillemin in his work "Le Ciel" (1877), p. 104. This correspondence would appear as follows:—

Sun-spot Year.	Interval.	Great colds.	Interval.
1639·5 min.		1638	
	20·5		23
1660 min.		1661	
	25		23
1685 min.		1684	
	20·5		24
1705·5 min.		1708	
	12·5		8
1718 min.		1716	
	20·7		23
1738·7 min.		1739	
	16·8		15
1755·5 max.	22·8	1754	23
	6·0		8
1761·5 max.		1762	

J. P. O'Reilly.

Royal College of Science for Ireland, Stephen's Green,
Dublin, September 25.

Occurrence of the Ringed Snake in the Sea.

The readiness with which the British snake (*Tropidonotus natrix*) will enter fresh water is well known. Its occurrence in the sea seems anomalous, and therefore I venture to submit the following details.

The specimen in question was seen on September 7, from a small boat on the east coast of the Isle of Wight, while about a thousand yards distant from the shore, and about midway between Shanklin and Luccombe Chines. When first seen it was swimming straight out to sea—viz. in an easterly direction. The sea was calm and a strong current was flowing from the south, so that the creature was swimming across the current. At first it took no notice of the boat, but as the boat was rowed towards it, it changed its course and swam directly away from the boat. It was soon captured, and found to be uninjured and in good condition. Upon dissection it proved to be a male; the entire alimentary canal was absolutely empty. The internal organs were free from disease or other abnormality. It measured 33 inches in length. It is most probable that this snake entered the sea about a mile from where it was obtained, as the beach is bounded by almost perpendicular cliffs, some 300 feet high, at that place.

J. Cowper.

A Rare Phenomenon.

Mr. Wilson's letter in your issue of September 24 (p. 494), recalls what I myself saw on the same evening. On Friday, the 11th, I was returning with a friend to town after a day's ramble in Epping Forest. We caught the 8.36 p.m. train at Epping, which is due at Woodford at 8.59, and was, I think, only a few minutes late. Just as the train was nearing Woodford Station, my friend and myself simultaneously noticed a luminous band, such as that observed by Mr. Wilson, and extending from the horizon almost to the zenith. Our first unreflecting thought was to refer it to the revolving light at the Naval Exhibition, only it did not revolve, and the direction was quite wrong. The fact that both of us thought of this is indicative of the appearance which the luminous beam bore. The night was clear and starlit, and I observed that the point in the horizon from which the beam rose was almost under the Great Bear, but a little to the left as I faced it. We saw it only for a minute or two before it was hidden from us by the shed of Woodford Station, in which station we stayed for what seemed a long while. When we got into the open country again, the phenomenon had disappeared. I may add, that my own eye being unfortunately defective for red, I asked my companion if he noticed any red tinge in the light, and he answered that it seemed quite white.

Burlington House. Herbert Rix.

The narrow luminous band described in Nature, September 24 (p. 494) was seen here on Friday, the 11th inst., between 8.30 and 9 p.m., at the same time at which it was seen by Mr. Wilson in the county Westmeath, but about twenty-two hours later than it was seen by Prof. Copeland in Aberdeenshire. It passed close south of Cassiopeia, and nearly through the zenith. Half an hour later it had drifted 8° or 10° southward, and had become very faint.

There can be little doubt that the very rapidly moving "comet" seen by Mr. Eddie at Grahamstown, South Africa, on October 27, 1890, was a phenomenon of this kind.

J. L. E. Dreyer.

The Observatory, Armagh, September 28.

The Heights of Auroras.

The rare part of the phenomena described by your correspondents is the extreme narrowness of the auroral arches seen on the 10th in the north of Scotland, and on the 11th at Ryde. I take all the other descriptions on the 11th to refer to one arch—a different one from that seen at Ryde; and it was a much wider one, and therefore less unusual, its width having been about 5° as seen here. Your correspondents do not give its width, except that, as seen from Nottingham, it was evidently very broad, and is not stated to have been an arch at all, though I should suppose it was one. The observation at Nottingham Forest, compared with those further north, gives a good opportunity for ascertaining the height of the top of the aurora; but, as Mr. A. Marshall has not given the altitude of the base of the aurora as seen from Nottingham, there are no materials for calculating the height of that. I made several observations of the position of the central line of the arch. I might specify that at 9.25 it was at R.A. 20h. 42m., Decl. + 33½°, and R.A. 0h. 43m., Decl. + 33°, and it moved very slowly.

Is it not time some systematic effort was made to calculate the heights of auroras? A good many observations have been made on this point, showing great variation in height; and yet, beyond the conclusion that it seems probable they may be seen at lower elevations nearer the magnetic pole than elsewhere, we know nothing as to whether they vary in height with the place, the time, or the nature of the auroras. Now is the time, seeing that auroras appear to be becoming more numerous than they have been for many years past. T. W. Backhouse.

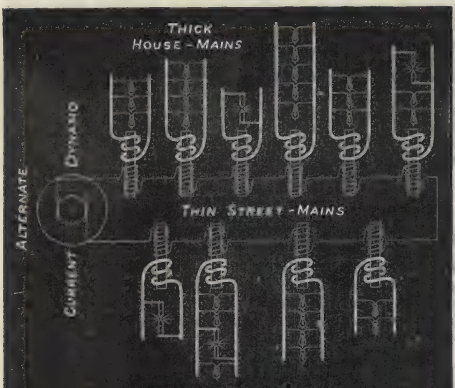
West Hendon House, Sunderland, October 5.

SOME NOTES ON THE FRANKFORT INTERNATIONAL ELECTRICAL EXHIBITION.[1]

III.

From One Hundred to Twenty Thousand Volts.

The incandescent lamp having, by 1885, reached a fair degree of perfection, it appeared that the one need still remaining, in connection with the distribution of the electric light over a large area, would be supplied by the use of transformers. For a transformer with many convolutions of fine insulated wire on one coil, and a few convolutions of thick insulated wire on the other, would transform a large pressure and small current into a small pressure and large current; hence, if such a transformer were placed in each house, it would be possible to light up even a scattered district by a comparatively fine wire from a central station, whereas previously it had seemed that it would be necessary to use copper conductors many square inches in cross-section to light many houses even when at no great distance from one another.



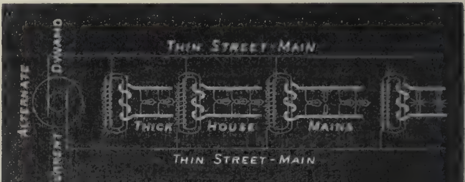
Transformers in Series
Gaulard and Gibbs' Original Arrangement.
Fig. 2

Hence, in the autumn of 1885 we find Messrs. Gaulard and Gibbs making preparations at the Grosvenor Gallery, Bond Street, for establishing there the pioneer central station for London.

But the method they adopted was that of placing the transformers in series, as seen in Fig. 2, and this system has the great disadvantage that the brightness of the electric lamps in a house cannot be kept automatically constant when other lamps in the same house are turned on or off. There are, of course, two conditions to be fulfilled in electric lighting: one, that turning on or off lamps in one house shall not affect the brightness of the lamps in any other house; the other, that turning on or off lamps in one room shall not affect the brightness of the lamps in any other room of the same house. With transformers in series, the first condition is satisfied by keeping the alternating current which passes through the fine wire or *primary coil* of the transformer perfectly constant; but this does not render the potential difference between the wires from the *secondary circuit*, or house mains, independent of the current in this secondary circuit—that is, independent of the number of lamps turned on in the house. Consequently, the series arrangement of transformers adopted by Messrs. Gaulard and Gibbs, while rendering the lamps in one house independent of those in another, did not attain the same result for lamps in different rooms of the same house.

Complaints, therefore, became general. Various unsuccessful devices were tried to remedy this evil, when an application was received from Mr. Sebastian Ziani de Ferranti to be allowed to try a transformer which he had designed. The application was accepted, for Mr. Ferranti, although quite young, was already known as having constructed an ingenious alternate-current dynamo, and in February 1886 the charge of the Grosvenor Gallery central station passed over into Mr. Ferranti's hands.

The new engineer recommended that the system of placing the transformers in series should be totally discarded, and that a parallel arrangement should be adopted in its place, as in Fig. 3, because a well-made transformer had this important property—that if the potential difference at the terminals of the primary coil were kept constant, the potential difference between the terminals of the secondary coil would also remain nearly constant whatever were the current passing through this circuit; so that if the pressure between the street mains were always kept the same, the brightness of the lamps would hardly be affected either by turning on or off lamps in the same or in any other house.



Transformers in Parallel
Arrangement Now Adopted
Fig 3.

Placing the transformers in parallel, however, would necessitate working at a low pressure, said the press, and would rob the transformer system of all its value, for "it is surely not proposed for one moment to work a parallel system where the primary has a difference of potential of 2000 volts." However, that is exactly what Mr. Ferranti not only proposed to do, but what he actually carried out on a large scale, so that his mains by 1888 stretched from Regent's Park to the Thames, and from Chancery Lane to Hyde Park, supplying current to some 20,000 glow-lamps. The Board of Trade had made regulations, about 200 volts being the maximum pressure permitted in a house; Parliament had passed the Electric Lighting Act of 1882, containing clauses rendering the development of the electric lighting industry well nigh commercially impossible; but Mr. Ferranti overcame all these legalities by bridging his mains from house-top to house-top, instead of putting them under the streets and himself under the control of the authorities.

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[1] Continued from p. 524.

But every corner at the Bond Street central station had soon to be utilized; a dynamo weighing tons had on one occasion to be lifted into position over a steam-engine necessarily kept always running to maintain a constant supply of current to the houses. New customers were added daily to the list, more and more current had to be generated nightly, in the face of engineering difficulties, and in the teeth of injunctions against smoke, injunctions against dust, and injunctions against noise.

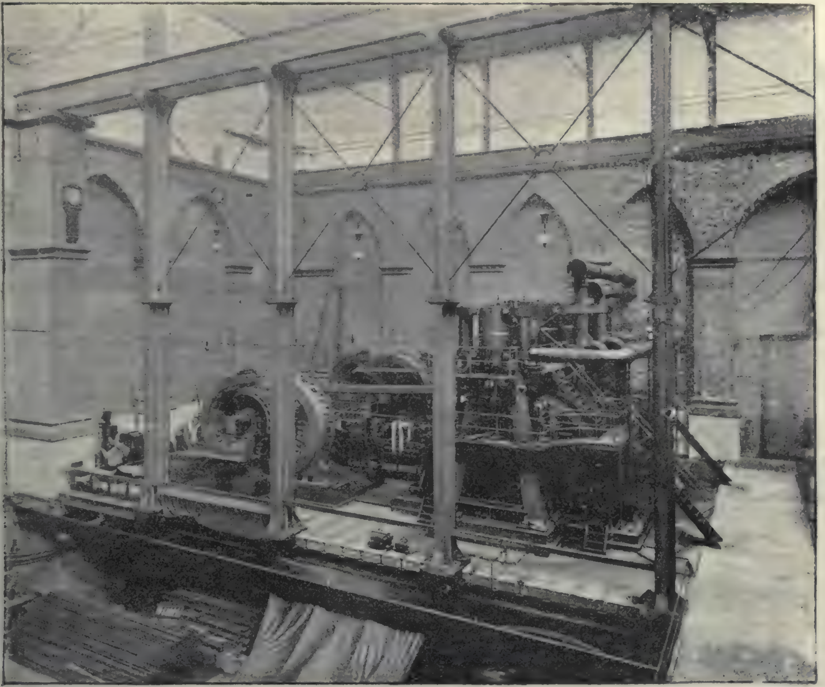
Fig. 4.—Two 1250 horse-power dynamos (opened for inspection) at Deptford.

A fresh start became imperative, so it was decided to build at Deptford, 6 miles away from Bond Street, a vast generating station, which should be the largest in the world, and to use the Grosvenor Gallery, and probably fresh sites to be obtained in town, merely as transforming stations. In the mains between Deptford and London it was decided to employ 10,000 volts, to be reduced to 2400 in London, and the power then distributed by the existing overhead mains, and again reduced to 100 volts on entering the houses, as before.

The scheme was a far-reaching one; permission was asked from the Board of Trade by the London Electric Supply Corporation, the outcome of the original Grosvenor Gallery Syndicate, to run wires along 27 railways and through 30 parishes; two dynamos, each to furnish 1250 horse-power at 10,000 volts, were built with special engines to drive them, as seen in Fig. 4, and a cable laid to London. But on starting the dynamos, when they were completed, it was found that the insulation of the cable would not stand 10,000 nor even 5000 volts; and for a time power was supplied direct from Deptford to the houses in London, one transformation at the houses themselves being alone effected.

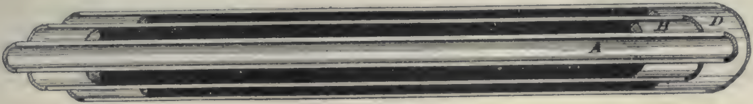

Fig. 5.—Longitudinal section of the Ferranti main. A, inner copper tube; B, outer copper tube; D, iron protecting tube; waxed paper insulation shaded black.

Then Mr. Ferranti carried out his original intention of constructing the main of two concentric copper tubes, to serve respectively as the going and return conductor. The inner copper tube, 20 feet long, seen in section, A, W.E. Ayrton, "Some Notes on the Frankfort Int. Electrical Exh." 44

Fig. 5, has brown paper soaked in ozokerit rolled round it to a thickness of about five-eighths of an inch. Outside this is slipped a larger copper tube, B, Fig. 5, and the whole is drawn through a taper die under great pressure, which has the effect of forcibly compressing the paper and consolidating the mass. Next, more brown paper soaked in melted ozokerit is rolled on, to a thickness of one-eighth of an inch, and the whole slipped loosely into an iron tube, D, Fig. 5, which protects the cable subsequently from mechanical injury. To fill up any air spaces that may have been left between the iron and the outer copper tubes, the 20-feet section is placed over a fire, and melted wax pumped in between the two through a tube inserted in a hole drilled in the middle of the iron tube.

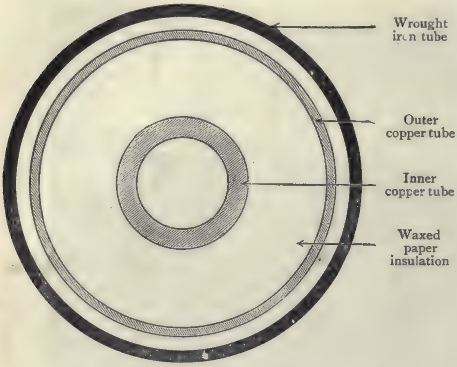

FIG. 6.—Cross-section of the Ferranti main; full size.

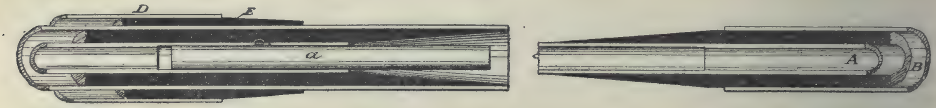

FIG. 7.—Ends of two pieces of main, tapered ready for jointing. *a*, copper rod to make electric connection between inner copper tubes; E, waxed paper coned like a pencil.

Fig. 6 shows a cross-section of the finished main full size, and as the sectional area of the metal in each of the copper tubes is about a quarter of a square inch, the main can transmit about 2000 horse-power at 10,000 volts.

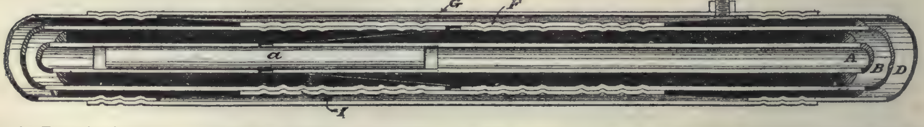

FIG. 8.—Ferranti main, jointed. F, copper sleeve slipped over two ends of outer copper tubes, and then corrugated with special tool; G, iron sleeve slipped over two ends of iron tubes, and corrugated with special tool; H, screw-hole to run in melted wax, I.

The main being constructed in lengths of only 20 feet, some 1500 joints have had to be made in 6 miles of main, or 6500 joints altogether in the five mains which have been laid from London to Deptford. These joints have been made without solder, in the way shown in Figs. 7 and 8, pressure alone between the copper tubes having been relied on to maintain good contact.

The object of using concentric tubes is twofold—first, as the outer copper tube is kept practically at the potential of the earth, it is impossible to get a severe shock unless the inner tube is touched, and this, of course, can only be done by first cutting through the outer; second, the effective increase of the resistance and of the self-induction which occurs with rapidly alternating currents in consequence of the mutual action of the currents in different parts of the conductor on one another is much less for a given cross-section of copper with concentric tubes than with two insulated rods placed side by side. For example, Sir William Thomson has calculated that if copper be employed in the form of a solid rod, 1·2 inch in diameter, the resistance for an alternating current of a frequency of 80 per second will be 31 per cent. greater than for a steady current.

It is very questionable, however, whether these advantages of using concentric tubes are not more than compensated for by the large electrostatic capacity that such a cable possesses. For, as is now fully recognized, the combination of capacity and self-induction can by a species of resonance cause the difference of potential in the circuit to be far greater than the E.M.F. of the dynamo itself, and in certain cases, very dangerously greater.

As soon as the Deptford main was constructed to stand 10,000 volts, it was found that one of the dynamos seen in Fig. 4 broke down at this pressure, and therefore for many months the current was sent from Deptford at only 5000 volts; next, the transformer room at the Grosvenor Gallery was burnt down through carelessness, some £8000 worth of transformers destroyed, and a portion of London left in darkness for two or three weeks. New transformers were hastily, too hastily, constructed, and the current was turned on again at the commencement of last December; but after a few days the transformers were, one after another, short-circuited by the electric current sparking from the primary coil to the iron core of the transformers, and all the houses on the London Electric Supply Corporation's system again left in darkness during the nearly perpetual night of a densely foggy winter. The Metropolitan Electric Supply Company—which also distributes an alternating current by means of transformers, but from several central stations in the heart of London itself, and therefore requiring to use only 1000 volts and a single transformation—came to the rescue in certain districts, but in others the householders had to be left to their fate, as it would have been far too expensive to run special mains from the Metropolitan Company's stations merely as a temporary expedient.

Finally, in March of this year, current was again turned on from Deptford, at the pressure originally proposed, viz. 10,000 volts. It was not, however, supplied from the dynamos illustrated in Fig. 4; but, instead, Messrs. Deprez and Carpentier's plan of transforming up and transforming down again, illustrated in Fig. 1, p. 522, was employed. For, by this time, two dynamos, formerly at the Grosvenor Gallery, each of 600 horse-power, had been taken to Deptford and erected there, as seen in Fig. 9; new steam-engines, more powerful than those formerly employed at the Grosvenor Gallery, having been constructed to drive them.

These dynamos generate the current at 2400 volts, then, by means of transformers at Deptford, this is raised to 10,000 volts. On the power arriving in London, the pressure is transformed down again to 2400 volts, and at the houses there is a further transformation of this 2400 volts to 100 volts. There are, therefore, no less than three transformations of pressure between the dynamo terminals at Deptford and the lamps in the houses in London.

Regarded as a gigantic experiment in electrical engineering, the Deptford scheme has achieved a gallant victory, for, with a buoyancy that no disaster could crush, and with the determination of a Napoleon to conquer every mechanical and electrical obstacle in the way, Mr. Ferranti has step by step succeeded in distributing current to quite distant parts of London at a pressure which in 1885 was regarded as quite impracticable, and for the last seven months he has been sending the power to London at a pressure which, even at the end of last year, was deemed simply visionary.

But as a commercial undertaking the Deptford transmission is a dreary failure, since what is the advantage of transmitting the current 6 miles that is in any way commensurate with the capital already expended? When power can be obtained very cheaply, from a rapid river for example, it may be highly remunerative to transport it in some such way as is now being done between Lauffen and Frankfort. But can power be obtained so much more cheaply at Deptford than in London to make it worth while transmitting it over 6 miles? Land undoubtedly costs much less down the river than in the heart of London, coal can be very easily brought to a generating station on the banks of the Thames, and the water might perhaps be employed to work condensing steam-engines; but such economies can only compensate for a fraction of the yearly interest on the capital expended on the Deptford scheme. Indeed, even if the station at Deptford had been built with rigid economy, and only large enough for the present demand, it is questionable whether the loss of power in three transformations of the pressure would not eat up much of the saving that could be effected by having the generating station quite out of London.

As it is, however, the London Electric Supply Company have been so engrossed with the electric lighting of London in the *future*, that they have practically ignored the present wants of the householder; the vast building at Deptford has been constructed to carry a second story

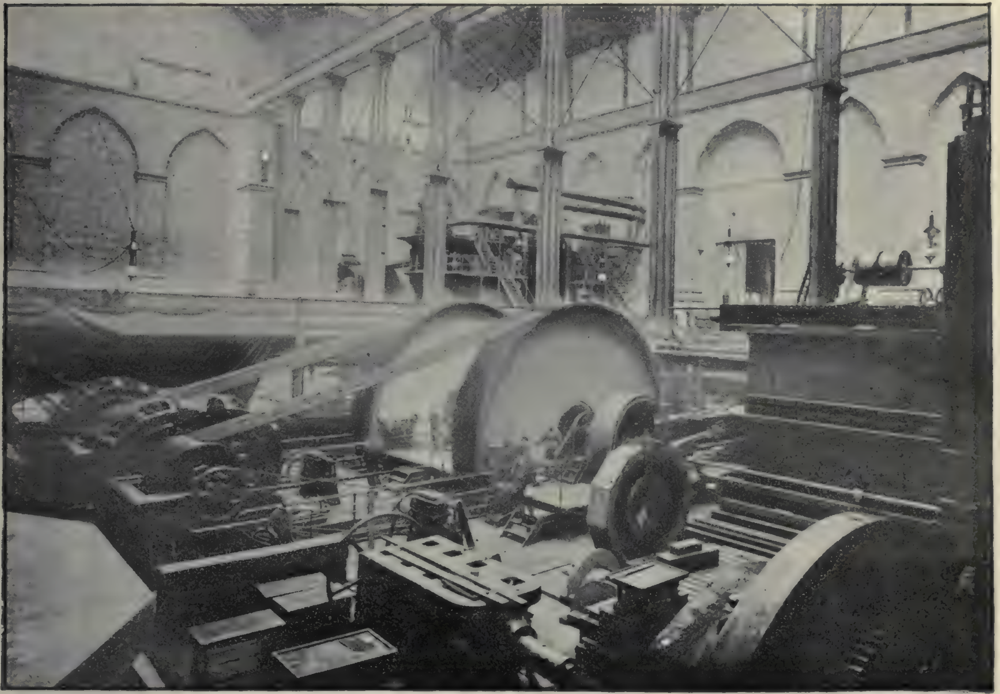

FIG. 9.—Two of the Grosvenor Gallery dynamos re erected at Deptford and driven by new steam-engines. Two 1250 horse-power dynamos at the back.

of boilers and engines, when it is very doubtful if even the present story can be wholly utilized for a long time to come; rows of boilers and furnaces were erected some two or three years ago to supply steam to drive dynamos which are not yet made; tens of thousands of pounds have been expended on machinery to be employed in constructing two ten-thousand horse-power dynamos, and the armature of one of them, 43 feet in diameter, has had to be left abandoned only half finished, because there is neither money nor present need for such a dynamo at Deptford.

And while all these provisions for the future electric lighting of London on a vast scale were slowly proceeding, the present customers were left sometimes for hours, sometimes for days, and occasionally even for weeks in darkness: what wonder is it, then, that all over London there have have been growing up central stations supplying a direct current at low pressure, and that many of the householders who formerly received current from the overhead wires of the London Electric Supply Corporation have had their houses connected instead with the low-pressure underground mains of other companies?

To the world at large, however, the Deptford undertaking has been of immense value, for it has shown the possibility of practically using the very high potential differences absolutely necessary for economically transmitting power over such distances as that between Lauffen and Frankfort. Hence, maintaining 20,000 volts between *bare* wires running for 109 miles along the side of the Neckar railway, at a height of only 16 feet from the ground, sounds much less startling now than did Mr. Ferranti's proposal made and acted on five years ago to bring only one-tenth of this pressure, by means of india-rubber covered conductors, into locked transformer rooms built of brick in the basement of the houses supplied with current from the Grosvenor Gallery.

In fact, the results that have been attained through Mr. Ferranti's undaunted courage, and the well-filled purses of his friends, have led people to look on a pressure of 20,000 volts as they regard a velocity of 70 miles an hour, so that to day, in order to prevent boys climbing up any one of the 3000 ordinary telegraph poles which carry the wires from Lauffen to Frankfort, it is thought sufficient to merely paint a skull and cross-bones on every post as an indication of the deadly fate that awaits the climber.[1]

(*To be continued.*)

ON VAN DER WAALS'S TREATMENT OF LAPLACE'S PRESSURE IN THE VIRIAL EQUATION: IN ANSWER TO LORD RAYLEIGH.

MY DEAR LORD RAYLEIGH,—As you are aware, I did not see your letter of September 7 (NATURE, 24/9/91) till a fortnight after its date; and my reply has been further delayed for a week in consequence of the closing of Edinburgh University Library at this season. Even now I can refer only to the German version of Van der Waals's pamphlet.

Partly on account of its unfamiliar language, but more especially on account of a very definite unfavourable opinion expressed by Clerk-Maxwell (NATURE, 15/10/74) I did not attempt to read the pamphlet when it appeared; and it was not till 1888 that, in consequence of some hints from Dr. H. Du Bois, I hastily perused it in its German form.

The passage which you quote from my paper (where, by the way, the printers have unfortunately put *resistance* for *resilience*) is certainly not a very accurate description of Van der Waals's method, but it represents faithfully the difficulties which I felt on first reading the pamphlet. I said that Van der Waals's "justification of the introduction of the term a/v^2 into an account already closed, as it were, escapes me." And I am not surprised that it did so. For the statement of Clerk-Maxwell had prepared me to look for error; and when, at the end of Chap. VI., I met with the formula

$$p(v-b) = \mathrm{R}(1+at),$$

which, a couple of pages later (nothing but general reasoning intervening), somehow developed itself into

$$\left(p+\frac{a}{v^2}\right)(v-b) = \mathrm{R}(1+at),$$

I naturally concluded that this was the matter adverted to. I spoke of the first of these equations as a "closed account," because of the process by which b had been introduced. To this point I must presently recur.

I had not examined with any particular care the opening chapters, to which your letter chiefly refers; probably having supposed them to contain nothing beyond a statement and proof of the Virial Theorem (with which I was already familiar) along with a reproduction of a good deal of Laplace's work.

Of course your account of this earlier part of the pamphlet (which I have now, for the first time, read with care) is correct. But I do not see that any part of my statements (with perhaps the single exception of the now italicized word in the phrase "the *whole* procedure is erroneous") is invalidated by it. No doubt, the sudden appearance of a/v^2 in the formula above quoted is, to some extent at least, accounted for; but is the term correctly introduced?

The formula you give would lead, on Van der Waals's principles as to the interpretation of $\frac{1}{3}\Sigma(mV^2)$, to

$$v(p+\mathrm{K}) = \mathrm{R}(1+at),$$

or

$$v\left(p+\frac{a}{v^2}\right) = \mathrm{R}(1+at).$$

But how can the factor $(v-b)/v$, which Van der Waals introduces on the left in consequence of the finite diameters of the particles, be justifiably applied to the term in K as well as to that in p? Yet to apply it so is essential to Van der Waals's theory; for without it the resulting equation will not give a cubic in v, and cannot therefore be applied to the isothermals for which it is required. And, in any case, it could scarcely be said that the K term, after being manipulated in this manner, is, in any strict sense, "extracted from the term $\Sigma(\mathrm{R}r)$."

A very strange thing appears, in this connection, in the German version. A result, due it seems to Lorentz (which, in ignorance of his work, I had reproduced and published in the first part of my paper), leads directly to the equation

$$pv = \mathrm{R}(1+at)\left(1+\frac{b}{v}\right);$$

which is then put in the confessedly approximate form

$$p(v-b) = \mathrm{R}(1+at).$$

Of this it is remarked:—"was genau mit dem obigen Resultate [that obtained by the use of the factor $(v-b)/v$] übereinstimmt." It is obvious that, when we have to divide both sides by $v-b$, we ought to restore the proper factor on the right; and thus that the equation ought to take the final form

$$p+\frac{a}{v^2} = \mathrm{R}(1+at)\,\frac{v+b}{v^2},$$

instead of the more convenient form

$$p+\frac{a}{v^2} = \frac{\mathrm{R}(1+at)}{v-b},$$

in which Van der Waals employs it. But then it would not give the required cubic in v!

I think that the mere fact of Van der Waals's saying (in a passage which is evidently applicable to his own processes, though it is applied only to that of Lorentz) "die ganze Rechnung doch nur bis auf Grössen der ersten Ordnung (wie b/v) genau ist" throws very grave doubt on the whole investigation. For in the most interesting part of the critical isothermal of CO_2 the fraction b/v cannot be looked upon as a small quantity of the first order. In fact, without raising the question, either of Van der Waals's mode of interpreting the term $\frac{1}{2}\Sigma(mV^2)$ or of the paucity of constants in his equation, the above consideration would of itself render the results untrustworthy. Van der Waals has most opportunely and effectively called attention to an exceedingly promising mode of attacking a very difficult problem, and his methods are both ingenious and suggestive; but I do not think that his results can be regarded, even from the most favourable point of view, as more than "*Guesses at Truth.*"

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[1] We have to thank the *Electrician* and the *Electrical Review* for some of the illustrations used in this article.

For, if we take the experimental test, there can be no doubt that (as I have stated in § 65 of my paper) "Van der Waals's curves cannot be made to coincide with those of Andrews." And I think I have given reasons for believing that "the term of Van der Waals's equation, which he took to represent Laplace's K, is not the statical pressure due to molecular forces but (approximately) its excess over the repulsion due to the speed of the particles." Of course I mean by this that, when Van der Waals, comparing his equation with experiment, assigns a numerical value to his term a/v^2, he is not justified in regarding it as the value of Laplace's K; though that quantity was, he tells us, the main object of his inquiry.

Believe me yours very truly,

P. G. TAIT.

St. Andrews, September 28.

THE EXISTING SCHOOLS OF SCIENCE AND ART.

AT a meeting of influential science and art teachers held at the Charterhouse School of Science and Art, Goswell Road, on the 3rd instant, the position of existing schools, with regard to the fierce opposition offered by highly endowed Polytechnics, was calmly and broadly discussed.

For many years, under the system not only recognized but encouraged by the Science and Art Department, schools have been established in London and the provinces. The aid afforded by the Department has mainly been (1) to contribute largely to the building fund of schools intended for the exclusive teaching of science and art subjects, and (2) to remunerate by Government grant the services of the teachers engaged. The regulations of the Department provide that such aid is given to any centre where the need of it is apparent. It is, however, perfectly well known that the teacher, in the majority of cases, was the person upon whom the duty fell to organize the classes and set the ball rolling, and it would be difficult to mention any school or institute in which the motive spirit was not a teacher.

By recent Acts of Parliament a great impetus has been given to that side of science and art instruction known as technical education. Funds which in past times could only have been raised by persistent begging are now forthcoming almost as a matter of routine. In the provinces there is every sign that the authorities having the administration of the grant of public moneys intend to recognize existing schools. In London it is not so Schemes for the erection of new buildings are pushed forward without due regard to those institutions already doing a good work. At the meeting of teachers already referred to several instances were cited. The People's Palace, erected almost in the very shadow of the Bow and Bromley Institute, has, by reason of its endowment, greatly hampered and harassed the older institution. The West London School of Art succumbed two years ago to the attack of the Regent Street Polytechnic; and now the St. Martin's School of Art, one of the best known centres of instruction in the metropolis, has closed its doors. Without endowment it could not compete with its more favoured rivals. The closing of this school is the more to be regretted because of the high tone of the work carried on within its walls.

Unfortunately, it cannot be denied that many so-called schools of science and art are simply carried on as "grant-earning" establishments, and the country would lose little or nothing if they were closed at once. But there are others affording excellent science and art instruction; and though these may not be affected by the present Polytechnics, it is evident that the schemes yet in embryo for the erection of other buildings will, if not properly checked, raise an undignified competition with the older schools. It is therefore a matter of great public importance that the established institutions should not be overlooked by the London County Council. If new buildings are deemed to be necessary, the old school of science and art should be treated as the nucleus of the enlarged scheme.

Two points of error seem to be apparent in the plan of campaign of the supporters of Polytechnics—(1) that educational work must be associated with recreation; and (2) that technical education has a very limited area, and that science and art education in its fullest sense is unnecessary.

"Schools of art," said a gentleman to me recently, "are dead." Surely nothing could be more absurd. As I understand technical education, it is the application of general principles to a specific purpose. Schools of science and art—*i.e.* schools for the study of science as science, and art as art—should be encouraged as much as before. This can be done without interfering with the specific application of such study to a particular purpose.

With regard to the question of recreation, I think it would be found that, although those institutes which make much of athleticism and such matters attract the largest proportion of students, the attendance *pro ratâ* in the classrooms, and the results obtained there, would not favourably compare with an institute carrying out a purely educational programme. At the meeting referred to, one teacher stated that, although at a Polytechnic with which he had been connected only seven students entered the class, scores of young men could be found in the billiard-room and gymnasium. At the Science and Art Institute, Wolverton, one of the best and most practical schools in the country, it was decided to close the billiard-room in consequence of the serious effect it had upon the attendance of students at the classes. I am personally acquainted with the science and art work carried on at the Regent Street Polytechnic. Excellent as it is, it would be still better if it could be relieved of the recreative element.

The London County Council has shelved for a time the appropriation of the funds provided by the Excise Act, 1890, for the promotion of technical education. But the matter must soon come up again. Healthy competition is excellent, but in this matter it is clearly not to the interest of the public that its money should be used for pushing on a new venture as a competitor to, and in antagonism with, an existing institution. The best butcher's shop in London would stand a poor chance if a rival establishment run with money raised by taxation, and not of necessity expected to pay its way, opened its doors on the opposite side of the road; and this is practically the state of affairs. The teachers, moreover, have a perfect right to be heard on this question. Devoting their best years to the training necessary for science and art teaching, it may be urged that they have a moral, if not a legal, claim to be considered.

In concluding, I would point out that the exponents of technical instruction are too keen on "centralization."

Let us have large buildings with costly apparatus and every convenience, but do not entirely crush the small schools. To the working man with limited time and means, weary with his day's toil, a modest school close at hand is of greater service than a huge building six miles away involving railway fare and loss of time. By careful arrangements such smaller schools can be preserved, and largely used as "feeders" for the institutes of magnitude.

The whole matter, therefore, of science and art schools and future Polytechnics should be referred to duly qualified men. There is no reason why existing machinery should not fit in with the new plant to make an harmonious whole.

OLIVER S. DAWSON.

NOTES.

THE autumn meeting of the Iron and Steel Institute was opened at the Royal Arsenal, Woolwich, on Tuesday, the greater part of the day being devoted to an examination of the various departments of the Arsenal. On Wednesday papers were discussed, and to-day visits are to be made to the Naval Exhibition, the Enfield Small Arms Factory, and the Thames Iron Works. We hope to print next week an account of the proceedings.

AN exhibition of cone-bearing trees and shrubs, asters, and sunflowers, and a conference upon them, were opened in the Royal Horticultural Society's Gardens, Chiswick, on Tuesday. Large numbers of conifers were sent from various parts of the country, no fewer than 30 collections coming from Scotland. The first prize was awarded to the Dowager Marchioness of Huntly for her collection of conifers, the second to Lord Wimborne. The largest araucarian cones were sent from Lady Fortescue's, at Dropmore, Maidenhead, where there is an araucaria 68 feet high—the tallest male araucaria in this country. Kew Gardens contributed about 200 different conifers. On Tuesday papers were read on asters and sunflowers. The conference on conifers began on Wednesday, and is being continued to-day.

A COMMISSION of engineers representing the various European Powers is to meet shortly at Cairo to consider the question of a storage reservoir, and to advise the Egyptian Government on the subject. The Commission will be required to select a site to the north of Wady Halfa, or within the present limits of Egypt.

THE organizers of the International Folk Lore Congress are to be congratulated on the success of their undertaking. The attendance was good; many excellent papers were read; and there were animated and suggestive discussions on most of the problems which are now of especial interest to students of folk-lore. Mr. Andrew Lang, as President, delivered the opening address, in which he presented a most interesting statement of what he conceives to be the fundamental principles of the science. Admirable addresses were also delivered by Mr. Sidney Hartland, Prof. Rhys, and Sir Frederick Pollock, who presided respectively over the Sections devoted to folk-tales, mythology, and institutions and customs. The members of the Congress dined together at the Criterion Restaurant on Tuesday evening.

STUDENTS of psychology and philosophy will read with regret Prof. Croom Robertson's "valedictory" words in *Mind*, from the editorship of which the state of his health makes it necessary for him to retire. For sixteen years he has done his work as editor with conspicuous ability and success. A second series of the Review will be begun next quarter. It will be under a co-operative direction which promises, Prof. Croom Robertson thinks, "a far more effective covering of the ground of psychology and philosophy than has hitherto been attained."

THE seventh of the series of One Man Photographic Exhibitions is now being held at the Camera Club. It is open to visitors from 10 a.m. to 4 p.m. on presentation of cards, which can be obtained from members or from the Hon. Secretary. The exhibition consists of photographs by Mr. Ralph W. Robinson.

WE learn from the *Botanical Gazette* that Mr. O. F. Cook, Instructor in Biology at the University of Syracuse, U.S.A., intends starting about November 1 in charge of an expedition to Liberia and other parts of Africa, with the object of studying the natural history of the country, especially the plants and insects. Mr. Cook will be glad to hear from anyone who would like to have material from that region.

YESTERDAY evening a meeting of the Medical Society, University College, London, was held in the Botanical Theatre, University College. Dr. W. H. Gaskell, F.R.S., delivered an address on a new theory of the origin of Vertebrates, deduced from the study of vertebrate anatomy and physiology.

THE Belgian Minister of Public Instruction offers a prize of 25,000 francs for the best memoir on the meteorological, hydrological, and geological conditions of the countries of equatoria Africa, regarded from the sanitary point of view. The subject must be studied with special reference to the welfare of Europeans resident in the Congo State.

IN the Proceedings of the Academy of Natural Sciences of Philadelphia for 1891, some parts of which have just reached us, there is an excellent memoir of the late Dr. Joseph Leidy, by Dr. Henry C. Chapman. It is followed by a list of Dr. Leidy's numerous writings.

IN a valuable paper on the "Rapakiwi," J. J. Sederholm, o the Geological Survey of Finland, has furnished petrographers with a trustworthy description of the mode of occurrence and minute structure of a granitic rock which has excited much interest, but has hitherto been very imperfectly understood. The official maps of the district where the Rapakiwi is found, with the accompanying memoirs, were published about a year ago; and the last number of Tschermak's *Mineralogischen und Petrographischen Mittheilungen*, now edited by Dr. F. Becke contains a full discussion of the petrological peculiarities of the rock. Writing from the famous laboratory of Heidelberg, Herr Sederholm naturally adopts the nomenclature of Prof. Rosenbusch, and it would appear from his description that the Rapakiwi will have to take its place among the numerous types of "granophyre" (using this term as Rosenbusch does, and not as originally defined by Vogelsang) which constitute links between the plutonic granites and the volcanic rhyolites. The excellent photographic illustrations accompanying the memoir give an admirable idea of the peculiar nodular structure of the rock, which has attracted so much attention to it. In the same journal, we find a second memoir by Herr Sederholm, on the Archæan rocks of South-West Finland, describing a varied series of igneous rocks, and discussing the effect of dynamo-metamorphic action upon them. The general conclusions of the author agree with those to which the study of similar rocks in other districts has led Lossen, Roland, Irving, Lehmann, Williams, Reusch, and Teall.

EXCELLENT arrangements have been made for the establishment of a good system of technical instruction in Essex. An organizing joint committee of the County Council and the Essex Field Club was lately appointed to deal with the question, and funds were placed at its disposal. This body has now issued a preliminary schedule of subjects to be taught. Local technical instruction committees are invited to select from the list one or more subjects which they may deem specially suitable for their respective neighbourhoods. When several such bodies, representing adjacent districts, have chosen a particular subject, the organizing committee will select a teacher or lecturer, and endeavour to arrange a circuit for him comprising the centres needing his services, apparatus and illustrations being provided by means of the fund for that purpose. By this means the aid of thoroughly qualified and equipped instructors may be obtained by the local committees at a cost considerably less than would be incurred if each centre were to act independently.

STRENUOUS efforts are being made in Scotland to secure that the country shall be supplied with a sound and adequate system of technical instruction. An important public meeting will be held at Edinburgh, on Thursday, October 29, for the consideration of the subject. Lord Elgin will preside, and it is expected that several members of both Houses of Parliament, and others interested in the question, will take part in the proceedings. The following are the provisional agenda:—(1) Chairman's address; (2) report on action taken up to this time by Town and County Councils—(*a*) in England, (*b*) in Scotland—with reference to the application of the sums available for technical education under the Local Taxation (Customs and Excise) Act, 1890; (3) the relation of the Local Taxation Act to technical (including commercial and agricultural) education; (4) report on various agencies already available for technical instruction in Scotland—(*a*) in rural districts, (*b*) in towns; (5) the amendments necessary in the Technical Schools (Scotland) Act, 1887.

THE Nicholson Institute, Leek, of which Sir Philip Magnus is President, has issued its Calendar for the session 1891–92; and an admirable Calendar it is, presenting many varied elements of interest. In the technical school connected with the Institute there will be classes for the study of wood-carving, modelling, bleaching, hygiene, and other subjects; and in the "science department" instruction will be given in botany, physiology, physiography, machine construction and drawing, and practical plane and solid geometry.

AN Agricultural and Mechanical College is about to be established at São Paulo, in Brazil, an endowment of 200,000 dollars having already been promised, and the further aid of the Government secured. The Presidency of the College has been offered to Prof. L. H. Bailey, the American botanist.

IN the Report for 1891 of the Governors of the Baltimore Fishing School, an interesting sketch of the history of the institution is given. The progress of the school encourages the Governors to believe that its success will prove of great advantage to Irish fisheries. They point out, however, that its operations are not on the enlarged scale originally contemplated; and to all who can appreciate the importance of the youth of the Irish coasts being trained in remunerative industrial pursuits, the Governors appeal for contributions to enable them to extend their work. The boys are thoroughly instructed in everything that pertains to the labours of fishermen. They also receive the literary education usual in such establishments; and a special class has been formed for the teaching of elementary navigation in connection with the Science and Art Department. At the last examination in this subject twenty-four pupils presented themselves. Of these, not one failed, twenty-two passing in the first division, and two in the second.

WE have received from the Meteorological Council a copy of the "Meteorological Observations at Stations of the Second Order" for the year 1887, containing observations and results for 66 stations. At 21 stations the observations taken at 9h. a.m. and 9h. p.m. are printed *in extenso*, and the whole work is on the same plan as in the volume for 1886 (NATURE, vol. xliii. p. 20), viz. the barometer observations are given without reduction to sea-level, and the differences between the dry and wet bulb thermometer readings are given as the "depression of wet-bulb." The maximum and minimum thermometers are read at 9h. p.m., and the readings entered to the day on which they were read. The rainfall is measured at 9h. a.m., and the amount registered entered to the previous day. Fog is only entered when the observer is quite enveloped in it. This work has been continued in a more or less complete form since 1866 (when, however, there was only one station); and the summaries contain, *inter alia*, very useful *résumés* of the state of the weather and wind-distribution, and afford excellent materials for preparing a revised climatology of the British Isles. The work is accompanied by a key map, showing the distribution of the stations, and indicating those which belong to the Royal and Scottish Meteorological Societies: it will be seen that all districts are well represented except, perhaps, on the more exposed western coasts and islands. A special table is also given, showing the number of hours of bright sunshine in each month for those stations at which sunshine-recorders exist.

THE Chief Signal Officer of the U.S. Army has, just before the transfer of the Meteorological Service to the Agricultural Department, issued three atlases, bearing upon the meteorology of the United States, showing—(1) The isobars, isotherms, and winds for each month from January to December for the years 1871–73, a period prior to the regular publication of the monthly charts. The data used include all the materials possessed by the Smithsonian Institution. (2) The probability of rainy days, prepared from observations for 18 years (1871–88). The average number of such days for all months and for each station has been calculated, and the percentages thus obtained are graphically shown on the charts. The data show great differences of distribution of rainfall in localities not far distant from each other; the influence of the prevailing direction of the wind in increasing the number of rainy days is particularly noticeable in the Lake region. (3) The average monthly cloudiness for the period 1871–88. Cloud observations show indirectly the relative amount of sunshine, as it may be assumed, within reasonable limits, that the complement of cloudiness will be sunshine. The investigation of this element is useful in determining the suitableness of certain localities for health resorts, or for the ripening of crops, and the charts may be considered as standard cloud maps of the United States.

DR. KING, Director of the Botanical Survey of India, has issued a Report on the working of the Botanical Survey in Assam and Burmah, for which 2000 rupees are annually allowed, with a view to arranging a plan for working by native collectors. Dr. King visited Assam in the latter half of last year, and found the local authorities ready to afford every assistance. Two native collectors were secured, and set to work near Golaghat, and in the Khasia Hills. The Conservator of Forests also sent a large number of specimens to the Herbarium at Calcutta, and a Eurasian collector was employed for a time in Cachar. Some interesting plants were also obtained from the base of the Eastern Himalayas. Fairly good work was done in Upper Burmah by a native collector, and his specimens are now in course of being arranged at the Calcutta Herbarium. The collecting agencies continue working during the present year.

DR. PRAIN, the Curator of the Herbarium of the Calcutta Botanical Gardens, accompanied the surveying ship *Investigator* during part of her operations in the Bay of Bengal last year. By a special arrangement, Dr. Prain was put down on the Great Coco Island for a few days, and was also enabled to pay short visits to the Little Coco and to Rutland Islands. Except for the visit made by Dr. Prain under similar circumstances the previous year, the Great Coco had not before been explored by a botanist, and the Little Coco and Rutland Islands were this year visited for the first time. Accounts of these visits are to be officially published in due course.

A NUMBER of small expeditions in the Chin Hills and on the Bhamo frontier of Upper Burmah have been arranged for next cold season. In the Chin country, a column will explore the Chinboh country, and four other columns will visit the Baunghshe, Tashon, Tlangton, Kanhow, and Nwengal tribes. In order to effect a settlement of the Kachyen tribes, columns will be sent out from Bhamo, Mogoung, and Myitkynia. An expedition will also proceed to explore the amber-mines and the india-rubber tracts, and, if practicable, join hands with Assam.

To estimate the relative merits of different kinds of points for lightning conductors, Dr. Hess recently collected and examined nineteen heads of conductors that had been struck by lightning (*Electrot. Zeits.*). His conclusions are as follows: (1) the fusion of points of lightning conductors by lightning causes no danger of fire through scattering of fused drops, for this does not occur; (2) fine and smooth points receive the lightning stroke in concentrated form, while sharply angled and ribbed, also blunt points, divide it into threads; (3) platinum needles and tips have no advantage over copper points; (4) there are lightning strokes which are capable of making brass wire 7·2 mm. (say 0·29 inch) thick, incandescent. Unbranched copper conductors should therefore never be thinner than 7·0 mm.

IN submitting to the Wellington Philosophical Society some "Coccid Notes" lately, Mr. W. M. Maskell expressed regret that entomologists generally did not devote more attention to the Coccidæ. He believed he was the only person in New Zealand who had published anything on the subject. In the Coccidæ there was infinite variety—a variety of life-history, habits, and customs that seemed greater than that afforded by any other branch of entomology. He gave instances of peculiarities in these insects—wonderful vitality in some cases, and the boring habits of one particular insect after it had thrown off legs, mouth, &c.—all tending to prove that these little despised creatures were more interesting for study than "all the butterflies"

FARMERS in many parts of Victoria seem to be fully alive to the necessity of adapting their methods to the conditions under which they have to carry on their work. Mr. David A. Crichton, in a report printed in the latest Bulletin (No. 12) of the Victoria Department of Agriculture, says that, although farmers are supposed to be too conservative in their practice to do much in the way of new industries, he has been agreeably surprised to find that a very large number are anxious to try crops other than cereals. Fruit culture in particular is attracting great attention, and he feels confident that before long it will become one of the staple agricultural industries of the colony. He is doing his best to stimulate this particular industry, and, in addition to the information afforded by his lectures, he makes it a practice to visit as many places as possible, to advise upon the selection of sites for orchards and vineyards, and give practical lessons in pruning, training, and other matters. He finds that this assistance is highly appreciated, and his services are in great demand in this respect Mr. Crichton's position in connection with the Victoria Department of Agriculture is that of "the fruit and special industries' expert."

MR. JOHN H. COOKE is publishing in the *Mediterranean Naturalist* an interesting series of observations on the geology of the Maltese islands. In the September number he refers to Cala Hein, a little bay between Comino and Cominotto. On a bright day, he says, this bay presents an endless succession of the most brilliant colours, "which commences with a deep blue, and from thence passes through every conceivable gradation of green, orange, and white, after attaining the last of which it again graduates onward in the distance to that cerulean blue that is so characteristic of Mediterranean waters." The setting of the picture is not less effective than the picture itself. Around the bay are many caverns, which have sombre-looking entrances and wildly-fantastic shapes. The sides of these caverns are full of interest for geologists, as "they literally teem with the remains of creatures that formerly lived and died in the waters in which the islands were built up"

MR. W. PRENTIS, of Rainham, Kent, describes in the October number of the *Zoologist* an interesting case of a wild duck's forethought. A mowing machine was set to work round the outside of a field of lucerne bordering a marsh, diminishing the circle each time round the field, leaving about two acres in the centre. A wild duck was seen by the shepherd to fly from the piece of lucerne that was left with something in her beak, and, happening to fly near him, she dropped a three parts incubated egg. She was again observed by the shepherd, and also by the sheep-shearer, carrying another egg in her beak, this time over the marsh-wall towards the saltings; and again she was seen for the third time carrying an egg in her beak in the same direction. Next day, when the field was "finished" by the removal of the last piece of lucerne, the wild duck's nest from which the eggs had been removed was discovered.

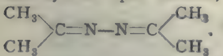
MR. W. H. HARRIS, Ealing, records in *Nature Notes* (September 15) a remarkable instance of "frugality" in bees. The recent extremely rainy weather seems to have suggested to his bees that there would probably soon be an end of honey-making. Accordingly, although there was "a crate of fairly filled sections above the stock-box," they adopted vigorous measures to prevent future inconvenience. "It is a positive fact," says Mr. Harris, "that my bees, not content with ejecting larvæ of both drones and workers, proceeded to suck out the soft contents of the corpses, leaving only the white chitinous covering, which had not hardened sufficiently to prevent the workers from piercing it with their mandibles, and then inserting their tongues."

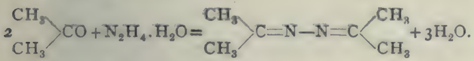
MESSRS. R. FRIEDLÄNDER AND SON, Berlin, send us the latest of their catalogues of botanical books. This list, besides various works on the distribution of plants and on botanical exploration, includes a great number of writings on the floræ of different parts of the world.

MESSRS. KEGAN PAUL, TRENCH, TRÜBNER, AND CO. announce the following books on scientific subjects:—"Colour Blindness and Colour Perception," by F. W. Edridge-Green, M.D., with three coloured plates (International Scientific Series); "Descriptive Catalogue of the Nests and Eggs of Birds found breeding in Australia and Tasmania," by A. J. North, with 21 full-page plates; "English Folk Rhymes," by G. F. Northall; the following volumes of a series, "Modern Science," to be edited by Sir John Lubbock—"The Cause of an Ice Age," by Sir Robert Ball, F.R.S., "The Horse: a Study in Natural History," by William Henry Flower, C.B., "The Oak: a Popular Introduction to Forest Botany," by H. Marshall Ward, F.R.S., "The Laws and Properties of Matter," by R. T. Glazebrook, F.R.S.;—"On Seedlings," by Sir John Lubbock, with numerous figures in text; "How to Use the Ophthalmoscope," elementary instruction in ophthalmoscopy, by Edgar A. Browne, fourth edition, completely revised; "Principles of Political Economy," by Arthur Latham Perry; "Moral Order and Progress," an analysis of ethical conceptions, by S. Alexander, second edition (Trübner's Philosophical Library); "Chemistry of the Carbon Compounds, or Organic Chemistry," by Prof. Victor von Richter, authorized translation by Edgar F. Smith, new and enlarged edition.

TWO more papers by Prof. Curtius, upon the reactions of the hydrate of his recently isolated hydrazine or diamidogen, $\begin{matrix}NH_2\\|\\NH_2\end{matrix}$, are contributed to the most recent numbers of the *Journal für praktische Chemie*. The earlier communication describes, for the first time, the neutral sulphate of hydrazine

$(N_2H_4)_2 \cdot H_2SO_4$. Hydrazine is found to form two sulphates—an acid one, $N_2H_4 \cdot H_2SO_4$, and the neutral one now described. The acid sulphate is a beautifully crystalline salt—an account of which was given in Nature, vol. xliii. p. 205. It is distinguished by its high melting-point, 254° C., and its difficult solubility. The neutral sulphate now described is obtained by evaporation of the solution formed by neutralizing hydrazine hydrate with dilute sulphuric acid, first, over a water-bath, and finally, as the new salt is very deliquescent, *in vacuo*. It crystallizes in large brilliant tables, melting at 85°. It is precipitated in a most curious manner from its aqueous solution by alcohol, separating as an oil, which, on being stirred with a glass rod, and in contact with a small crystal of the salt, immediately solidifies to a fine mass of crystals, which, like those obtained by evaporation, consist of anhydrous $(N_2H_4)_2 \cdot H_2SO_4$.

The second and much longer communication describes an important series of new compounds, the ketazines, obtained by the action of hydrazine hydrate upon ketones. The simplest of these new substances, the one obtained by the action of hydrazine hydrate upon acetone, is represented by the formula  $\begin{matrix}CH_3\\CH_3\end{matrix}\!>\!C{=}N{-}N{=}C\!<\!\begin{matrix}CH_3\\CH_3\end{matrix}$. When hydrazine hydrate is dropped upon acetone, a most violent reaction occurs, resulting in an explosion unless the acetone is surrounded by a freezing mixture. When thus moderated, however, the substance above formulated is produced together with water, the reaction occurring according to the following equation:—



$$2\begin{matrix}CH_3\\CH_3\end{matrix}\!>\!CO + N_2H_4 . H_2O = \begin{matrix}CH_3\\CH_3\end{matrix}\!>\!C{=}N{-}N{=}C\!<\!\begin{matrix}CH_3\\CH_3\end{matrix} + 3H_2O.$$

By allowing the product to remain for some hours in contact with caustic potash the water is removed, and upon distillation the new ketazine passes over in the pure state. It is a clear liquid possessing a sharp odour somewhat resembling that of the alkaloid coniine. It boils without decomposition at 131°. By employing other ketones, such as methyl ethyl ketone, diethyl ketone, and others of the same type, a large number of these ketazines have been prepared. Those containing fatty radicles are liquids, and those containing aromatic groups are solids. The lowest members only dissolve in water, the solubility rapidly diminishing with increase of carbon atoms. Acids decompose them in the cold, with assimilation of water, into their constituents; towards alkalies, however, they are comparatively stable. Light exerts a decomposing action upon them, specimens placed in bright sunshine rapidly becoming yellow. Reducing agents, such as sodium amalgam, are without action upon them, and they appear further to be incapable of reducing either Fehling's solution or (except after long boiling) ammoniacal solutions of silver salts.

The additions to the Zoological Society's Gardens during the past fortnight include two —— Cormorants (*Phalacrocorax*, sp. inc.) from New Zealand, presented by the Earl of Onslow, G.C.M.G.; a Velvet Monkey (*Cercopithecus lalandii* ♀) from South Africa, a White-fronted Lemur (*Lemur albifrons* ♀) from Madagascar, presented by Captain R. C. Stevenson; a Golden Agouti (*Dasyprocta agouti*), a Garden's Night Heron (*Nycticorax gardeni*), a —— Heron (*Ardea*, sp. inc.) from Surinam, presented by Mr. Frank Fisher; a Common Paradoxure (*Paradoxurus typus*) from India, presented by Miss Bason; two Blackcaps (*Sylvia atricapilla*), two Lesser Whitethroats (*Sylvia curruca*), two Goldfinches (*Carduelis elegans*), a Marsh Tit (*Parus palustris*), British, presented by Mr. J. Young, F.Z.S.; three Common Vipers (*Vipera berus*), British, presented by Messrs. A. H. R. and F. R. Wollaston; a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mrs. Gwynne; an Indian Civet (*Viverricula malaccensis*) from India, presented by Mr. Herbert Courtney Hodson; two Chilian Sea Eagles (*Geranoaëtus melanoleucus*) from Chili, presented by Mr. H. Berkeley James, F.Z.S.; two Grey-breasted Parrakeets (*Bolborhynchus monachus*) from Monte Video, presented by Mr. J. C. Wallace; two Nightingales (*Daulias luscinia*), two Common Whitethroats (*Sylvia cinerea*), a Blackcap (*Sylvia atricapilla*), British, presented by Mr. J. Young, F.Z.S.; four Yellow Wagtails (*Motacilla raii*), British, presented by Mr. W. Swaysland; a Common Cormorant (*Phalacrocorax carbo*) from Scotland, presented by Mr. F. T. Barry, M.P.; fifteen Striped Snakes (*Tropidonotus sirtalis*) from North America, presented by Mr. J. Gray; a Solitary Thrush (*Monticola cyanus*), European, a Macaque Monkey (*Macacus cynomolgus*) from India, deposited; a Sharpe's Wood Owl (*Syrnium nuchale*) from West Africa, a Testaceous Snake (*Ptyas testacea*) from California, two Quebec Marmots (*Arctomys monax*) from North America, two Scaly Doves (*Scardafella squamosa*) from South America, purchased; a Ruddy-headed Goose (*Bernicla rubidiceps*) from Falkland, received in exchange.

OUR ASTRONOMICAL COLUMN.

Physical Appearance of Periodic Comets.—Comets possess no personal characteristic appearance; but Mr. Barnard, writing to the *Astronomical Journal*, No. 246, suggests that it may be possible to arrange those of short period according to their physical peculiarities. To the first class he would assign those comets which are large, round, and very gradually brighter in the middle, with no special condensation, and of a very diffused nature. They have no nucleus or tail, and are so decidedly periodic that, trusting to this peculiarity, Mr. Barnard predicted that the comet discovered by Swift in November 1889, and D'Arrest's comet at its return last year, were of short period. The most distinctive members of this class of comets are D'Arrest's, Swift's 1880, Brooks's 1886, and Swift's 1889. There are few nebulæ that resemble this class. A much larger and less exclusive class contain comets which are comparatively small, and which have an indefinite central brightness or nucleus. Many of the parabolic comets resemble these, and there are hundreds of nebulæ exactly like them in telescopic appearance. To this class are assigned comets Faye; Wolf, 1884 III.; Finlay, 1886 VII.; Brooks, 1889 V.; Spitaler, 1890. It is possible that the peculiarities of these two distinct classes of short-period comets may furnish some information as to their relative ages.

Discovery of Tempel-Swift's Comet.—Mr. Barnard found this comet on September 28, and Mr. W. F. Denning discovered it independently two days later about 4° south-west of its computed position. The comet passes perihelion in November. Its position, according to M. Bossert's ephemeris, is as follows:—

Ephemeris for Paris Midnight.

1891.	Right Ascension.			Declination.		Brightness.
	h.	m.	s.	°	′	
Oct. 6	21	6	2	+ 3	24·8	7·01
„ 8		6	19	3	54·0	
„ 10		6	55	4	24·7	7·77
„ 12		7	51	4	56·9	
„ 14		9	9	5	30·8	8·61
„ 16		10	48	6	6·4	
„ 18		12	49	6	43·9	9·54
„ 20		15	13	7	23·4	
„ 22		18	0	8	4·8	10·54
„ 24		21	12	8	48·3	
„ 26		24	50	9	34·0	11·64
„ 28		28	56	10	22·2	
„ 30	21	33	30	11	12·8	12·83

The comet is therefore in Equuleus at the present time, and moving towards Pegasus.

PHOTOGRAPHIC DEFINITION.

I.

It is a matter of some interest to determine what are the limits to the definition obtainable in photographs. In examining this question, three distinct classes of problems present themselves—namely :—

(1) Those depending on the wave-length of light, and the action of a perfect lens on such wave-lengths.

(2) The various aberrations of real lenses.

(3) The qualities of the different sensitive surfaces on which the pictures are formed.

Taking these divisions of the subject in the order given, I will inquire first what is the limit to photographic definition on the supposition that the lens has no aberration of any kind, *i.e.* that all the waves which reach it from any point arrive at the image of that point in the same phase.

The image thus formed consists, as is well known, of a bright disk surrounded by alternate dark and bright rings, the intensity of the illumination of the rings decreasing rapidly at each successive ring, reckoning outwards from the centre.

In order that the images of two neighbouring points may appear separated from one another, the central disks of their images ought not to overlap. If the disks are just in contact, it is possible that they would appear as a double object in the photograph, and this may be taken as the limit of the defining power of a lens. (See Airy "On Light," and Lord Rayleigh "On the Theory and Manufacture of Diffraction Gratings," *Phil. Mag.*, 1874.)

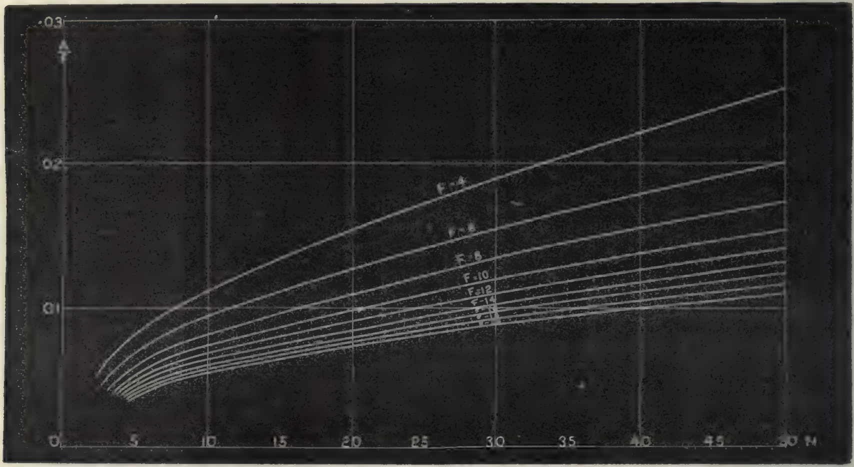


Fig. 1.

But, in ordinary photography, objects at very various distances have to be simultaneously represented, and it is to the definition attainable under these circumstances that I wish now to direct attention.

On referring to the papers above-mentioned, it will be seen that the diameter of the central disk is

$$1{\cdot}219\,\frac{\lambda F}{A},$$

where λ is the wave-length of light,
F the focal length of the lens,
A the aperture of the lens.

This gives the effective diameter of the image of a point truly in focus when not far removed from the axis of the lens.

Points nearer to, or further from, the lens than that which has its image on the plate will be represented on the latter by round patches of light ; these being the sections by the plate of the cones of rays which have for their summits the geometrical foci of the points, and for their slant the radius of the aperture ÷ focal length.[1] Thus, if e is the distance before or behind the plate of the focus of a point, it will be represented on the plate by a patch of light of diameter

$$e\,\frac{A}{F}.$$

This diameter can be diminished by the use of a diaphragm, *i.e.* by diminishing A, but this at the same time increases the diameter of the images of points whose foci are on the plate. And the resulting average definition will be improved by diminishing A until the patch of light, representing the point most out of focus, has the same diameter as the diffraction disk of the image point in focus.

If we suppose the photographic plate to be placed at such a distance from the lens that the focus of the nearest object is as much behind the plate as the focus of very distant objects is in front of it, we shall have, to determine the diameter of the stop giving the best average definition, the following equation :—

Putting F = principal focal length,
D = distance of nearest object,
$q = 1{\cdot}219\lambda$,

$$q\,\frac{F+e}{A} = e\,\frac{A}{F+e}\,;$$

$$\therefore\quad q\,\frac{(F+e)^2}{e} = A^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

but, by the ordinary formulæ, connecting the conjugate foci of lenses, we have, if $D = F + g$,

$$2eg = F^2\,;$$

$$\therefore\quad e = \frac{F^2}{2g} = \frac{F^2}{2(D-F)}\,;$$

---

[1] This is an approximate statement only. The true expression involves an investigation of the intensity of the light immediately in front of and behind a caustic.

whence, substituting for e in (1) we have

$$A = \sqrt{\frac{q}{2}}\,\frac{2D - F}{\sqrt{D - F}}\ .\ .\ .\ .\ .\ .\ .\ (2)$$

Let the nearest object be at n times the focal length of the lens. Then, putting nF for D,

$$A = \sqrt{\frac{qF}{2}}\,\frac{2n - 1}{\sqrt{n - 1}}\ .\ .\ .\ .\ .\ .\ (3)$$

This gives the value of A as a linear quantity; it is usual, however, to reckon the diameter of stops as fractions of the focal length.

Dividing, therefore, (3) by F,

$$\frac{A}{F} = \sqrt{\frac{q}{2F}}\,\frac{2n - 1}{\sqrt{n - 1}}\ .\ .\ .\ .\ .\ .\ (4)$$

From (4) the accompanying table has been computed, giving $\frac{A}{F}$ for various values of F and n. (Fig. 1 gives the same graphically.)

Table showing ratio of aperture to focal length which gives the best average definition when the nearest object to be photographed is at "n" times the focal length of the lens, and distant objects are a'so in view.

F	$\frac{A}{F}$ n=5	n=10	n=15	n=20	n=25	n=30	n=35	n=40	n=45	n=50
in.										
4	·00785	·0110	·0130	·0150	·0171	·0188	·0201	·0216	·0229	·0244
6	·00640	·00900	·0106	·0124	·0139	·0152	·0165	·0177	·0188	·0199
8	·00554	·00775	·0092	·0107	0121	·0132	·0142	·0154	·0162	·0172
10	·00495	·00699	·00825	·0096	·0108	·0118	·0129	·0137	·0146	·0154
12	·00452	·00635	·00755	·0087	0098	·0108	·0117	·0125	·0133	·0141
14	·00420	·00580	·00695	0081	·0091	·0100	·0108	·0116	·0123	·0130
16	·00392	·00552	·00651	·0076	·0085	·0093	·0101	·0108	·0115	·0122
18	·00370	·00520	·00615	·0071	·0080	·0088	·0095	·0102	·0109	·0115
20	·00350	·00494	·00584	0068	0076	·0084	·0090	·0097	·0103	·0109

I have not before seen it pointed out that the ratio $\frac{A}{F}$, which gives the best average definition, alters with the value of F.

If α is the least angular distance between two points (as seen from the centre of the lens) which are shown as separate points on the photograph, α must at any rate not be less than $\frac{q}{A}$, or

$$\sqrt{\frac{2q}{F}}\,\frac{\sqrt{n - 1}}{2n - 1},$$

showing that, if the foreground is kept at a distance proportional to the focal length of the lens, the definition improves with an increase of the focal length.

On the other hand, if the nearest object is at some fixed distance, D, from the lens, we have as the limit for α,

$$\sqrt{2q}\,\frac{\sqrt{D - F}}{2D - F},$$

an expression which increases with F, so that for a given picture taken from a fixed position, definition will be gained by the use of a short focus.

The gain, however, in this respect is not great, for in practice D is always a considerable multiple of F, and writing

$$\sqrt{\frac{1}{4D + \frac{F^2}{D - F}}}\ \text{for}\ \frac{\sqrt{D - F}}{2D - F},$$

it will be seen that when D is many times F, $\frac{F^2}{D - F}$ may be neglected in comparison with 4D.

Thus, in ordinary cases the limit for α is $\sqrt{\frac{q}{2D}}$, and is independent of the focal length of the lens employed.

If we inquire how close the nearest object may be to the lens when a view containing also distant objects has to be photographed with a definition reaching a certain standard, we have, on the above supposition,

$$D = \frac{q}{2\alpha^2};$$

and if we put $\alpha = 1'$, which is often taken as the least angle separable by the unaided eye, and λ as $\frac{1}{50000}$ inch,

$$D = 150 \text{ inches},$$

showing that if the picture is to appear as well defined as the natural objects themselves, to the eye placed at the position of the lens, no object in the view must be nearer the latter than about 13 feet.[1]

Though, as above stated, the focal length does not affect the definition, when the right-sized stop is used, it does the rapidity with which a picture may be taken, for the intensity of the light on the plate is measured by $\frac{A^2}{F^2}$ or $\frac{q}{2F}\,\frac{(2n - 1)^2}{n - 1}$.

That is, in these circumstances, the exposure is inversely as the focal length.

All that has been hitherto said refers to the definition in the central parts of the plate.

The definition for the oblique pencils is necessarily worse. For even if it were assumed that the lens was perfect for oblique pencils, the points out of focus would be no longer represented by circular areas, but by the elliptic projections of these circles on the plane of the plate.

The assumption, however, that a lens is perfect for oblique pencils is too far removed from actual fact to make it worth while to consider the results to which such a supposition would lead.

The definition for the marginal parts of the photograph depends on the various aberrations which all combinations of lenses suffer from in some degree, but which in well-made examples are completely, or almost completely, corrected for direct pencils.

These aberrations are (1) spherical, (2) chromatic, (3) astigmatism, (4) curvature of field.

The effects of the two last are much the most important, and will be considered first.

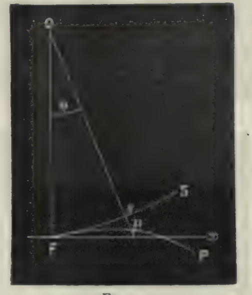


FIG. 2.

Let O (Fig. 2) be the optic centre of the lens, OF the axis of the lens, and F the principal focus.

Let Fx be the plane of the plate, FP and FS the curves on which the primary and secondary foci respectively lie.

Let Op be the axis of a pencil inclined to OF at an angle θ, and meeting FP and FS in p and s. Then sp measures the astigmatism of the lens for a pencil of obliquity θ.

Putting y_p and y_s for the ordinates of the curves FP and FS at p and s, it will be seen that a point distant θ from the axis of the lens, will be represented on the plane of the plate by an oval patch of light whose axes are $A\frac{y_p}{\cos\theta}$ and $A\frac{y_s}{\cos\theta}$ in directions parallel and perpendicular to Fx; A, as before, being the aperture of the lens.

Any formula depending on the actual data of real combinations of lenses, and giving the values of y_p and y_s in terms of radii of curvature and refractive indices, &c., of the lenses composing them, would be a very unmanageable thing for the purpose in hand; but I give the curves in question obtained experimentally for seven lenses of different types in my possession (see Figs. 3 to 9). All these lenses except Fig. 5 are by makers reputed to be the best.

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[1] I have verified this with a lens of 10-inch focus.

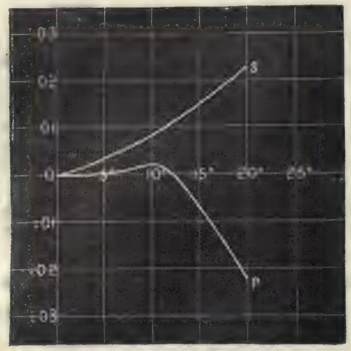


Fig. 3.—English Portrait.

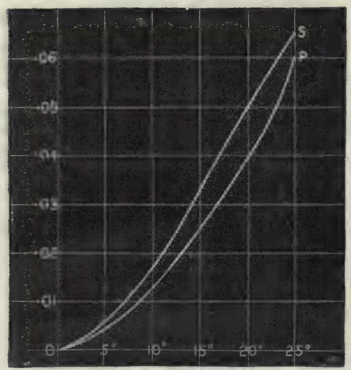


Fig. 4.—English Portrait.

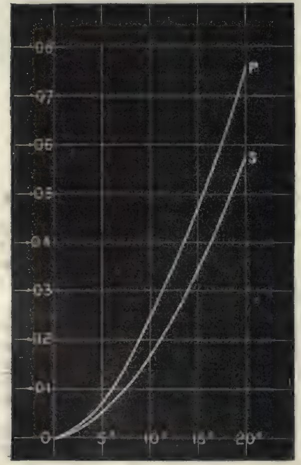


Fig. 5.—French Portrait.

[Fig. 5 is peculiar in having the usual relative positions of the primary and secondary foci reversed.]

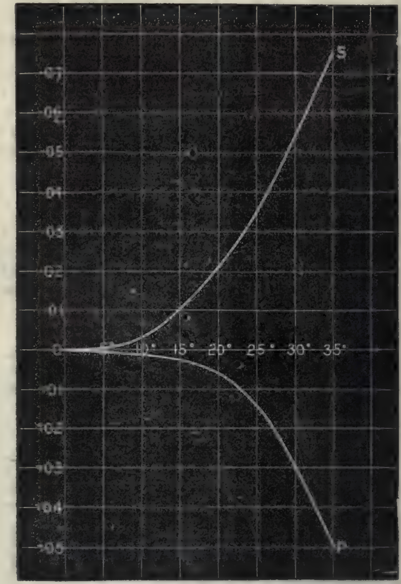


Fig. 6.—English Wide-angle Doublet.

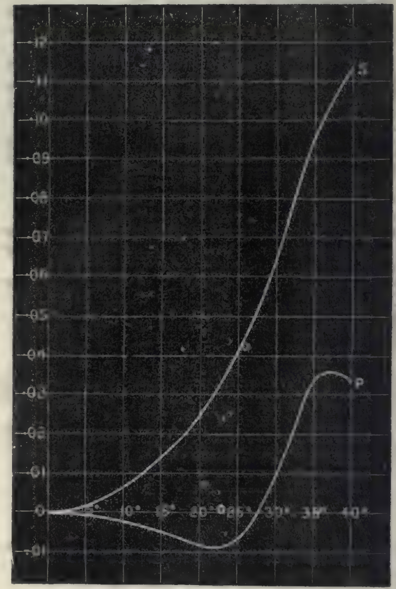


Fig. 7.—French Wide-angle Doublet.

The ordinates of these curves are the distances of the primary and secondary foci from a plane through the principal focus at right angles to the axis of the lens, and are expressed as fractions of the focal length.

The abscissæ are the inclinations (in degrees) of the pencils to the axis

Suppose, now, that the plate is placed at a distance e behind

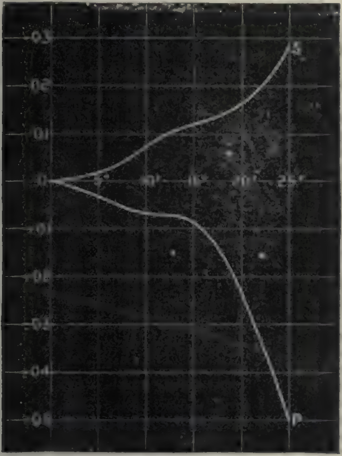


FIG. 8.—Rapid Rectilinear.

the principal focus, e being half the distance between the foci for direct pencils of the nearest and distant objects.

The worst defined point in the centre of the picture will then be represented as having a width $\frac{A}{F}e$ nearly, while at the obliquity θ this width becomes $\frac{A}{F}\frac{e \pm y}{\cos\theta}$ nearly, according to whether the

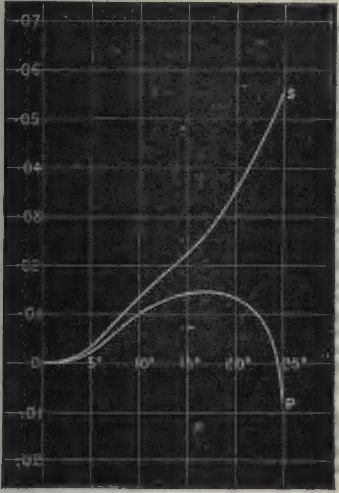


FIG. 9.—Triplet.

image under consideration is that of the most distant or the nearest point.

Hence, unless y is small compared with e, the definition for rays of obliquity θ will be sensibly worse than in the centre of the plate, and a reference to the curves for y_p and y_s shows at a glance that this must be the case even at 10° for all the lenses unless the general standard of the definition is lowered by a large value of e.

As an example of the use of the curves, let us take the rapid rectilinear No. 6, and compare the definition at 20° obliquity with that at the centre, supposing that the nearest object is at a distance of 25F.

This gives $e = {·}02$F nearly, and at 20° $y_p = -{·}023$F, $y_s = +{·}016$ F, hence we have as follows:—

		At 20° from axis		
		due to primary focus.	due to secondary focus.	At centre.
Width of image of				
nearest point...	$\frac{A}{F\cos\theta} \times$ ...	·043 ...	·004 ...	·02
Distant do.	...	·003 ...	·036 ...	·02

This shows that while the nearest points at this obliquity are represented by long ovals placed as if radiating from the axis, the most distant points become similar but rather smaller ovals with their long axes at right angles to the former, and that the length of the ovals is about twice the diameter of the image formed by the direct pencils.

In the same way the definition, as far as it depends on astigmatism and curvature of field, at any obliquity may be found for any lens for which y_p and y_s are known.

Lauriston Hall, September 9. A. MALLOCK.

(*To be continued.*)

THE KOH-I-NUR—A CRITICISM.

THE true history of the Koh-i-Nur diamond, if it could be written, would be a singularly interesting one. But the historian would have a difficult task. The pages that I purpose writing will be devoted to the criticism, possibly the refuting, of some fallacies that hang round the subject; but they will not deal with some other historical difficulties that I have not space even to indicate, but which do not belong to those portions of the history for criticism on which the following pages are designed.

The period in the history of the Koh-i-Nur that has attracted the notice of all modern writers on the diamond, and to a degree, I think, somewhat beyond its importance, is the five or ten minutes during which the French diamond-dealer, Tavernier, held in his hand the most important of the Crown jewels of the Emperor Aurungzebe. It was a great diamond, and the record Tavernier has handed down in his "Voyages," of its weight, its form, and its history, will have to be critically dealt with.

It may be at once stated that the disputable point regarding this diamond is whether it was a certain ancient diamond of fame in India, or one much larger than this ancient stone, that had been found not very long before Tavernier was present at the Court of Aurungzebe. For the larger stone I shall retain the name of "the Great Mogul"; for the older and more famous one the title of the Koh-i-Nur. Some hold that Tavernier saw and handled the Koh-i-Nur; others that his own story is correct, and that it was the Great Mogul that he described. And I should add that some, in addition to this latter view, believe the Great Mogul ought to be called the Koh-i-Nur.

In order to clear the ground, I may say that while attaching no very great importance to the question as to which of the two first views is the correct one—and I must add also, valuing at a somewhat low estimate the historical or technical accuracy of Tavernier's statements on this and many other matters—I, some thirty-five years ago, came to the conclusion that the diamond Tavernier saw was probably the Koh-i-Nur, and that he muddled its history with the other and larger diamond that I showed to have been probably at the time in the keeping of Shah Jahan, the captive father of Aurungzebe. The merits of the question will be discussed in their proper place; but while holding myself open to conviction if any new arguments can be brought forward against my view, I may state that none yet announced have shaken that opinion.

Until the fifteenth century there appears to have been one and only one very large diamond known in India or in the world. I might have said until the sixteenth century but that there is a record of two and an unauthenticated rumour of a third during that century, the largest of which, however, was very likely the Koh-i-Nur. But that one large diamond of the earlier time had been a famous stone for centuries. Legends had gathered round it, and tradition had linked the legends with authentic history in the dawn of the fourteenth century. The tale was told briefly by Prof. H. H. Wilson in the sketch of the Koh-i-Nur which he contributed to the official catalogue of the Exhibition of 1851. No more competent person could have performed the task than the great Orientalist and Sanscrit scholar, with his large experience of Hindoo customs and modes of thought. And he wrote the notice with the statements before him that had been collected in the bazaars of India by order of the Company at the time when the Koh-i-Nur became a Crown jewel of the Queen.

The latest historian of the Koh-i-Nur, however, dismisses this curious tradition and its distinguished narrator by the somewhat flippant remark that "it has afforded sundry imaginative writers a subject for highly characteristic paragraphs."

The gentleman who writes in this tone of the eminent custodian of the East India Company's Library cannot be expected to treat Mr. King or any other man of learning less contemptuously; but his qualifications for dealing with the subject at all from a wider point of view than that of the old French diamond-dealer will, perhaps, be fairly called in question by the readers of the following pages.

Yet Dr. Ball, of the Science and Art Department in Dublin, has had Indian experience on the Geological Survey, an office that ranks deservedly high even among the great departments of the Indian public service. He has, furthermore, recently thought the Indian part of Tavernier's "Voyages" worthy of a fresh translation, which he has effected with judgment and with notes, the topographic part of which, at least, appears to be of considerable value and interest; and he has otherwise been an author on subjects that came before him in India as a geologist and a sojourner.

It is probably a sort of loyalty to the author whom he has deemed worthy of so much of his time and industry that blinds him in his advocacy of Tavernier's statements, notwithstanding their manifold inconsistencies and absence of scholarlike quality. I hope, while criticizing his hypotheses and statements regarding the Koh-i-Nur, I may not in any respect quit a judicial attitude to appear in that of a partisan.

The great diamond to which allusion has been made emerges in history in the first years of the fourteenth century. It was in 1300 A.D. in the hands of the Rajahs of Malwa, an ancient Ráj that had at one time spread over Hindostan, and in all the vicissitudes of a thousand years had never bent to a Muhammadan conqueror, until the generals of the Delhi Emperor Alá-ud-dín Muhammad Shah overran its rich territory, and carried away the accumulated treasure of Ujjein in the first decad of the fourteenth century.

The date of 1304 is that given by Ferishtá for this conquest, and then it was that the great diamond takes its place in history. In 1526 the invasion of India by Babar was crowned by his victory on the famous battle-field of Panaput. Babar himself—in those memoirs that rank only after the "Commentaries" of Cæsar as the most interesting records penned by a great conqueror—describes the reception by his son Humayún of the great diamond among the treasures which he was sent forward to secure at the strong fortress at Agra. Babar gives the weight of the diamond as being computed at 8 mishkals, and in another place he compares the Muhammadan weights with those of the Hindoo system, putting the mishkal as equivalent to 40 of the little Hindoo units of weight, the rati. The diamond, then, weighed near about 320 of these ratis. There are several lines of investigation for determining the weight of the mishkal; and without here entering on a long but interesting discussion of this weight, it will suffice to say that the most important of them converge on a value of from 73 to 74 troy grains. If the mishkal weighed 73·636 troy grains, 8 such mishkals would be 589·088 grains. The weight of the Koh-i-Nur diamond in the Exhibition of 1851 was 589·52 troy grains. It may be added that this latter weight is equivalent to 186$\frac{1}{16}$ English carats of 3·1682 troy grains, and would require, to make up the 320 ratis, a rati of the value of 1·8425 troy grains.

It is very remarkable how numbers closely corresponding to one or other of these values for the weight of a great diamond, in carats or ratis, will recur in the subsequent discussion. Thus Anselm de Boot, in commenting in the early years of the seventeenth century upon some observations on Indian diamonds made in the previous century by Garcias de Orto (a Portuguese physician at the Viceregal Court of Goa), states the largest diamond Garcias had seen to have weighed 187½ carats. Garcias puts its weight at 140 mangelins. His translator (into Latin), Le Cluze, interprets the 140 mangelins as equivalent to 700 grains (apparently French grains of the old poids de marc). But De Boot evidently either had some separate authority for his statement that the largest diamond Garcias had seen weighed 187½ carats, or had the means of reckoning more correctly than Le Cluze the value in Dutch or in Portuguese carats of the 140 mangelins of Garcias. Garcias was in India for thirty years in the reign of Akbar, a reign that, commencing three years earlier and ending three years later, covered "the spacious times of great Elizabeth"; and if any European of the many visiting India at that time would have had special opportunity of seeing the great diamond in the treasury of Babar's grandson, it would have been the body-physician of the Portuguese Viceroy. Dr. Ball has got into a hopeless mess in an endeavour to discredit observations of mine, and of my late learned friend Mr. King, regarding this allusion of De Boot's to a diamond weighing 187½ carats. Dr. Ball is quite mistaken in supposing that he is the first person who had an acquaintance with De Boot's sources of information, with Le Cluze's translation of Garcias into excellent Latin, and with the commentators who edited De Boot and largely plagiarized from Le Cluze. In his "Natural History of Precious Stones," Mr. King gave, in 1866, an account of all these persons and their writings, but that accomplished scholar would certainly never have fallen into so absurd an error as Dr. Ball has rushed into in connection with De Boot's allusion to a 187½-carat diamond.

Garcias, like Le Cluze, was a botanist, and his treatise was on Indian botany. He, however, devoted a few pages to the precious stones in vogue in India, and one short chapter is given to the diamond. De Boot transcribed, with omissions, these chapters of Garcias, and with misprints that probably arose from the statements he made, and even the pages he incorporated, being in the form of notes culled by him from a great variety of sources, of which Garcias was only one. Among the misprints or misapprehensions in De Boot's very remarkable book on stones and gems, is that by which he always substitutes the name of Monardes, a writer on the botany of the New World, in lieu of that of Garcias, an error the source of which Mr. King explained in the treatise above alluded to. Upon the passage in which De Boot refers to the great diamond, and which runs thus: "Nunquam tamen majorem (adamantem) illo qui pendebat 187½ ceratia, cujus mentionem facit Monardes, inventum fuisse puto," Adrian Tull, a Belgian physician who edited the treatise of Anselm de Boot, adds a note to the chapter, correcting the name Monardes for that of Garcias, and then quoting from Le Cluze another note introduced at the end of his translation of the chapter, to the effect that he, Le Cluze, had never himself seen a larger diamond *in Belgium* than one which weighed 190 *grains*. Dr. Ball quotes this note in the Latin of Le Cluze to show that De Boot did not know what he was writing about, and still less that Mr. King and, of course, myself did, inasmuch as we had fastened upon De Boot's singular statement without due study of our authors. It is the writer of the "true history" of the Koh-i-Nur who has not gone to the authorities. Had he done so, he would have found in the 1605 edition three notes on this passage by Le Cluze. In the first he analyzes Garcias's 140 mangelins into "septingenta grana, sive unciam unam, drachmam unam, scriptula duo, grana quatuor. Nam mangelis, ut ante dixit noster auctor, quinque grana pendit, et septuaginta duobas granis dragma constat." His next note alludes to the diamonds he had seen himself in Belgium; and the third is upon certain crystals known as Bristol diamonds, found three miles from that city.

Passing from this curious aberration of Dr. Ball's, we may ask, What did De Boot mean by alluding in a second passage to the diamond Garcias had seen in India as weighing 187½ carats? As I have said, it is barely possible he had means external to Garcias's statement in his book of knowing the weight of this diamond. The weights summed together by Le Cluze were apothecary weights, varying somewhat in different localities in Western Europe from the corresponding divisions of the French ounce of 576 French grains, equivalent to 472·1875 troy grains. The weight of the diamond on the French system would be 573·776 grains troy according to Le Cluze's reckoning, In terms of the old Netherlands ounce of 474·75 grains, current in Antwerp, it would be 576·95 troy grains. But none of these are carat grains. De Boot, on the other hand, in estimating the 140 mangelins as 187½ carats, took the mangelin not at the 5 carat grains of Garcias, but at 5·3568 such grains, taking probably 1⅓ carat as the measure of the mangelin instead of 1¼ carat, the former being one among the several values which this variable unit had in different places.

The 187½ carats of De Boot would, on the value of the Amsterdam carat, 7½ of which equal an engle, which was the sixteenth part of the Dutch troy mark, give a weight for the diamond in question of 593·437 troy grains: the weight of the Koh-i-Nur having been 589·5 troy grains. It is very difficult to ascertain with accuracy the values of the different units—marks, ounces, carats—in the different countries and cities in the seventeenth century; but it is probable that even the mere 4 grains, or little more than a carat, difference between De Boot's estimate of the 140 mangelins and the traditional weight of the Koh-i-Nur would disappear if we possessed these data in a more complete form. There can be little doubt that Le Cluze was in error in taking the apothecary weight instead of carat weight in translating the grains of Garcias.

It may be asked, Why devote so much consideration to this casual statement of De Boot's? The answer is twofold. The astronomer has patiently searched in the records of early observations for any that might indicate the position at a former epoch of a new-found planet; and so, where the silence about an object of historical interest has been scarcely broken through two or three centuries, one tests any observation of the casual wayfarer in the domain of literature that may perhaps shed a ray of light on it. The other reason is that, if not disposed to resent, one is at least desirous to refute, attack on those who can no longer give their own answer to assailants of a new generation, who perhaps may not bring to an investigation the learning or the patient temper of those who have gone from us, and carried great stores of scholarly learning into the silence. Whether I am right or wrong in the explanation I have offered of De Boot's conversion of Garcias's 140 mangelins into 187½ carats, I trust that at any rate I have shown cause for the statement by Mr. King that "it seems as if he (De Boot) had heard of the Koh-i-Nur; it being scarcely probable that two stones should be co-existent of that extraordinary weight."

In dealing with another of those coincidences in weight to which allusion was made, and one example of which has just been discussed, we get on the delicate ground of the degree of confidence to be placed in Tavernier's facts and figures, and the not less delicate ground of a theory about the Koh-i-Nur, started by Dr. Ball, before which the other strange vicissitudes and hairbreadth escapes of that old talisman pale into insignificance.

We have made sufficient acquaintance with the historic Indian diamond to leave it for a while, in order to introduce that other greater stone which we have designated as the "Great Mogul."

Bernier, from personal contact with whom Tavernier no doubt derived much of what had an historical character in his volumes, describes the gift by Emir Jumla, a Persian adventurer of great ability in the service of the King of Golconda, of a large diamond to the Emperor Shah Jahan, "ce grand diamant que l'on estime sans pareil." It was an appeal to his cupidity, and to a real connoisseur's passion for precious stones, at a time when the Emir was effecting a change in his allegiance from Golconda to Delhi—in fact, appealing to a new master to induce him to assail the old one.

In 1665, Tavernier, who was no less a courtier than a dealer, was invited by Aurungzebe to present himself at his Court to inspect his jewels.

The Emperor, seated on the peacock throne, could see the ceremony that was conducted in a small apartment at the end of the hall. Tavernier describes the patient circumspection with which he was shown the various stones and jewels by a Persian custodian. First and foremost among them was the great diamond, "qui est une rose (a rose-cut stone) ronde (rounded but not necessarily circular in form) fort haute d'un côté." There was a small crack at the edge below, and a little flaw within. It was of fine water, and weighed 319½ ratis, which Tavernier states to be equivalent to "280 de nos carats," the rati being ⅞ of a carat, which, however, would give 279·58 carats. Such was the only great diamond that he saw, and as he first described it.

He proceeds to give his version of its history. It was the stone given by the Emir Jumla to Shah Jahan; but he adds that, whereas it had then a weight of 900 ratis or 787½ carats, it was worked down by a Venetian diamond-cutter, Hortensio Borgis, till it had only the 280 carats weight above noted. The word *égrisée* is that used; Dr. Ball interprets it as entirely *ground* down. But, though this is the most rational meaning of this technical word, it would, as Mr. King has remarked, have taken more time than the few months which intervened between the gift and the eclipse of Shah Jahan for the mere grinding down to have been accomplished by the processes in use in the seventeenth century, and especially in India. Undoubtedly, therefore, Hortensio must have availed himself of the cleavage property of the diamond to aid him in his grinding process. Tavernier goes on to say, "Après avoir bien contemplé cette grande pierre, et l'avoir remise entre les mains d'Akel-Kan, il me fit voir un autre diamant," &c., &c.; and he then describes a number of stones and pearls, of which he gives the weights, some more or less approximately, some definitely, in ratis or in melscals (or mishkals). The melscal he also states as giving 6 to the ounce, which I think is probably a mistake for 6½ to the ounce. Finally, he says that he had held all the jewels in his hand, and considered them with sufficient attention and leisure to be able to assure the reader that his description of them is exact and trustworthy, as was that of the thrones which he previously had ample time to inspect. It will be noted he does not say he weighed any of the stones; nor does his doing so seem compatible with his description of the scene.

But in another chapter near the end of the same book he gives a brief enumeration of the finest precious stones he had, in his long travels, known. The diamond described in the earlier chapter is alluded to now with slight but immaterial variations or corrections as to weight; but Tavernier here states that he was allowed to weigh the stone, and he further adds that it had the form of an egg cut through the middle. Dr. Ball truly notes that this process may be performed in one of two ways—longitudinally, or transversely; and that the Koh-i-Nur in 1850 represented the longitudinally bisected demi-egg, but, he naïvely adds, "This difference of form, as I shall explain, was the result of the mutilation to which it was subject."

Tavernier's statement that the diamond was "fort haute d'un côté" seems, indeed, hardly to accord with any other than a longitudinal section of the egg.

But then, as if to make his description inexplicable, Tavernier appends to this later chapter—written or edited probably by another hand four or five years after the event of his handling the stone—a rude sketch of the great diamond that he saw. It may be conceived as an extremely inaccurate sketch from memory of a semi-egg-shaped stone seen "end on," or of a cross-cut half-egg seen from any point of view; but, except for the trace of a small undercut face in his projection, it has not any resemblance to the Koh-i-Nur. In width, his sketch is very slightly larger than the length of the Windsor diamond, but in no other dimension does it at all compare with that stone as it was in 1850.

Then there is the question of weight. Babar's diamond, we have seen, weighed about 8 mishkals, or, in Indian weights, about 320 ratis (gold ratis). This would correspond to 240 pearl ratis, or may be represented as 224 of the Deccan ratis of Ferishtá.

The diamond Tavernier saw weighed, he said (was he merely told so, or did he really weigh it?), 319½ ratis, only half a rati different from Babar's diamond. But Tavernier's ratis were not those which Babar reckoned by, and his carats (*nos carats*) must (*pace* Dr. Ball) have been French carats. Dr. Ball supposes he has contributed to the published data of this tangle of contradictions one new fact in a final determination of Tavernier's carat, and, by implication, of his rati also. Tavernier gives the weight in carats of the yellow diamond of the Grand Duke of Tuscany, now in the Schatzkammer at Vienna. The weight of this stone being accurately known, and being also given by Tavernier as 139½ carats, it is not difficult to determine the value of this particular carat to be 3·037 troy grains. This is in fact identical with the Florentine or Tuscan carat, as Dr. Ball points out.

That gentleman assumes from this that Tavernier always employed this carat in his calculations. Such, however, is quite incompatible with his expression on other occasions, when he speaks of "*nos carats.*" It is clear that Tavernier took the weight of this Florentine diamond from some trustworthy Tuscan source, giving it in Florentine carats. In fact, it is an illustration of what seems to be indicated as his habit in many other instances. He gives the weights of stones he mentions in ratis or mangelins, or in mishkals, and proceeds to state the equivalent weights in terms of *nos carats*, *i.e.* of the Paris carat; for no Frenchman would designate any carat other than one current in France by such a term.

It would be a tedious task to inflict on a reader the minute detail of calculation and reference to statistical authorities that would be involved in a critical study of Tavernier's assertions regarding Indian and other weights, or Dr. Ball's incursion into that study.

But one fundamental error must be alluded to, that vitiates the accuracy of Dr. Ball's calculations. He is possessed of the singular belief that, in the seventeenth century, Tavernier would have been familiar with the French ponderary system known as the *système transitoire* or *usuel*, which was introduced by the law of May 1812 into France, in temporary substitution for the old livre (*poids de marc*) of 9216 French grains, and its subdivisions.

It is quite unnecessary to follow the results of this error; for the only interest as regards our inquiry concerns the significance of the 319·5 ratis which Tavernier states the great diamond of Aurungzebe to have weighed. 320 ratis was the Hindoo equivalent, in Babar's time, of the 8 mishkals of Babar's diamond, and the Koh-i-Nur in 1850 weighed those 8 mishkals.

Tavernier says that the 319·5 ratis correspond to 280 French carats (*nos carats*). Here, then, is a second of those marvellous coincidences in numbers to which we have already made allusion—I may call them impossible coincidences, unless they apply to one and the same diamond.

Dr. Ball sees, apparently, no difficulty in the recurrence of any number of these identical figures as representing the weights of huge diamonds. For his explanation of the matter is that the diamond Tavernier handled was, as the French merchant asserted, the stone that Bernier mentions as the gift of Emir Jumla to Shah Jahan; that it did weigh 319½ ratis, but that these were ratis of Tavernier's standard, equivalent, in fact, to 0·875 of a carat, whereas Babar's ratis were only 0·578 of a carat. Dr. Ball's assertion, however, is that this great diamond is the Queen's Koh-i-Nur, but that after Nadir Shah's time it had become diminished by successive chippings performed on it by needy princes, who in succession owned it, and turned its severed fragments to account, until finally, and presumably before it fell into the hands of Runjit Singh, this great Mogul diamond had shrunk in magnitude from its asserted 280 carats to 186 carats—from the 319½ ratis of Tavernier's reckoning to the 320 ratis on Babar's reckoning; in a word, it had become reduced by this astounding process to the precise 8 mishkals of the Koh-i-Nur in 1526. So here is a third coincidence that we are called on gravely to accept as serious history.

The only originality, however, involved in this singular view of history, and the way to write it, is the reason assigned for the whittling down of the diamond from the asserted 280 carats to 186 carats. Several ingenious persons have indulged before in speculations as to the synthesis of one big diamond to be called the Koh-i-nur from several smaller ones scattered about the world, with a fine scorn of shape and weight and "water" in the component fragments, and of any historical ground whatever for their hypotheses. The late Mr. Tennant, of the Strand, even engaged the services of the great Russian diamond in this mosaic, ignorant, apparently of the facts that, like the Koh-i-Nur, it is an Indian-cut stone of about 194 carats weight, and is of a brownish-yellow hue.

But the coincidences in weight of various phantom diamonds with that which Babar recorded do not come to an end even with this crowning wonder, as I shall presently show.

Perhaps some one may, in parenthesis, ask what evidence there is for the breaking up of a great diamond by owners who clung to the Koh-i-Nur with a tenacity second only to their own hold on life. To this the answer is very simple. Not one fact or plausible argument is adduced to support it. Dr. Ball's imagination is its argument; and, indeed, I cannot find one single contribution of *fact* from that gentleman to the history of the Koh-i-Nur that has any novelty at all. There remains, however, a question that has to be answered, whether this mutilation theory be ever so wild or were ever so sane. If Tavernier saw the Great Mogul diamond, where was the old Hindoo stone? or if it was, as I have supposed, the Hindoo Koh-i-Nur that Tavernier handled, where was the Great Mogul?

Tavernier saw no second diamond of the first rank in magnitude. But there were two great diamonds somewhere—Babar's and Mir Jumla's, or, as I have designated them, the Koh-i-Nur and the Great Mogul. One or other of these Tavernier has described: where was the one he did not see?

It is now thirty-five years ago that I suggested the answer. Supposing, as I did and do, that Tavernier handled the Koh-i-Nur, I indicated the prison-palace of Shah Jahan as the repository of the Great Mogul. But, whichever diamond it may have been that the French traveller saw, the other was assuredly among those splendid stones that the old Emperor told the son who had usurped his throne that he would pound to dust if their surrender was insisted on. Anyone read in Indian history needs not to be told that the threat never had to be fulfilled; that Aurungzebe, content with the realities of power, cared little for the splendours that environed it, and left his captive father in the enjoyment of the allurements and the external pomp and vanity of a sovereign's surroundings, including the collection of jewels and precious stones in which his soul delighted. On his death they were brought to Aurungzebe by his sister Jehanira, who had shared her father's captivity.

It matters nothing to the subsequent history of the Koh-i-Nur whether it or the Great Mogul was the stone that remained in the custody of the fallen Emperor. But I have maintained that it was more probable that Shah Jahan should have retained the diamond that may be styled his private property, as having been given him by the Emir Jumla; and that therefore the stone seen in Aurungzebe's possession would in every probability have been the diamond of Babar, which, like the peacock throne and other gorgeous adornments of the presence chamber, would, as a Crown jewel, have remained in the imperial treasury.

Of course, this view of the matter involves great misgivings as regards Tavernier's accuracy. It involves his having applied to the only big diamond he saw the stories he had heard, from Bernier, no doubt, and from others, regarding that other great diamond given by the Emir Jumla to Shah Jahan. It further involves his having attempted to represent in a drawing a diamond he had seen several years before, but in a drawing so absolutely unlike the Koh-i-Nur as to be hardly recognizable as representing the Queen's diamond, and even less the diamond that he himself described, as he saw it, among the treasures of Aurungzebe.

The Great Mogul diamond had been cut by a European cutter. But, so far as it is of any value at all as evidence, Tavernier's drawing suggests a characteristically Indian-cut stone, much resembling in form and facetting the Russian diamond known as the "Orloff," which I have inspected, and can aver to be Indian in its cutting. The Koh-i-Nur, too, to which I personally gave careful attention in 1851, was no less unquestionably Indian in its facetting. Models in plaster-of-Paris made directly from the diamond confirm this; and traces of the original faces of the diamond, besides two large octahedral faces, appear to have been worked into the design of the facetting. The rows of facets were obviously put on so as to humour the original form of the stone and diminish its weight as little as possible; and notably they were thus skilfully arranged in regard to the upper edge of one of two large octahedral faces that has erroneously been described as a cleavage plane due to a fracture after the cutting had been performed. In fact, it and another large face, forming the base of the crystal, had not the lustre of cleavage surfaces, but wore the aspect of faces that had so far undergone attrition, probably in a river-bed, that the angle between them was no longer quite the true octahedral angle. The facets in general presented an imperfect adamantine lustre, and appeared slightly rounded, the result, probably, of the imperfect processes employed by the native Hindoo lapidary, especially in very early times.

Even Tavernier's drawing rudely indicates three rows of facets, put on in a manner that hardly consists with the fashion of a rose-cut diamond of European workmanship.

With my profound scepticism as to the critical value of Tavernier's arithmetic, I have ventured to think that the simplest explanation of all these instances of marvellous recurrence in various forms of the numbers representing the weight of the Koh-i-Nur is best explained by supposing that Akhil Khan gave Tavernier the traditional weight of the Babar diamond which he had placed in his hand, and that the French merchant translated this weight into carats, not as from the old ratis of Babar's or even of Akbar's day, but from the pearl ratis, of one or other value, with which he had become acquainted in the bazaars of India. Tavernier's rati, as calculated from the Paris carat on the ratio of ⅞, should have a value of 2·77088 troy grains, and as drawn from his various statements of equivalent weights it varies from 2·4066, in one case 2·750, to 2·797 troy grains. His mishkal also he puts at ⅙ the French ounce, *i.e.* 78·7 troy grains; which should, however, probably have been 6½ ounces to the mishkal, and the rati of Tavernier is entirely dissimilar to any known rati of ancient or modern India.

The 319½ ratis is readily explained on this hypothesis; and it is really too large a demand on our credulity to believe that two of the largest diamonds in the world should be severally of 319½ ratis and 320 ratis, though of different units of value, when a simpler explanation is able to dispose of the anomaly.

I have said that the marvellous coincidences of weight imported into the Koh-i-Nur history do not come to an end with Babar's 8 mishkals, with Anselm de Boot's 187½ carats, with Tavernier's 319½ ratis, nor even with Dr. Ball's miraculous chipping process, resulting in a reduction of the Great Mogul diamond to the identical weight of the Koh-i-Nur in 1850. The original diamond of Babar had to be accounted for, and its ghost had to be laid. So another coincidence had to be imported into the narrative, or rather into the romance. Another diamond had to be found, also with the precise weight of the Koh-i-Nur, and this Dr. Ball has ready to hand. The Darya-i-Nur, or "Sea of Light," reposes in the treasury of the Shah. Sir J. Malcolm saw it, and casually stated its weight as given to him at 186 carats. Now Sir J. Malcolm, during his residence at the Court of the Shah, not only was acquainted with the marvellous treasures in jewels brought by Nadir from the palace of Delhi, but he was enabled to have *facsimile* drawings of them made.

By the kindness of his son, General Malcolm, I possess the tracings of this dazzling wealth of jewellery. The Darya-i-Nur is a large flat diamond with bevelled edges, and in the form of a long rectangle. When Malcolm knew it, it was set in a glorious galaxy of mighty rubies. He could therefore have only known its weight from hearsay evidence, and the recorded carats were most likely the echo of those associated with the fame of the Koh-i-Nur. Now, I have no hesitation in asserting this Darya-i-Nur to be an old acquaintance of those familiar with Tavernier's pages. Unless two diamonds, flat, bevelled, and of identical dimensions, can be shown to co-exist, of above 200 carats weight, the stone known as the Golconda diamond or the Table diamond is no other than the Darya-i-Nur.

It happens fortunately to be one of the few stones described by Tavernier to the form and weight of which, as given by him, we can attach complete confidence. He had a lead model made from it in order to negotiate its sale: and he gives its weight as 176⅛ mangelins, or $242\frac{3}{16}$ "de nos carats." This gives its weight at 767·42 troy grains, or 240 English carats, this particular mangelin being, on Tavernier's estimate of 1⅜ of a carat, about 4·357 troy grains. Tavernier having had a lead model made of this remarkable flat diamond, he figures it no doubt with much exactitude. A copy of his figure and of the tracing of the Darya-i-Nur is subjoined, in which it will be seen that if the unsymmetrical end be cut off and the sides more accurately squared, so as to make the diamond a symmetrical rectangle, the figures of the two stones become identical in form and dimension. A card cut to represent the "Golconda" diamond, and the parts of it as described, gave the ratio of

the Golconda : the Darya-i-Nur = 10 : 8·5,

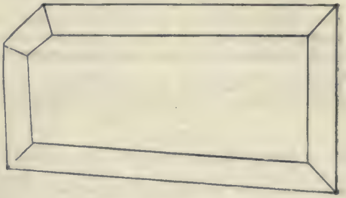
that is to say, the portion trimmed away was about 15 per cent.

The remaining stone would thus have a weight of about 214 English carats, and if 4 carats be allowed for the bevelling and squaring of the stone, the present weight of the Darya-i-Nur should be about 210 English carats.

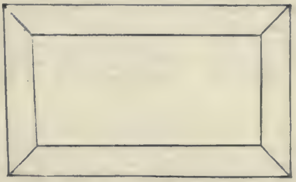
I trust I have thus laid this last phantom raised by the author of the "true history." But the final problem as to the Great Mogul diamond still remains.

If the Queen's proud trophy of the final conquest of India is indeed the great Koh-i-Nur, the old Malwa diamond descending to Her Majesty from the possession of Patan and Mogul dynasties of Delhi; carried off to Persia and named by Nadir; seized as the potent talisman of empire by Ahmed Shah, and held by his Durani descendants till it came back to India, the companion of the exile of Shah Sujah, and then torn from him by the grim Lion of Lahore—true to its destiny as "the possession, ever, of him that was the strongest,"—if this be indeed the stone that, from early times to 1850, preserved its form and weight of 8 mishkals, where was, and where is now, the Great Mogul diamond that Bernier told of? The answer is, I believe, the simplest and the most natural: It is, where the historian would look for it, in the treasury of Teheran. One large diamond, standing high upon an elliptic base, is there, or was there, in Sir John Malcolm's day. Its long diameter is much larger, and its shorter diameter smaller, than that of the diamond figured by Tavernier.

I do not assert it to be the Great Mogul. I assert merely that it probably is that great diamond; and I hope that in what has been said in the criticisms I have here offered upon the writers on the Koh-i-Nur I have averred nothing that does not rest on proof; that I have offered no conjecture that is not supported by reasonable probability; and that I have made no assault on any theory or fact asserted to be such by others, without at least offering some justification for my criticism in the reasons and facts I have been able to adduce.



Golconda Table Diamond.



Darya i-Nur.

A true history of the Koh-i-Nur has still to be written. I hope I have, in these criticisms, done something to clear the way for the writer of it. Other avocations and duties may prevent my undertaking the interesting task. At any rate, if it should ever be mine to perform it, I trust the result will at least bear some verisimilitude to a true history.

N. STORY-MASKELYNE.

SCIENTIFIC SERIALS.

A LARGE portion of the number of the *Botanical Gazette* for July is occupied by an instalment of Mr. John Donnell Smith's "Undescribed Plants from Guatemala"; several of the new species are figured. New parasitic or saprophytic Fungi—Hyphomycetes and Uredineæ—are described in this number by Mr. R. Thaxter, and in that for August by Mr. J. C. Arthur. In the latter, M . T. Holm continues his study of some anatomical characters of North American Gramineæ, and Mr. F. Lamson Scribner contributes a sketch of the flora of Orono, Maine.

THE numbers of the *Journal of Botany* for August and September contain the conclusion of Mr. G. Murray's important paper on the Algæ of the Clyde sea-area, accompanied by a map showing the various depths. This paper has now been issued separately. In his notes on Mycetozoa, Mr. A. Lister describes species found in various herbaria not included in Dr. Cooke's "Myxomycetes of Great Britain"—three of them new

The paper is illustrated by five plates. Three new British species of *Hieracium* are described by Mr. E. F. Linton and Mr. W. H. Beeby.

SOCIETIES AND ACADEMIES.

SYDNEY.

Royal Society of New South Wales, August 5.—H. C. Russell, F.R.S., President, in the chair.—On the microscopic structure of Australian rocks, by Rev. J. Milne Curran.—The Chairman presented the Society's bronze medal and a money prize of £25, which had been awarded to Father Curran for this paper.—Prof. Anderson Stuart exhibited his new instrument for demonstrating the nature of such waves as those of light.

August 10.—H. O. Walker, in the chair.—Notes on slicing rocks for microscopic study, by Rev. J. Milne Curran, illustrated by rock sections in various stages of preparation for mounting.

August 12.—C. W. Darley, in the chair.—Methods of determining the stresses in braced structures, by J. I. Haycroft.

PARIS.

Academy of Sciences, September 28.—M. Duchartre in the chair.—Notice of the works of M. P. P. Boileau, by M. Maurice Lévy.—Remarks on the international prototype of the metre, by M. Foerster.—Observations of four asteroids, discovered at Nice Observatory on August 28 and September 1, 8, and 11, by M. Charlois. The positions on the dates of discovery are given.—Verification of the law of refraction of equipotential surfaces, and measurement of the dielectric constant, by M. A. Perot.—Relation between the index of refraction of a body, its density, molecular weight, and diathermancy, by M. Aymonnet.—On the cyclone of August 18, at Martinique, by M. G. Tissandier.

BRUSSELS.

Academy of Sciences, August 1.—M. Plateau in the chair.—On the predominance and extension of Upper Eocene deposits in the region between the Senne and the Dyle, by M. Michel Mourlon.—Direct synthesis of primary alcohols, by Dr. P. Henry.—On circular sections in surfaces of the second degree, by Prof. Cl. Servais.—On the curvature of lines of the order p possessing a multiple point of the order $p - 1$, by M. A. Demoulin.—Preliminary notes on the organization and development of different forms of Anthrozoaires, by M. Paul Cerfontaine. The author describes a new Cerianthus from the Red Sea, and names it *Cerianthus brachysoma*. He has also studied in detail the tentacles of *Cerianthus membranaceus*, and the variations of these organs during successive stages of individual evolution, and relates an interesting case of regeneration observed in *Astrodes calycularis*.—Researches on the lower organisms, by M. Jean Massart.

GÖTTINGEN.

Royal Scientific Society.—The *Nachrichten* from June to August 1891 contain the following papers of scientific interest:—

June.—Karl Heun, Berlin, mathematical note on the integration of the equation for the motion of Gauss's bifilar pendulum.

July.—Fr. Schilling, note on an interpretation of the formulæ of spherical trigonometry when complex values are assigned to the sides and angles of a spherical triangle.

August.—Eduard Riecke, on the molecular theory of piezo-electricity and pyroelectricity.—Tammann and W. Nernst, on the maximum vapour tension of hydrogen liberated from solutions by metals.—Tammann, the permeability of precipitate-films.—Eduard Riecke, on a surface connected with the electrical peculiarities of tourmaline.—David Hilbert, the theory of algebraic invariants of forms with any number of variables.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Universal Atlas, Part 7 (Cassell).—Food, Physiology, &c.: W. Durham (Black).—British Edible Fungi: Dr. M. C. Cooke (Paul).—South Africa, from Arab Domination to British Rule: edited by R. W. Murray (Stanford).—An Elementary Hand-book on Potable Water: F. Davis (Gay and Bird).—The Birds of the Sandwich Islands, Part 2: Wilson and Evans (Porter).—Differential and Integral Calculus: T. H. Miller (Percival).—Physiography: J. Spencer (Percival).—Geodesy: J. H. Gore (Heinemann).—Electricity and Magnetism: A. Guillemin; translated by Prof. S. P. Thompson (Macmillan).—Annuaire de l'Observatoire Municipal de Montsouris, 1891 (Paris, Gauthier-Villars).—Stones for Building and Decoration: G. P. Merrill (New York, Wiley).—Taxidermy and Zoological Collecting: W. T. Hornaday (Paul).—Dynamics of the Sun: J. W. Davis (New York).—The Man of Genius: Prof. C. Lombroso (Scott).—Ninth Annual Report of the Fishery Board for Scotland, Three Parts (Edinburgh).—Richerche Sperimentali Intorno a Certe Scintille Elettriche constituite da Masse Luminose im Moto: Prof. A. Righi (Bologna).—Proceedings of the Liverpool Geological Society, Part 3, vol. vi. (Liverpool).—Mind, No. 64 (Williams and Norgate).—Journal of the Royal Statistical Society, September (Stanford).—Journal of the Royal Agricultural Society, 3rd series, vol. ii., Part 3 (Murray).

DIARY OF SOCIETIES.

LONDON.

THURSDAY, OCTOBER 8.

CAMERA CLUB, at 8.30.—Paper by Captain Abney.

MONDAY, OCTOBER 12.

CAMERA CLUB, at 8.30.—Lantern Evening.

THURSDAY, OCTOBER 15.

CAMERA CLUB, at 8.30.—Bacteria Photographed: Andrew Pringle.

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THURSDAY, OCTOBER 15, 1891.

PHYSICAL CHEMISTRY.

Outlines of General Chemistry. By Wilhelm Ostwald. Translated with the Author's sanction by James Walker, D.Sc., Ph.D. Pp. 396. (London: Macmillan and Co, 1890.)

THAT much may be gained by a judicious use of the methods of the physicist in elucidating chemical phenomena most chemists will admit; and, considering the rapid strides made of late years in physical chemistry, it seems surprising that so little has been done to give a connected account, suited to the wants of the student, of the main researches in this important field of investigation. Original communications on physical chemistry are on the increase. The chemist has now, in the *Zeitschrift für physikalische Chemie*, a periodical devoted exclusively to this branch of his science, and during the four years or so of the existence of this journal, its success has testified amply to the want which it supplies.

Ready access to original memoirs is not, however, the boon of the ordinary student; and, even if it were otherwise, the want of some scheme whereby to systematize his reading and classify his information, much of which is still open to wide difference of opinion, would almost invariably lead to confusion.

The majority of the text-books make little or no attempt at supplying this want. Occasionally a few of the larger chemical treatises spare a few pages to "physical methods," and such text-books as Meyer's "Modernen Theorien" or Muir's "Principles of Chemistry" contain much of the matter classed under physical chemistry.

Yet a comprehensive idea of what has been done in tracing relationships between physical properties and chemical composition and in utilizing physical measurements in investigating chemical change, cannot be obtained from most text-books. Indeed, so far as we know, only one is designed to serve this purpose, and that is the "Lehrbuch der Allgemeinen Chemie" of Prof. Ostwald. "Allgemeinen" rather than "Physikalische" "Chemie" has been used as a title for the work; but in the main it deals with physical chemistry. The book under notice seems to be an English translation of an abstract of the "Lehrbuch"; and, were it for no other reason than that it furnishes a well-conceived syllabus of the subject-matter of general and physical chemistry, it would be worthy of careful consideration.

The book is divided into two parts—Part I. chemical laws of mass; Part II. chemical laws of energy.

The first part opens with stoïchiometry. The laws of chemical combination, the determination of atomic weights, and a useful summary of the atomic weight estimations of the different elements are here given. Then follow sections treating of such of the physical properties of gases, of liquids, of solutions, and of solids as the chemist must be familiar with, and of the more important relations which have been established between such physical properties and chemical composition.

The section dealing with solutions is noteworthy as containing the first fairly complete statement, in an English text-book, of the facts grouped around the physical theory of solution which has arisen out of a knowledge of osmotic pressure. Part I. closes with chapters on chemical systematics—the choice of atomic weights, the periodic law, the development of the present conception of molecular structure.

In the earlier portions of the second part, thermochemistry, photo-chemistry, and electro-chemistry are discussed. The last takes up the constitution of electrolytes, electric conductivity, and the Arrhenius dissociation hypothesis.

Chemical dynamics and chemical affinity are treated in the last two sections, and afford many illustrations of the use of physical methods in the study of chemical change. In the case of acids competing for the same base are found instances where physical methods alone are available to estimate the nature and extent of the chemical action. In these sections, the exposition of the law of mass action, and of the velocity of chemical change, is especially clear. Owing to recent work on the subject, the discussion of affinity is here more complete than in the "Lehrbuch," and however unsatisfactory the notion of fixing specific affinity constants be considered, the account set out is the most systematic and plausible yet published.

There is no doubt that the general conception of the book is admirable; it contains much that is new; to the advanced reader it will be refreshing after the time-honoured methods of the ordinary text-books. Yet the general impression which we think will be formed on looking through it, is that the attempt made to compress information into too small a compass has detracted much from its value.

A certain amount of detail is always necessary to intelligent comprehension, and in many parts of the book there is too much bald statement to satisfy the reader who approaches the subject for the first time. Mainly for this reason it is a question whether the work will answer the expectation of the author that it will "meet the requirements of the student who, while not intending to devote himself to the detailed study of general chemistry, still wishes to follow *intelligently* the progress recently made in this important branch of science."

The time which has been spent in preparing the chapters on several important topics seems to have been inadequate. For instance, the molecular volumes of liquids are disposed of in little more than three pages. Kopp's laws are quoted, although not one of them can now be taken as valid; Schiff's inaccurate rule as to the volumes of isomers also finds a place. Instead of apparently settling the question by stating "molecular volumes to be additive magnitudes subject to constitutive influence," little more space would have been occupied in showing how, in different groups of isomers, the volume varies with the constitution. If recent progress on the subject was to be made use of, the facts that the effects of molecular weight and constitution cannot be disentangled, that even from the comparison of compounds of similar constitution, definite atomic volumes, determined for the boiling-point, cannot be obtained—that, in short, atomic volumes cannot be regarded as physical constants—ought surely to have been emphasized.

The desire to economize space is probably the cause of several examples of rather mixed information. The following paragraph occurs on p. 104:—

> "Ordinary dextrotartaric acid, for instance, has precisely the *same properties* as lævotartaric acid; but the compound of both which crystallizes from their mixed solutions on evaporation—racemic acid—has quite a different character. The first-named crystallize anhydrous, the last hydrated. The simple acids do not precipitate a solution of calcium sulphate. The compound acid does, and so forth. Yet it should be emphasized that such *differences* only occur with *solid* compounds; racemic acid behaves in *solution like a mixture* of the two components."

Seeing that this book is one of the very few in which Van der Waals's work obtains the prominence which it deserves, and which has been long delayed, it seems a pity that pains have not been taken to make the account accurate.

On p. 67 the reader is led to infer that b in Van der Waals's equation is the volume of the molecules; the true value of b is four times the volume of the molecules. Again, on p. 90, it is stated that the equation "is deduced only for the case where the volume of the substance is eight times as large as the magnitude b"; correctly given, this should be, "is deduced for cases where the volume is greater than $2b$."[1]

Admirable as may be the exposition of the theory of solution from the advanced standpoint here taken up, it may rightly be questioned whether the student is fairly treated. The physical theory of solution, the dissociation hypothesis, no one knows better than the author, are still strongly contested: should the student therefore not have heard a little more of the other side of the question? Particularly objectionable is the application of such terms as Boyle's law, Gay Lussac's law, &c., to solutions. In the opening chapters of the book the reader is familiarized with the kinetic theory of gases; he is enabled to form a mental picture of the mechanism which results in the pressure of a gas. How he, or, indeed, anyone, can form a similar picture for a solution, when the molecules of the solvent have also to be taken into consideration, it is difficult to imagine. By using for solutions a term such as Boyle's law, which for gases is capable of a perfectly definite interpretation, the real difficulty of the question is ignored, and misconception is almost sure to arise, especially in the case of the beginner.

We noticed in passing that, on p. 364, polybasic is used for dibasic; on p. 370, $k_1a = k'_2h^2a$ should be $k_1a = k'_1h^2a$. Frequently there is no distinction between the type of letters occurring in formulæ, and that in which the book is printed. Reference in the body of the book to portions of formulæ is therefore apt to lead to confusion, and in any case lacks clearness, as may be seen on pp. 297 and 369.

The work, from its very title, apart even from the reputation of the author, will no doubt appeal to a large class of readers; as an English text-book of chemistry it is unique. We venture to think, however, that if such points as those indicated were attended to, particularly the question of space, its sphere of usefulness would be materially enlarged.

J. W. R.

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[1] Physical Society Memoirs, i. 3, 453.

UNITED STATES FISH COMMISSION REPORTS.

Bulletin of the United States Fish Commission. Vol. VIII. for 1888. (Washington, 1890.)

IN 1881 the Senate and House of Representatives of the United States of America authorized the public printer to print from time to time any matter furnished to him by the United States Commissioner of Fish and Fisheries relative to new observations, discoveries, and applications connected with fish culture and the fisheries. The printed matter was to be capable of being distributed in parts, the whole was to form an annual volume or Bulletin not exceeding 500 pages, and the edition was to be limited to 5000 copies.

Seven volumes of this important series have since been published, and have been noticed in our pages. They were composed chiefly of translations or republications of articles on fish or fisheries which had appeared in European periodicals or as State documents; extracts from the official correspondence, with statistics of work done; and often of short articles of direct scientific interest on American fish; the whole forming a most valuable, practical encyclopædia of everything relating to the economic study of fish.

An eighth volume, dated 1890, but being the Bulletin for 1888, has just been issued from the Washington Press. The increased operations of the United States Fish Commission during 1888 have made it possible to devote almost the whole of this volume to the results of the work of the Commission, and it will be found to contain matter of considerable interest. The size of the volume has been slightly enlarged, so as to afford room for larger illustrations.

Of the twelve memoirs or papers contained in this volume, five relate to local collections of fishes. Mr. Tarleton H. Bean gives notes on a collection made at Cozumel, Yucatan: sixty species are enumerated; two new species are described and figured. Mr. C. H. Bollman reports on the fishes of Kalamazoo, Calhoun, and Antrim counties in Michigan. Mr. S. A. Forbes contributes a preliminary account of the invertebrate animals inhabiting Lakes Geneva and Mendota, in Wisconsin, and gives some particulars of the fish epidemic in the latter lake in 1884. Mr. C. H. Gilbert describes some fish from the lowlands of Georgia. Mr. D. S. Jordan gives a report of explorations made during 1888 in the Alleghany region of Virginia, North Carolina, and Tennessee, and in Western Indiana, with an account of the fishes found in each of the river-basins of those regions.

In a review of the genera and species of Serranidæ, by D. S. Jordan and C. H. Eigenmann, we have an enumeration of all the genera and species belonging to this family found in the waters of America and Europe, together with the synonymy of each, and analytical keys by which the different groups may be distinguished. One hundred and nineteen species are admitted, and thirty-four genera. This memoir is illustrated with ten plates. Mr. J. W. Collins contributes a paper on improved types of vessels for use in the market fisheries, with some notes on British fishing-steamers; and Mr. W. F. Page gives an account of the most recent methods of hatching fish-eggs. Mr. T. H. Bean reports favourably on the feasibility of introducing the mountain mullets of Jamaica (Agonostoma) into some of the Alpine streams of the Southern States; and Mr. R. Rathbun gives a detailed report on the introduction of lobsters to the Pacific shores of the United States.

The two most important contributions to this volume are, however, those by Lieutenant Tanner, "On the Result of the Explorations of the Fishing-grounds of Alaska, Washington Territory, and Oregon during 1888," and by Mr. John A. Ryder, "On the Sturgeons and Sturgeon Industries of the Eastern Coast of the United States."

Although it had been known for many years that the Pacific coasts of North America were abundantly provided with edible fishes, it was not until 1880 that the exact species of these were correctly determined; the Alaskan cod proving to be the same species as that of the North Atlantic. The absence of large and convenient markets hindered the development of the Pacific coast fisheries; but, with the completion of the railroad system, this state of things has changed, and a strong interest is now being shown in all that relates to the development of the fish industry. This Report affords us the first accurate information that has been obtained respecting most of the fishing-grounds in Alaska. The five banks whose positions were indicated by older surveys—namely, Davidson, Sannakh, Shumagin, Albatross, and Portlock banks—were more thoroughly examined than were the intervening areas, some of which, however, may, upon further examination, prove to contain fishing-banks of equal value, and not inferior in size, to at least the smaller of the banks mentioned.

Good fishing was obtained at nearly all localities where trials were made with hand-lines, whether upon defined banks or upon the more level grounds between them, and it seems natural to infer that the entire submerged plateau from off Unalashka Island to Fairweather Ground is one immense fishing-bank, limited upon the outer side only by the abrupt slope, which may be said to begin about the 100-fathom curve.

Although the great bulk of this Report relates to the fishing-banks and fishes, yet we get various glimpses of many interesting facts relating to other of the vertebrate and to many of the invertebrate forms met with. Off Popoff Island, large masses of sea-urchins, star-fishes, and large Medusæ were found in the seine nets, and the hooks became entangled with fine specimens of sea-pens (Pennatula). At the Lighthouse Rocks a landing was made, to examine a large rookery of Steller's sea-lion (*Eumetopias stelleri*). Several hundreds of these animals were found crowded together upon a very limited area. As the party landed, the old sea-lions came tumbling down over the rocks in great eagerness to reach the sea; a few, whose retreat was intercepted, were seen to jump from their high positions directly into the water, apparently sustaining no injury from the plunge, although the distance was considerable, especially for such large animals. A couple of killer whales (Orca), attracted by the disturbance and the sight of so many seals in the water, came quite close to the rocks, causing the seals to gather nearer the shore, and to cast frightened looks of alarm towards the whales, whose dorsal fins showed not less than four feet above the surface of the water. These rocks were entirely destitute of vegetation. Off Trinity Islands, large quantities of crustaceans, worms, mollusks, echinoderms, and sponges were taken—an especial feature of the haul consisting of over a hundred specimens of a fine large free crinoid. As all these specimens will find their way to the United States National Museum, we may expect soon to have recorded many additions to the marine fauna of the North Pacific.

Mr. John A. Ryder's paper will also be perused with great interest. Having undertaken to report on the sturgeons and sturgeon fisheries of the eastern rivers of the United States, he repaired in May 1888 to Delaware City, which is described as a very important centre of the sturgeon fishery. Two species of the genus Acipenser are to be found in the waters along the Atlantic coast of the United States; these are *A. sturio*, L., and *A. brevirostris*, Le Sueur. The former (the common sturgeon) is the only one of any commercial importance at Delaware, as Le Sueur's species is so rare that only five specimens of it were taken by Mr. Ryder; and since the date of its first being described, in 1817, it does not appear to have been until now again recognized. Of the other American species, one is the very distinct fresh-water sturgeon of the Lake region, and two others are to be found on the Pacific coast.

The embryological data of this memoir have been in a good measure drawn from the author's original investigations, but he has fortunately also given us in addition details from the writings of Balfour, Knoch, Parker, Zograff, and Salensky. He found it perfectly practicable to fertilize artificially the sturgeon's roe, and thinks it possible that millions of young sturgeon might be developed in this way. He treats in detail of the dermal armature of the sturgeon's body, illustrating this part of his subject by numerous photogravures, describes the organs of locomotion, the lateral line system, the viscera, and lymphatics. The sources of the food of this fish and its peculiar habits are next considered, and special information is given about the preparing of the flesh for market, and the manufacture of the caviare. A very useful bibliography of the literature relating to the sturgeon is appended. This memoir is illustrated by twenty-two plates.

THE CATALOGUE OF THE WASHINGTON MEDICAL LIBRARY.

Index Catalogue of the Library of the Surgeon-General's Office, U.S. Army. Vol. XI. Phædronus—Régent. Pp. 1102. (Washington, 1890.)

THE appearance of these very fine folios year by year for the last eleven years is a very good proof to all lovers of books and collections of books in Europe that they have some sympathetic friends in America who have the will and the power to make one at least of their finest libraries well known throughout the world. Its title as the Library of the Surgeon-General's Office may once have sounded like the name of a collection of musty Blue books tied together with red tape; but, thanks to the energy of its Librarian, Mr. J. S. Billings, which we feel constantly in the monthly publication of the *Index Medicus*, everyone knows now that it is nothing of the kind, but one of the first medical libraries, if not the first, in the world, containing much more medical literature than is to be found in the libraries of the richer English corporations, the Royal Colleges of Physicians and Surgeons, or of the more learned and active Societies, such as the Royal Medical and Chirurgical Society, or, indeed, in the British Museum or Bibliothèque Nationale. And though the Washington Library is of comparatively recent date, going back only some thirty years, yet it contains a very fine collection of books both of the fifteenth and sixteenth centuries; and at the same time the great difficulty of the maker of a catalogue to a modern library, viz. the immense mass of the newspaper and periodical literature of to-day, has been fairly faced and overcome. During the past year, 287 periodicals have been added to the list of those that are taken in, raising the total number to about 7500, of which at least 3900 are current. The vast aggregate of articles in these are duly catalogued, each under the head of its subject-matter. It is not surprising, therefore, that we should find 80 of these large square folio pages filled in the present volume with entries under the heading Phthisis, 78 under Puerperal Diseases, 67 under Pregnancy, and 56 under Pneumonia. Even as devoted entirely to a lesser matter like the pulse, there are catalogued 150 volumes and 350 articles in periodicals. The care with which the records of the smallest steps in the past history of medicine have been preserved is shown by the accumulation of twenty-five editions of the "Pharmacopœia" of the Royal College of Physicians of London from the years 1657 to 1851. Under such headings as Psychology, we may see the wide range also of the larger subjects embraced in the Library, for the collection under this heading begins with many expositions of Aristotle, and does not neglect Plato, but takes in also the recent books of modern authors, such as the last edition of Herbert Spencer's "Principles of Psychology" and Taine's "De l'Intelligence." The eleventh volume of this magnificent catalogue brings us to within measurable distance of the end; from the analogy of lesser works, in fact, it seems probable it may be completed in three or at most four volumes, and it will then be a great monument among modern catalogues, and in its articles under subject titles form a most valuable dictionary to all who are seeking a clue to the complete historical study of medicine and surgery.

A. T. Myers.

OUR BOOK SHELF.

Dictionary of Political Economy. Edited by R. H. Inglis Palgrave, F.R.S. Part I. Abatement—Bede. (London: Macmillan and Co., 1891.)

This is a first instalment of what promises to be a very valuable addition to the English library of political economy. The plan of the work is laid down on broad lines, and includes not only articles dealing with strictly economic subjects, and explanations of legal and business terms, but good (though necessarily brief) accounts of historical events bearing on economic history, such as the establishment and downfall of the *ateliers nationaux* in Paris in 1848, and biographical notices of deceased writers whose life and work has had any connection with the development of economic theory or practice. That the biographical section of the dictionary is conceived in a liberal spirit is sufficiently proved by the fact that the first part, now under review, includes notices of Addison and Thomas Aquinas; the claim of the former to a place in a dictionary of political economy is based in the main on the fact that he held an official position in the Government of his time as one of the Lords Commissioners of Trade. This rather remote connection with economics may be open to criticism; and it remains to be seen whether Mr. Palgrave will include in his dictionary the honoured names of William Wordsworth and Robert Burns. It is not, however, desirable to say anything in the way of criticism which should tend to narrow the scope of the work. Its interest and vitality depend, to a large degree, on its broad inclusiveness.

The biographical articles are particularly well done, and we would single out that on the late Mr. Bagehot for special commendation. It gives not only the dry facts of his career, but presents a living picture of a peculiarly fascinating personality, and also a very just estimate of his place in, and services to, economic literature. Among the most important articles in the present instalment of the dictionary may be mentioned that on agricultural communities, by Prof. J. S. Nicholson, and that on banks. The former gives an admirable summary of the conditions of life in existing village communities in Russia and India, and also a digest of the results arrived at by the researches of Sir Henry Maine, Mr. Seebohm, and M. de Laveleye, as to the existence of various forms of village communities in the remote past in our own and other countries. The article on banks gives an historical sketch of the development of banking in various countries, contributed by different writers, each with special knowledge of his own portion of the subject. Thus we have brought together within the compass of a few pages an account of the land banks and the Schulze Delitsch credit banks of Germany, the savings banks (trustee and Post-office) of England, and the popular banks of Italy.

The names of the contributors to the present volume, and also those who have promised their assistance in the preparation of the rest of the work, are a guarantee of its high value to all students of social and economical subjects.

South Africa, from Arab Domination to British Rule. Edited by R. W. Murray, F.R.G.S. With Maps, &c. (London: Edward Stanford, 1891.)

One of the objects of this book is to bring out the contrast between Portuguese rule in South Africa and the influence exerted by England. The contrast is certainly striking enough; and it is shown most clearly, as in the present work, by a simple statement of historic facts. In the first chapter, Prof. Keane sketches the career of the Portuguese in the various South African regions they have dominated. This is followed by translations from the "Africa" of Dapper, a Dutch writer of the seventeenth century, showing that at that time the Portuguese stationed on the African coasts made no effort to acquire extensive knowledge of the interior. The editor then records the main facts relating to the Dutch and English settlements in the south, and the recent movements northward to Bechuanaland, Matabeleland, and Mashonaland. Mr. J. W. Ellerton Fry, late of the Royal Observatory, Cape Town, Lieutenant of the British South African Company's expeditionary force, gives an account of what he himself observed during the march into Mashonaland in 1890; and much information with regard to the east coast of Africa at Beira, Pungwe, and the Zambesi is presented in notes from the diary and correspondence of Mr. Neville H. Davis, late surveyor and hydrographer to the Queensland Government, who, in 1890, accompanied an expedition sent to East Africa to discover whether there was any mineral or other wealth in concessions granted by the Mozambique Company. The book has not been very systematically planned; but it brings together so many facts which are not readily accessible elsewhere, that it cannot fail to interest readers whose attention is for any reason especially directed to South Africa. It includes several excellent maps, and two engravings of Cape Town, showing Cape Town as it was in 1668, and as it is in 1891.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

A Pink Marine Micro-organism.

WHILE dredging lately in Loch Fyne, I noticed through the clear water, in a little shallow bay on the north side of the entrance to East Loch Tarbert, a number of pink patches on the sand. These could just be reached by wading from a boat at the lowest tides, and were then found to be roughly circular spots, about a foot in diameter, where the clean white sand was discoloured, most of the surface grains being almost exactly the tint of ordinary pink blotting-paper.

Under a low power of the microscope, it is seen that the pink particles are ordinary clear quartz sand-grains, incrusted with little bright pink jelly masses, generally of elongated or sausage-like forms, and averaging 0·1 mm. in length. Further magnification shows that each jelly mass is crowded with minute very short rods, or ellipsoids, of about 0·0015 mm. in length, and about half as much in breadth.

This appears to be a micro-organism in the zooglœa condition, and I do not know that any such pink marine form, living on clean sand, in *pure* sea water, has been noticed. It may possibly be one of the forms of *Beggiatoa rosea-persicina*, but it does not agree satisfactorily with any of the descriptions I have access to here. I have still some of the material alive in sea water, and shall be glad to hand it over to any biologist who is now working specially at such forms, and would like to investigate this one.

W. A. HERDMAN.

University College, Liverpool, October 6.

Advertisements for Instructors.

THE friends of technical education can no longer complain that the subject is not receiving attention. The numerous advertisements for instructors of all sorts, from County Councils and other bodies, colleges and schools are full evidence that much is being attempted.

Whether all the plans and proposals and experiments will lead to the hoped-for results only time will show. Some of us have our doubts as regards many of them.

Meantime, one of the advertisements deserves a passing notice. A well-known technical school is in search of "a demonstrator in the Metallurgical Department to take the lectures in geology and mineralogy, and to give instruction in dry assaying and in iron and steel analysis" (see NATURE of this week).

This is certainly a large and considerably mixed "order," calculated to make thoughtful people wonder what sort of instruction is expected to be given by this gifted person (who is to have the princely sum of £100 per annum); and whether, if the "metallurgical demonstrator" is to throw in geology and mineralogy as a sort of extra to his own special work, the other demonstrators and professors are expected to be equally widely qualified; let us say a chemical demonstrator to give lectures on mechanical engineering and ship-building?

Newcastle-on-Tyne, October 10. M.

"Rain-making."

I THINK the following will be of interest to your readers in connection with the "rain-making" experiments in Texas. On October 1, at 5 p.m., five tons of gunpowder was exploded in a single blast at the Penrhyn slate quarries in order to clear away a very large mass of useless rock. A strong wind had been blowing all day, and the clouds, though heavy, were high; there had been no rain, and not much sunshine, and the temperature was somewhat low.

Immediately after the explosion the wind fell to a dead calm, which lasted about 5 or 6 minutes, and 20 minutes later a fine rain began to fall, which soon became heavy and continued for an hour and a half. By 7 p.m. all disturbances produced by the explosion had apparently passed away, and the weather was again similar to what it had been during the day. The rainfall was entirely local, there being none, as far as I could learn, outside a radius of 6 or 7 miles from the quarry.

W. R. PIDGEON.

Alum Solution.

WITH reference to the question raised by Mr. H. N. Draper in NATURE, vol. xliv. p. 446, as to the practical superiority of an alum solution over simple water in absorbing such radiations as are chiefly instrumental in producing heat, I may recall some experiments made by myself five years ago (Brit. Assoc. Report, 1886, p. 309). The source of radiation employed was a paraffin lamp with a glass chimney, the various solutions were contained in a glass cell with parallel sides, and the "radiometer" was a delicate thermopile, the face of which was blackened with camphor smoke. The following results, among others, were obtained:—

Solutions, &c.	Diathermancy.
Empty cell	1000
Water distilled	197
Water from tap	200
Alum, saturated solution	204

It is clear therefore that, at least under conditions like those of my experiment, plain water will answer the purpose of an absorbent rather better than an alum solution. Possibly the "alum cell" tradition rests upon no better foundation than many others, which are generally accepted simply because it does not occur to people to question them.

SHELFORD BIDWELL.

October 10.

B.Sc. Exam. Lond. Univ. 1892.

THERE are, I believe, in London at the present time a number of men desirous of offering geology as one of three subjects required at the Degree Examination in Science, but who are deterred from so doing by the fact that it is impossible to obtain adequate evening class tuition in this subject.

Enquiries at the various teaching institutions have failed to discover a single opportunity for working up to the required standard in both theoretical and practical branches.

I have therefore laid the matter before Prof. Wiltshire, of King's College, Strand, with the result that he has very kindly consented, in the event of enough men requiring it, to supplement his lectures on geology and mineralogy by a course of instruction in petrology, embracing the study of hand specimens and microscopical examination of rock sections.

By giving publicity to the matter, it is hoped that a sufficient number of B.Sc. candidates will be forthcoming to ensure the establishment of this class.

The time-table for the complete course will be as follows:—

Monday	Petrology ...	6–7 p.m.
	Mineralogy ...	7–8 ,,
	Geology ...	8–9 ,,

The lectures and practical work, together with the summer field excursions, under the direction of Prof. Wiltshire, will prove a great boon to such as are prevented from attending day courses, and will undoubtedly secure admirable preparation for the examination specified.

I shall be glad to hear from anyone interested in the matter, so that arrangements may at once be made for the first sitting to take place on Monday, October 19.

EDWARD J. BURRELL.

People's Palace, Mile End Road, E.

Some Notes.

THOSE who have visited Venice in spring know how rampant mosquitoes become after the flight of the swallows, which have kept them in check, for the north—usually in May.

A word for the sparrows—which have been very active in the gardens hereabouts this season, preying on the green flies and larvæ infesting the creepers and ferns in particular; but very few starlings have been observed, to the great increase of earthworms in the lawns. The crane-fly, which usually swarms in the fields of the Mansfield estate in September, has been very rare, too, this season. The dragon-fly visited us this summer for the first time.

Apropos to the records of the "rare phenomenon," such a summer aurora was observed at Rothbury, Northumberland, in the latter half of August 1880.

To conclude this farrago of notes: for "*non* pas travaillés" in Mr. Sclater's quotation of the Prince of Canino's words (xliv. p. 518), read "*n'ont* . . ." J. J. WALKER.

Hampstead, N.W., October 3.

THE MOLECULAR PROCESS IN MAGNETIC INDUCTION.[1]

MAGNETIC induction is the name given by Faraday to the act of becoming magnetized, which certain substances perform when they are placed in a magnetic field. A magnetic field is the region near a magnet, or near a conductor conveying an electric current. Throughout such a region there is what is called magnetic force, and when certain substances are placed in the magnetic field the magnetic force causes them to become magnetized by magnetic induction. An effective way of producing a magnetic field is to wind a conducting wire into a coil, and pass a current through the wire. Within the coil we have a region of comparatively strong magnetic force, and when a piece of iron is placed there it may be strongly magnetized. Not all substances possess this property. Put a piece of wood or stone or copper or silver into the field, and nothing noteworthy happens; but put a piece of iron or nickel or cobalt and at once you find that the piece has become a magnet. These three metals, with some of their alloys and compounds, stand out from all other substances in this respect. Not only are they capable of magnetic induction—of becoming magnets while exposed to the action of the magnetic field—but when withdrawn from the field they are found to retain a part of the magnetism they acquired. They all show this property of retentiveness, more or less. In some of them this residual magnetism is feebly held, and may be shaken out or otherwise removed without difficulty. In others, notably in some steels, it is very persistent, and the fact is taken advantage of in the manufacture of permanent magnets, which are simply bars of steel, of proper quality, which have been subjected to the action of a strong magnetic field. Of all substances, soft iron is the most susceptible to the action of the field. It can also, under favourable conditions, retain, when taken out of the field, a very large fraction of the magnetism that has been induced—more than nine-tenths—more, indeed, than is retained by steel; but its hold of this residual magnetism is not firm, and for that reason it will not serve as a material for permanent magnets. My purpose to-night is to give some account of the molecular process through which we may conceive magnetic induction to take place, and of the structure which makes residual magnetism possible.

When a piece of iron or nickel or cobalt is magnetized by induction, the magnetic state permeates the whole piece. It is not a superficial change of state. Break the piece into as many fragments as you please, and you will find that every one of these is a magnet. In seeking an explanation of magnetic quality we must penetrate the innermost framework of the substance—we must go to the molecules.

Now, in a molecular theory of magnetism there are two possible beginnings. We might suppose, with Poisson, that each molecule becomes magnetized when the field begins to act. Or we may adopt the theory of Weber, which says that the molecules of iron are always magnets, and that what the field does is to turn them so that they face more or less one way. According to this view, a virgin piece of iron shows no magnetic polarity, not because its molecules are not magnets, but because they lie so thoroughly higgledy-piggledy as regards direction that no greater number point one way than another. But when the magnetic force of the field begins to act, the molecules turn in response to it, and so a preponderating number come to face in the direction in which the magnetic force is applied, the result of which is that the piece as a whole shows magnetic polarity. All the facts go to confirm Weber's view. One fact in particular I may mention at once—it is almost conclusive in itself. When the molecular magnets are all turned to face one way, the piece has clearly received as much magnetization as it is capable of. Accordingly, if Weber's theory be true, we must expect to find that in a very strong magnetic field a piece of iron or other magnetizable metal becomes *saturated*, so that it cannot take up any more magnetism, however much the field be strengthened. This is just what happens: experiments were published a few years ago which put the fact of saturation beyond a doubt, and gave values of the limit to which the intensity of magnetization may be forced.

When a piece of iron is put in a magnetic field, we do not find that it becomes saturated unless the field is exceedingly strong. A weak field induces but little magnetism; and if the field be strengthened, more and more magnetism is acquired. This shows that the molecules do not turn with perfect readiness in response to the deflecting magnetic force of the field. Their turning is in some way resisted, and this resistance is overcome as the field is strengthened, so that the magnetism of the piece increases step by step. What is the directing force which prevents the molecules from at once yielding to the deflecting influence of the field, and to what is that force due? And again, how comes it that after they have been deflected they return partially, but by no means wholly, to their original places when the field ceases to act?

I think these questions receive a complete and satisfactory answer when we take account of the forces which the molecules necessarily exert on one another in consequence of the fact that they are magnets. We shall study the matter by examining the behaviour of groups of little magnets, pivoted like compass needles, so that each is free to turn except for the constraint which each one suffers on account of the presence of its neighbours.

But first let us see more particularly what happens when a piece of iron or steel or nickel or cobalt is magnetized by means of a field the strength of which is gradually augmented from nothing. We may make the experiment by placing a piece of iron in a coil, and making a current flow in the coil with gradually increased strength, noting at each stage the relation of the induced magnetism to the strength of the field. This relation is observed to be by no means a simple one: it may be represented by a curve (Fig. 1), and an inspection of the curve will show that the process is divisible, broadly, into three tolerably distinct stages. In the first stage (*a*) the magnetism is being acquired but slowly: the molecules, if we accept Weber's theory, are not responding readily—they are rather hard to turn. In the second stage (*b*) their resistance to turning has to a great extent broken down, and the piece is gaining magnetism fast. In the third stage (*c*) the rate of increment of magnetism falls off: we are there approaching the condition of saturation, though the process is still a good way from being completed.

Further, if we stop at any point of the process, such as P, and gradually reduce the current in the coil until there is no current, and therefore no magnetic field, we shall get a curve like the dotted line PQ, the height of Q showing the amount of the residual magnetism.

If we make this experiment at a point in the first stage

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[1] Abstract of a Friday Evening Discourse delivered at the Royal Institution, on May 22, 1891, by J. A. Ewing, M.A., F.R.S., Professor of Applied Mechanics and Mechanism in the University of Cambridge.

(*a*), we shall find, as Lord Rayleigh has shown, little or no residual magnetism; if we make it at any point in the second stage (*b*), we shall find very much residual magnetism; and if we make it at any point in the third stage (*c*), we shall find only a little more residual magnetism than we should have found by making the experiment at the end of stage *b*. That part of the turning of the molecules which goes on in stage *a* contributes nothing to the residual magnetism. That part which goes on in stage *c* contributes little. But that part of the turning which goes on in stage *b* contributes very much.

In some specimens of magnetic metal we find a much sharper separation of the three stages than in others. By applying strain in certain ways it is possible to get the stages very clearly separated. Fig. 2, a beautiful instance of that, is taken from a paper by Mr. Nagaoka—one of an able band of Japanese workers who are bidding fair to repay the debt that Japan owes for its learning to the West. It shows how a piece of nickel which is under the joint action of pull and twist becomes magnetized in a growing magnetic field. There the first stage is exceptionally prolonged, and the second stage is extraordinarily abrupt.

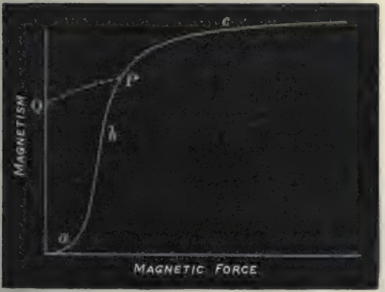

Fig. 1.

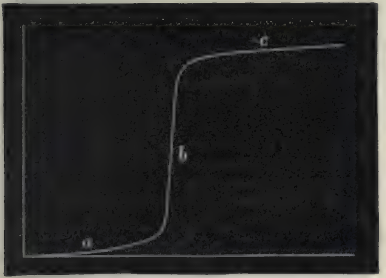

Fig. 2.

The bearing of all this on the molecular theory will be evident when we turn to these models, consisting of an assemblage of little pivoted magnets, which may be taken to represent, no doubt in a very crude way, the molecular structure of a magnetizable metal. I have here some large models, where the pivoted magnets are pieces of sheet steel, some cut into short flat bars, others into diamond shapes with pointed ends, others into shapes resembling mushrooms or umbrellas, and in these the magnetic field is produced by means of a coil of insulated wire wound on a large wooden frame below the magnets. Some of these are arranged with the pivots on a gridiron or lazy-tongs of jointed wooden bars, so that we may readily distort them, and vary the distances of the pivots from one another, to imitate some of the effects of strain in the actual solid. But to display the experiments to a large audience a lantern model will serve best. In this one the magnets are got by taking to pieces numbers of little pocket compasses. The pivots are cemented to a glass plate, through which the light passes in such a way as to project the shadows of the magnets on the screen. The magnetic force is applied by means of two coils, one on either side of the assemblage of magnets and out of the way of the light, which together produce a nearly uniform magnetic field throughout the whole group. You see this when I make manifest the field in a well-known fashion, by dropping iron filings on the plate.

Fig. 3.

We shall first put a single pivoted magnet on the plate. So long as no field acts it is free to point anyhow—there is no direction it prefers to any other. As soon as I apply even a very weak field it responds, turning at once into the exact direction of the applied force, for there was nothing (beyond a trifling friction at the pivot) to prevent it from turning.

Now try two magnets. I have cut off the current, so that there is at present no field, but you see at once that the pair has, so to speak, a will of its own. I may shake or disturb them as I please, but they insist on taking up a position (Fig. 3) with the north end of one as close as possible to the south end of the other. If disturbed they return to it: this configuration is highly stable. Watch what happens when the magnetic field acts with gradually growing strength. At first, so long as the field is weak (Fig. 4), there is but little deflection; but as the deflection increases it is evident that the stability is being lost, the state is getting more and more critical, until (Fig. 5) the tie that holds them together seems to break, and they suddenly turn, with violent swinging, into almost perfect alignment with the magnetic force H. Now I gradually remove the force, and you see that they are slow to return, but a stage comes when they swing back, and a

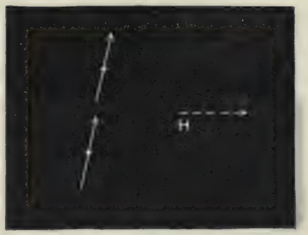

Fig. 4.

complete removal of the force brings them into the condition with which we began (Fig. 3).

If we were to picture a piece of iron as formed of a vast number of such pairs of molecular magnets, each pair far enough from its neighbours to be practically out of reach of their magnetic influence, we might deduce many of the observed magnetic properties, but not all.

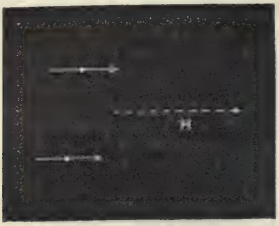

Fig. 5.

In particular, we should not be able to account for so much residual magnetism as is actually found. To get that, the molecules must make new connections when the old ones are broken; their relations are of a kind more complex than the quasi-matrimonial one which the experiment exhibits. Each molecule is a member of a larger community, and has probably many neighbours close enough to affect its conduct.

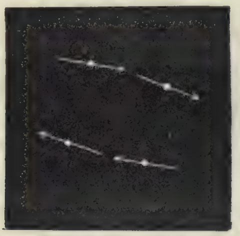

Fig. 6.

We get a better idea of what happens by considering four magnets (Fig. 6). At first, in the absence of deflecting magnetic force, they group themselves in stable pairs—in one of a number of possible combinations. Then—as in the former case—when magnetic force is applied, they are at first slightly deflected, in a manner that exactly tallies with what I have called the stage *a* of the magnetizing process. Next comes instability. The original ties break up, and the magnets swing violently round; but finding a new possibility of combining (Fig. 7), they take to that. Finally, as the field is further strengthened, they are drawn into perfect alignment with the applied magnetic force (Fig. 8).

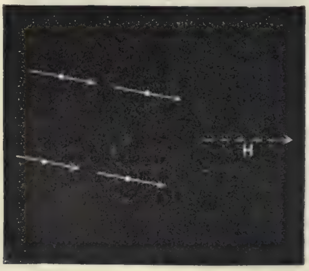

Fig. 7.

We see the same three stages in a multiform group (Figs. 9, 10, 11). At first, the group, if it is shuffled by any casual disturbance, arranges itself at random in lines that give no resultant polarity (Fig. 9). A weak force produces no more than slight quasi-elastic deflections; a stronger force breaks up the old lines, and forms new ones more favourably inclined to the direction of the force (Fig. 10). A very strong force brings about saturation (Fig. 11).

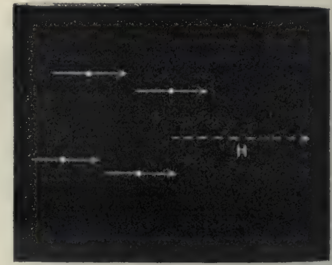

Fig. 8.

In an actual piece of iron there are multitudes of groups lying differently directed to begin with—perhaps also different as regards the spacing of their members. Some enter the second stage while others are still in the first, and so on. Hence, the curve of magnetization does not consist of perfectly sharp steps, but has the rounded outlines of Fig. 1.

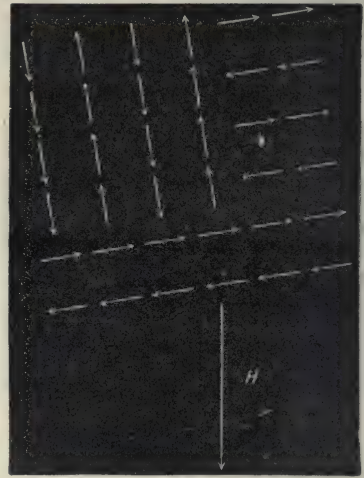

Fig. 9.

Notice, again, how the behaviour of these assemblages of elementary magnets agrees with what I have said about residual magnetism. If we stop strengthening the field before the first stage is passed—before any of the magnets have become unstable and have tumbled round into new places—the small deflection simply disappears, and there is no residual effect on the configuration of the group. But if we carry the process far enough to have unstable deflections, the effects of these persist when the force is removed, for the magnets then retain the new grouping into which they have fallen (Fig. 10). And again, the quasi-elastic deflections which go on during the third stage do not add to the residual magnetism.

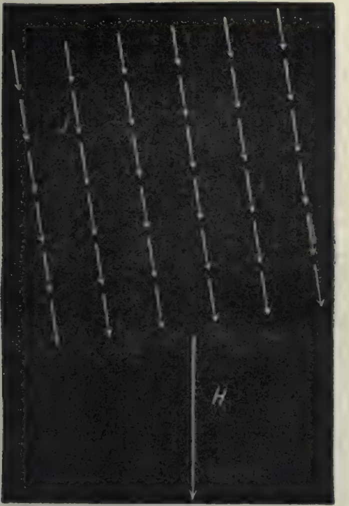

Fig. 10.

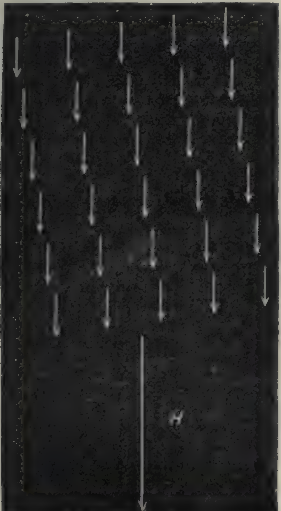

Fig. 11.

Notice, further, what happens to the group if after applying a magnetic force in one direction and removing it, I begin to apply force in the opposite direction. At first there is little reduction of the residual polarity, till a stage is reached when instability begins, and then reversal occurs with a rush. We thus find a close imitation of all the features that are actually observed when iron or any of the other magnetic metals is carried through a cyclic magnetizing process (Fig. 12). The effect of any such process is to form a *loop* in the curve which expresses the relation of the magnetism to the magnetizing force. The changes of magnetism always lag behind the changes of magnetizing force. This tendency to lag behind is called magnetic *hysteresis*.

We have a manifestation of hysteresis whenever a magnetic metal has its magnetism changed in any manner through changes in the magnetizing force, unless indeed the changes are so minute as to be confined to what I have called the first stage (*a*, Fig. 1). Residual magnetism is only a particular case of hysteresis.

Hysteresis comes in whatever be the character or cause of the magnetic change, provided it involves such deflections on the part of the molecules as make them become unstable. The unstable movements are not reversible with respect to the agent which produces them; that is to say, they are not simply undone step by step as the agent is removed.

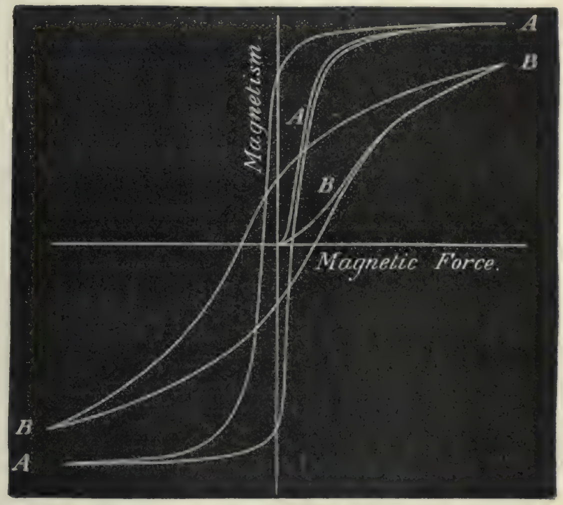

Fig. 12.—Cyclic reversal of magnetization in soft iron (AA), and in the same iron when hardened by stretching (BB).

We know, on quite independent grounds, that when the magnetism of a piece of iron or steel is reversed, or indeed cyclically altered in any way, some work is spent in performing the operation—energy is being given to the iron at one stage, and is being recovered from it at another; but when the cycle is taken as a whole, there is a net loss, or rather a waste of energy. It may be shown that this waste is proportional to the area of the loop in our diagrams. This energy is dissipated; that is to say, it is scattered and rendered useless: it takes the form of heat. The iron core of a transformer, for instance, which is having its magnetism reversed with every pulsation of the alternating current, tends to become hot for this very reason; indeed, the loss of energy which happens in it, in consequence of magnetic hysteresis, is a serious drawback to the efficiency of alternating-current systems of distributing electricity. It is the chief reason why they require much more coal to be burnt, for every unit of electricity sold, than direct-current systems require.

The molecular theory shows how this waste of energy occurs. When the molecule becomes unstable and tumbles violently over, it oscillates and sets its neighbours oscillating, until the oscillations are damped out by the eddy currents of electricity which they generate in the surrounding conducting mass. The useful work that can be got from the molecule as it falls over is less than the work that is done in replacing it during the return portion of the cycle. This is a simple mechanical deduction from the fact that the movement has unstable phases.

I cannot attempt, in a single lecture, to do more than glance at several places where the molecular theory seems to throw a flood of light on obscure and complicated facts, as soon as we recognize that the constraint of the molecules is due to their mutual action as magnets.

It has been known since the time of Gilbert that vibration greatly facilitates the process of magnetic induction. Let a piece of iron be briskly tapped while it lies in the magnetic field, and it is found to take up a large addition to its induced magnetism. Indeed, if we examine the successive stages of the process while the iron is kept vibrating by being tapped, we find that the first stage (*a*) has practically disappeared, and there is a steady and rapid growth of magnetism almost from the very first. This is intelligible enough. Vibration sets the molecular magnets oscillating, and allows them to break their primitive mutual ties and to respond to weak deflecting forces. For a similar reason, vibration should tend to reduce the residue of magnetism which is left when the magnetizing force is removed, and this, too, agrees with the results of observation.

Perhaps the most effective way to show the influence of vibration is to apply a weak magnetizing force first, before tapping. If the force is adjusted so that it nearly but not quite reaches the limit of stage (*a*), a great number of the molecular magnets are, so to speak, hovering on the verge of instability, and when the piece is tapped they go over like a house of cards, and magnetism is acquired with a rush. Tapping always has some effect of the same kind, even though there has been no special adjustment of the field.

And other things besides vibration will act in a similar way, precipitating the break-up of molecular groups when the ties are already strained. Change of temperature will sometimes do it, or the application or change of mechanical strain. Suppose, for instance, that we apply pull to an iron wire while it hangs in a weak magnetic field, by making it carry a weight. The first time that we put on the weight, the magnetism of the wire at once increases, often very greatly, in consequence of the action I have just described (Fig. 13). The molecules have been on the verge of turning, and the slight strain caused by the weight is enough to make them go. Remove the weight, and there is only a comparatively small change in the magnetism, for the greater part of the molecular turning that was done when the weight was put on is not undone when it is taken off. Reapply the weight, and you find again but little change, though there are still traces of the kind of action which the first application brought about. That is to say, there are some groups of molecules which, though they were not broken up in the first application of the weight, yield now, because they have lost the support they then obtained from neighbours that have now entered into new combinations. Indeed, this kind of action may often be traced, always diminishing in amount, during several successive applications and removals of the load (see Fig. 13), and it is only when the process of loading has been many times repeated that the magnetic change brought about by loading is just opposite to the magnetic change brought about by unloading.

Whenever, indeed, we are observing the effects of an alteration of physical condition on the magnetism of iron, we have to distinguish between the primitive effect, which is often very great and is not reversible, and the ultimate effect, which is seen only after the molecular structure has become somewhat settled through many repetitions of the process. Experiments on the effects of temperature, of strain, and so forth, have long ago shown this distinction to be exceedingly important: the molecular theory makes it perfectly intelligible.

Further, the theory makes plain another curious result of experiment. When we have loaded and unloaded the iron wire many times over, so that the effect is no longer complicated by the primitive action I have just described, we still find that the magnetic changes which occur while the load is being put on are not simply undone, step by step, while the load is being taken off. Let the whole load be divided into several parts, and you will see that the magnetism has two different values, in going up and in coming down, for one and the same intermediate value of the load. The changes of magnetism lag behind the changes of load: in other words, there is *hysteresis* in the relation of the magnetism to the load (Fig. 14). This is because some of the molecular groups are every time being broken up during the loading, and re-established during the unloading, and that, as we saw already, involves hysteresis. Consequently, too, each loading and unloading requires the expenditure of a small quantity of energy, which goes to heat the metal.

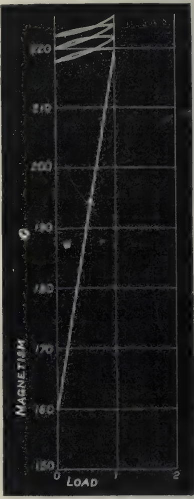

Fig. 13.—Effects of loading a soft iron wire in a constant field.

Moreover, a remarkably interesting conclusion follows. This hysteresis, and consequent dissipation of energy, will also happen though there be no magnetization of the piece as a whole: it depends on the fact that the molecules are magnets. Accordingly, we should expect to find, and experiment confirms this (see Phil. Trans., 1885, p. 614), that if the wire is loaded and unloaded, even when no magnetic field acts and there is no magnetism, its physical qualities which are changed by the load will change in a manner involving hysteresis. In particular, the length will be less for the same load during loading than during unloading, so that work may be wasted in every cycle of loads. There can be no such thing as perfect elasticity in a magnetizable metal, unless, indeed, the range of the strain is so very narrow that none of the molecules tumble through unstable states. This may have something to do with the fact, well known to engineers, that numerous repetitions of a straining action, so slight as to be safe enough in itself, have a dangerous effect on the structure of iron or steel.

Another thing on which the theory throws light is the phenomenon of time-lag in magnetization. When a piece of iron is put into a steady magnetic field, it does not take instantly all the magnetism that it will take if time be allowed. There is a gradual creeping up of the magnetism, which is most noticeable when the field is weak and when the iron is thick. If you will watch the manner in which a group of little magnets breaks up when a magnetic force is applied to it, you will see that the process is one that takes time. The first molecule to yield is some outlying one which is comparatively unattached—as we may take the surface molecules in the piece of iron to be. It falls over, and then its neighbours, weakened by the loss of its support, follow suit, and gradually the disturbance propagates itself from molecule to molecule throughout the group. In a very thin piece of iron—a fine wire, for instance—there are so many surface molecules, in comparison with the whole number, and consequently so many points which may become origins of disturbance, that the breaking up of the molecular communities is too soon over to allow much of this kind of lagging to be noticed.

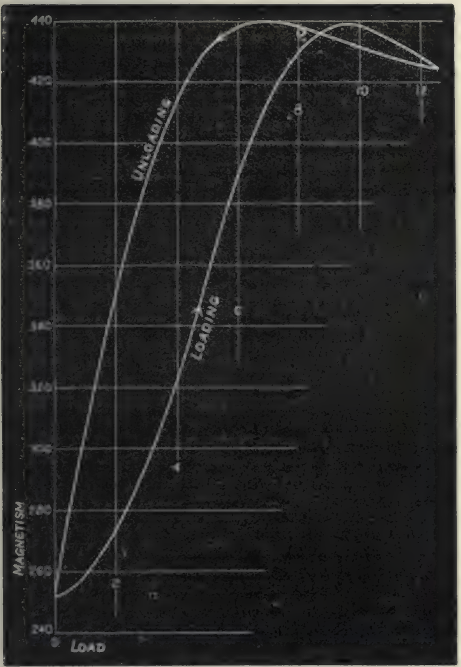

FIG. 14.—Cycle of loading and unloading.

Effects of temperature, again, may be interpreted by help of the molecular theory. When iron or nickel or cobalt is heated in a weak magnetic field, its susceptibility to magnetic induction is observed to increase, until a stage is reached, at a rather high temperature, when the magnetic quality vanishes almost suddenly and almost completely. Fig. 15, from one of Hopkinson's papers, shows what is observed as the temperature of a piece of steel is gradually raised. The sudden loss of magnetic quality occurs when the metal has become red-hot; the magnetic quality is recovered when it cools again sufficiently to cease to glow. Now, as regards the first effect—the increase of susceptibility with increase of temperature—I think that is a consequence of two independent effects of heating. The structure is expanded, so that the molecular centres lie further apart. But the freedom with which the molecules obey the direction of any applied magnetic force is increased not by that only, but perhaps even more by their being thrown into vibration. When the field is weak, heating consequently assists magnetization, sometimes very greatly, by hastening the passage from stage *a* to stage *b* of the magnetizing process. And it is at least a conjecture worth consideration whether the sudden loss of magnetic quality at a higher temperature is not due to the vibrations becoming so violent as to set the molecules spinning, when, of course, their polarity would be of no avail to produce magnetization. We know, at all events, that when the change from the magnetic to the non-magnetic state occurs, there is a profound molecular change, and heat is absorbed which is given out again when the reverse change takes place. In cooling from a red heat, the iron actually extends at the moment when this change takes place (as was shown by Gore), and so much heat is given out that (as Barrett observed) it reglows, becoming brightly red, though, just before the change, it had cooled so far as to be quite dull. [Experiment, exhibiting retraction and re-glow in cooling, shown by means of a long iron wire, heated to redness by the electric current.] The changes which occur in iron and steel about the temperature of redness are very complex, and I refer to this as only one possible direction in which a key to them may be sought. Perhaps the full explanation belongs as much to chemistry as to physics.

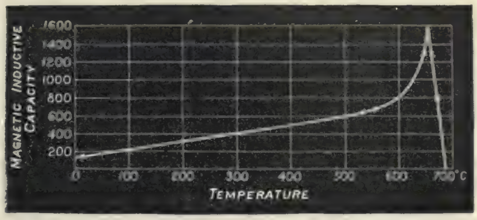

FIG. 15.—Relation of magnetic inductive capacity to temperature in hard steel (Hopkinson).

An interesting illustration of the use of these models has reached me, only to-day, from New York. In a paper just published in the *Electrical World* (reprinted in the *Electrician* for May 29, 1891), Mr. Arthur Hoopes supports the theory I have laid before you by giving curves which show the connection, experimentally found by him, between the resultant polarity of a group of little pivoted magnets and the strength of the magnetic field, when the field is applied, removed, reversed, and so on. I shall draw these curves on the screen, and rough as they are, in consequence of the limited number of magnets, you see that they succeed remarkably well in reproducing the features which we know the curves for solid iron to possess.

It may, perhaps, be fairly claimed that the models whose behaviour we have been considering have a wider application in physics than merely to elucidate magnetic processes. The molecules of bodies may have polarity which is not magnetic at all—polarity, for instance, due to static electrification—under which they group themselves in stable forms, so that energy is dissipated whenever these are broken up and rearranged. When we strain a solid body beyond its limit of elasticity, we expend work irrecoverably in overcoming, as it were, internal friction. What is this internal friction due to but the breaking and making of molecular ties? And if internal friction, why not also the surface friction which causes work to be spent when one body rubs upon another? In a highly suggestive passage of one of his writings,[1] Clerk Maxwell threw out the hint that many of the irreversible processes of physics are due to the breaking up and reconstruction of molecular groups. The models help us to realize Maxwell's notion, and, in studying them to-night, I think we may claim to have been going a step or two forward where that great leader pointed the way.

THE SUN'S MOTION IN SPACE.

SCIENCE needed two thousand years to disentangle the earth's orbital movement from the revolutions of the other planets, and the incomparably more arduous problem of distinguishing the solar share in the confused multitude of stellar displacements first presented itself as possibly tractable little more than a century ago. In the lack for it as yet of a definite solution there is, then, no ground for surprise, but much for satisfaction in the large measure of success attending the strenuous attacks of which it has so often been made the object.

Approximately correct knowledge as to the direction and velocity of the sun's translation is indispensable to a profitable study of sidereal construction; but apart from some acquaintance with the nature of sidereal construction, it is difficult, if not impossible, of attainment. One, in fact, presupposes the other. To separate a common element of motion from the heterogeneous shiftings upon the sphere of three or four thousand stars is a task practicable only under certain conditions. To begin with, the proper motions investigated must be established with *general* exactitude. The errors inevitably affecting them must be such as pretty nearly, in the total upshot, to neutralize one another. For should they run mainly in one direction, the result will be falsified in a degree enormously disproportionate to their magnitude. The adoption, for instance, of a system of declinations as much as 1″ of arc astray, might displace to the extent of 10° north or south the point fixed upon as the apex of the sun's way (see L. Boss, *Astr. Jour.*, No. 213). Risks on this score, however, will become less formidable with the further advance of practical astronomy along a track definable as an asymptote to the curve of ideal perfection.

Besides this obstacle to be overcome, there is another which it will soon be possible to evade. Hitherto, inquiries into the solar movement have been hampered by the necessity for preliminary assumptions of some kind as to the relative distances of classes of stars. But all such assumptions, especially when applied to selected lists, are highly insecure; and any fabric reared upon them must be considered to stand upon treacherous ground. The spectrographic method, however, here fortunately comes into play. "Proper motions" are only angular velocities. They tell nothing as to the value of the perspective element they may be supposed to include, or as to the real rate of going of the bodies they are attributed to, until the size of the sphere upon which they are measured has been otherwise ascertained. But the displacements of lines in stellar spectra give directly the actual velocities relative to the earth of the observed stars. The question of their distances is, therefore, at once eliminated. Now the radial component of stellar motion is mixed up, precisely in the same way as the tangential component, with the solar movement; and since complete knowledge of it, in a sufficient number of cases, is rapidly becoming accessible, while knowledge of tangential velocity must for a long time remain partial or uncertain, the advantage of replacing the discussion of proper motions by that of motions in line of sight is obvious and immediate. And the admirable work carried on at Potsdam during the last three years will soon afford the means of doing so in the first, if only a preliminary investigation of the solar translation based upon measurements of photographed stellar spectra.

The difficulties, then, caused either by inaccuracies in star-catalogues or by ignorance of star-distances, may be overcome; but there is a third, impossible at present to be surmounted, and not without misgiving to be passed by. All inquiries upon the subject of the advance of our system through space start with an hypothesis most unlikely to be true. The method uniformly adopted in them—and no other is available—is to treat the *inherent* motions of the stars (their so-called *motus peculiares*) as pursued indifferently in all directions. The steady drift extricable from them by rules founded upon the science of probabilities is presumed to be solar motion visually transferred to them in proportions varying with their remoteness in space, and their situations on the sphere. If this presumption be in any degree baseless, the result of the inquiry is *pro tanto* falsified. Unless the deviations from the parallactic line of the stellar motions balance one another on the whole, their discussion may easily be as fruitless as that of observations tainted with systematic errors. It is scarcely, however, doubtful that law, and not chance, governs the sidereal revolutions. The point open to question is whether the workings of law may not be so exceedingly intricate as to produce a grand sum-total of results which, from the geometrical side, may justifiably be regarded as casual.

The search for evidence of a general plan in the wanderings of the stars over the face of the sky has so far proved fruitless. Local concert can be traced, but no widely-diffused preference for one direction over any other makes itself definitely felt. Some regard, nevertheless, *must* be paid by them to the plane of the Milky Way; since it is altogether incredible that the actual construction of the heavens is without dependence upon the method of their revolutions.

The apparent anomaly vanishes upon the consideration of the profundities of space and time in which the fundamental design of the sidereal universe lies buried. Its composition out of an indefinite number of partial systems is more than probable; but the inconceivable leisureliness with which their mutual relations develop renders the harmony of those relations inappreciable by short-lived terrestrial denizens. "Proper motions," if this be so, are of a subordinate kind; they are indexes simply to the mechanism of particular aggregations, and have no definable connection with the mechanism of the whole. No considerable error may then be involved in treating them, for purposes of calculation, as indifferently directed; and the elicited solar movement may genuinely represent the displacement of our system relative to its more immediate stellar environment. This is perhaps the utmost to be hoped for until sidereal astronomy has reached another stadium of progress.

Unless, indeed, effect should be given to Clerk Maxwell's suggestion for deriving the absolute longitude of the solar apex from observations of the eclipses of Jupiter's satellites (Proc. Roy. Soc., vol. xxx. p. 109). But this is far from likely. In the first place, the revolutions of the Jovian system cannot be predicted with anything like the required accuracy. In the second place, there is no certainty that the postulated phenomena have any real existence. If, however, it be safe to assume that the solar system, cutting its way through space, virtually raises an ethereal counter-current, and if it be further granted that light travels faster *with* than *against* such a current, then indeed it becomes speculatively possible, through slight alternate accelerations and retardations of eclipses taking place respectively ahead of and in the wake of the sun, to determine his absolute path in space as projected upon the ecliptic. That is to say, the longitude of the apex could be deduced together with the resolved part of the solar velocity; the latitude of the apex, as well as the component of velocity perpendicular to the plane of the ecliptic, remaining, however, unknown.

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[1] "Encyc. Brit.," Art. "Constitution of Bodies."

The beaten track, meanwhile, has conducted two recent inquirers to results of some interest. The chief aim of each was the detection of systematic peculiarities in the motions of stellar assemblages after the subtraction from them of their common perspective element. By varying the materials and method of analysis, Prof. Lewis Boss, Director of the Albany Observatory, hopes that corresponding variations in the upshot may betray a significant character. Thus, if stars selected on different principles give notably and consistently different results, the cause of the difference may with some show of reason be supposed to reside in specialities of movement appertaining to the several groups. Prof. Boss broke ground in this direction by investigating 284 proper motions, few of which had been similarly employed before (*Astr. Jour.*, No. 213). They were all taken from an equatorial zone 4° 20′ in breadth, with a mean declination of +3°, observed at Albany for the catalogue of the Astronomische Gesellschaft, and furnished data accordingly for a virtually independent research of a somewhat distinctive kind. It was carried out to three separate conclusions. Setting aside five stars with secular movements ranging above 100″, Prof. Boss divided the 279 left available into two sets—one of 135 stars brighter, the other of 144 stars fainter, than the eighth magnitude. The first collection gave for the goal of solar translation a point about 4° north of α Lyræ, in R.A. 280°, Decl. + 43°; the second, one some thirty-seven minutes of time to the west of δ Cygni, in R.A. 286°, Decl. + 45°. For a third and final solution, twenty-six stars moving 40″–100″ were rejected, and the remaining 253 classed in a single series. The upshot of their discussion was to shift the apex of movement to R.A. 289°, Decl. + 51°. So far as the difference from the previous pair of results is capable of interpretation, it would seem to imply a predominant set towards the north-east of the twenty-six swifter motions subsequently dismissed as prejudicial, but in truth the data employed were not accurate enough to warrant so definite an inference. The Albany proper motions, as Prof. Boss was careful to explain, depend for the most part upon the right ascensions of Bessel's and Lalande's zones, and are hence subject to large errors. Their study must be regarded as suggestive rather than decisive.

A better quality and a larger quantity of material was disposed of by the latest and perhaps the most laborious investigator of this intricate problem. M. Oscar Stumpe, of Bonn (*Astr. Nach.*, Nos. 2999, 3000) took his stars, to the number of 1054, from various quarters, if chiefly from Auwers's and Argelander's lists, critically testing, however, the movement attributed to each of not less than 16″ a century. This he fixed as the limit of secure determination, unless for stars observed with exceptional constancy and care. His discussion of them is instructive in more ways than one. Adopting, the additional computative burden imposed by it notwithstanding, Schönfeld's modification of Airy's formulæ, he introduced into his equations a fifth unknown quantity expressive of a possible stellar drift in galactic longitude. A negative result was obtained. No symptom came to light of "rotation" in the plane of the Milky Way.

M. Stumpe's intrepid industry was further shown in his disregard of customary "scamping" subterfuges. Expedients for abbreviation vainly spread their allurements; every one of his 2108 equations was separately and resolutely solved. A more important innovation was his substitution of proper motion for magnitude as a criterion of remoteness. Dividing his stars on this principle into four groups, he obtained an apex for the sun's translation corresponding to each as follows:—

Group.	Number of included stars.	Proper motion.	Apex.	
I. ...	551 ...	0″·16 to 0″·32 ...	R.A. 287°·4	Decl. + 42°
II. ...	340 ...	0·32 to 0·64 ...	,, 279·7	,, 40·5
III. ...	105 ...	0·64 to 1·28 ...	,, 287·9	,, 32·1
IV. ...	58 ...	1·28 and upwards	,, 285·2	,, 30·4

Here, again, we find a marked and progressive descent of the apex towards the equator with the increasing swiftness of the objects serving for its determination, leading to the suspicion that the most northerly may be the most genuine position, because the one least affected by stellar individualities of movement. By nearly all recent investigations, moreover, the solar *point de mire* has been placed considerably further to the east and nearer to the Milky Way, than seemed admissible to their predecessors; so that the constellation Lyra may now be said to have a stronger claim than Hercules to include it; and the necessity has almost disappeared for attributing to the solar orbit a high inclination to the medial galactic plane.

From both the Albany and the Bonn discussions, there emerged with singular clearness a highly significant relation. The mean magnitudes of the two groups into which Prof. Boss divided his 279 stars, were respectively 6·6 and 8·6, the corresponding mean proper motions 21″·9 and 20″·9. In other words, a set of stars on the whole six times brighter than another set owned a scarcely larger sum-total of apparent displacement. And that this approximate equality of movement really denoted approximate equality of mean distance was made manifest by the further circumstance that the secular journey of the sun proved to subtend nearly the same angle whichever of the groups was made the standpoint for its survey. Indeed, the fainter collection actually gave the larger angle (13″·73 as against 12″·39), and so far an indication that the stars composing it were, on an average, nearer to the earth than the much brighter ones considered apart.

A result similar in character was reached by M. Stumpe. Between the mobility of his star groups, and the values derived from them for the angular movement of the sun, the conformity proved so close as materially to strengthen the inference that apparent movement measures real distance. The mean brilliancy of his classified stars seemed, on the contrary, quite independent of their mobility. Indeed, its changes tended in an opposite direction. The mean magnitude of the slowest group was 6·0, of the swiftest 6·5, of the intermediate pair 6·7 and 6·1. And these are not isolated facts. Comparisons of the same kind, and leading to identical conclusions, were made by Prof. Eastman at Washington in 1889 (Phil. Society Bulletin, vol. xi. p. 143; Proceedings Amer. Association, 1889, p. 71).

What meaning can we attribute to them? Uncritically considered, they seem to assert two things, one reasonable, the other palpably absurd. The first—that the average angular velocity of the stars varies inversely with their distance from ourselves—few will be disposed to doubt; the second—that their average apparent lustre has nothing to do with greater or less remoteness—few will be disposed to admit. But, in order to interpret truly, well-ascertained if unexpected relationships, we must remember that the sensibly moving stars used to determine the solar translation are chosen from a multitude sensibly fixed; and that the proportion of stationary to travelling stars rises rapidly with descent down the scale of magnitude. Hence a mean struck in disregard of the zeros, is totally misleading; while the account is no sooner made exhaustive than its anomalous character becomes largely modified. Yet it does not wholly disappear. There is some warrant for it in nature. And its warrant may perhaps consist in a preponderance, among suns endowed with high *physical* speed, of small, or slightly luminous, over powerfully radiative bodies. Why this should be so, it would be futile, even by conjecture, to attempt to explain.

A. M. CLERKE.

NOTES.

THE respect in which science is held in Germany was strikingly displayed on Tuesday, when Prof. Virchow celebrated his seventieth birthday. The occasion was regarded as one of national importance, and much honour was done to the investigator who, in the course of his great career, has given a fresh impetus to so many departments of research. In the morning, congratulations were offered to him in the large hall of the Kaiserhof Hotel, Berlin. The room was crowded with professors, academicians, and men of science from all parts of Europe; and on a long table were innumerable presents, medals, diplomas, and addresses. Short speeches were delivered on behalf of a series of deputations, the first of which was headed by Dr. Bartsch, one of the chief officials of the Ministry. A deputation, consisting of the professors of the Medical Faculty of the University of Berlin, and headed by Prof. Hirsch, the Dean, was followed by another from the Berlin Academy of Science, for which Prof. von Helmholtz spoke. Dr. von Forckenbeck, the Burgomaster of Berlin, heading a deputation from the Municipality of the capital, presented Prof. Virchow with the freedom of the city, referring gratefully to all that he had done to improve the health of the community. An address and medal, sent by English scientific bodies, were presented by Dr. Simon and Mr. Horsley, and then came congratulatory addresses from the Medical Faculties of many foreign cities, including Amsterdam, Brussels, Stockholm, St. Petersburg, Moscow, Pavia, and Tokio. The Virchow gold medal, for which contributions had been sent from all sections of the medical world, was presented by Prof. Waldeyer. Frau Virchow received a silver replica, and bronze copies were given to the other members of the family and to the scientific bodies which had subscribed for the medal. In the afternoon, a second meeting was held in the large hall of the Pathological Institute, where, as the Berlin correspondent of the *Times* says, "an almost endless procession of learned bodies and other corporations, presenting gifts and addresses, defiled before Prof. Virchow." The day's proceedings lasted from 10 a.m. to 4 p.m.; but it was noted, we are glad to say, that Prof. Virchow "seemed in no way fatigued by his exertions." More speeches were delivered in the evening, when a "Commers," or reunion, of his friends and admirers was held in Kroll's Theatre.

THE ordinary general meeting of the Institution of Mechanical Engineers will be held on Wednesday evening, October 28, and Thursday evening, October 29, at 25 Great George Street, Westminster. The chair will be taken at half-past seven p.m. on each evening by the President, Mr. Joseph Tomlinson. The ballot lists for the election of new members, associates, and graduates having been previously opened by the Council, the names of those elected will be announced to the meeting. The nomination of officers for election at the next annual general meeting will take place. The following papers will be read and discussed, as far as time permits:—On some details in the construction of modern Lancashire boilers, by Mr. Samuel Boswell (Wednesday); First Report to the Alloys Research Committee, by Prof. W. C. Roberts-Austen, C.B., F.R.S. (Thursday).

THE anniversary meeting of the Mineralogical Society will be held in the apartments of the Geological Society, Burlington House, on Tuesday, November 10, at 8 p.m.

THE International Congress of Analytical Chemists and Microscopists met at Vienna on October 12 and 13. The subject discussed was the adulteration of food-stuffs.

GREAT preparations are being made for the meeting of the Australasian Association for the Advancement of Science which is to be held at Hobart, Tasmania, in January next. It is expected that the meeting will be most successful. The members of the Royal Society of Tasmania are congratulating themselves that Mr. Giffen, the eminent statistician and political economist, proposes to attend the meeting and to read a paper. His Excellency Sir R. G. C. Hamilton, who will preside, tried some time ago to secure the presence of Prof. Huxley also. Prof. Huxley replied that he had pleasant recollections of Tasmania as it was forty-three years ago, and it would have interested him very much to revisit the colony and compare the present with the past, but he regretted that the state of his health prevented him from accepting the invitation.

ONE of the last surviving pupils of Dalton died at Bolton on October 6. Mr. William B. Watson was born at Bolton in January 1812, and educated at the local grammar-school. He afterwards studied for some years under Dalton at Manchester, and became so devoted to his teacher that he was chosen to help in the nursing of Dr. Dalton during the illness following his first paralytic seizure. Mr. Watson also assisted in many of Dalton's researches, and is mentioned by name in his papers in the Philosophical Transactions on the composition of the atmosphere as "an ingenious pupil of mine, Mr. William Barnett Watson." Mr. Watson had a wonderful store of anecdotes about his old master, and used to speak with pride of the great care he took in all his work. As an instance may be mentioned the pains he took to compensate for his colour-blindness. Dalton used to say that the bloom on a maiden's cheek and the colour of a faded green table-cloth seemed to him one and the same, and that he could only distinguish between the fruit and leaves on an apple-tree by their difference in shape. Dalton had a book containing different colours of floss silk, and below these he carefully noted the names given to them by non-colour-blind people, adding what the colour appeared to him to be. Careful methods such as these enabled him generally to give an accurate description of the colour of a precipitate. Mr. Watson carried on, together with his elder brother, Mr. H. H. Watson, a very extensive practice as an analytical chemist, and was much consulted in legal and commercial cases.

THE death of Mr. Charles Smith Wilkinson, the Government Geologist of New South Wales, will be felt as a great loss, especially in his own colony. His enthusiasm in the cause of geological science, his extensive knowledge of the geological features of Eastern Australia, and his admirable personal qualities had made him greatly valued. Mr. Wilkinson was an original member of the Linnean Society of New South Wales, and president of that Society in the years 1883 and 1884. His death, which took place at the age of forty-seven, on the 26th of August, was announced to the Society on the evening of the same day.

La Nature announces the death of Prof. Edouard Lucas, who presided over the Sections of Mathematics and Astronomy at the recent meeting, at Marseilles, of the French Association for the Advancement of Science. A pile of plates fell one day after dinner while he was at Marseilles, and he happened to be struck in the cheek by a fragment of the broken earthenware. The hurt became more and more troublesome, and after his return to Paris he died of erysipelas. M. Lucas was forty-nine years of age. He was a brilliant lecturer, and the author of several valuable books, the most important of which is his "Récréations Mathématiques."

APPLICATION has been made for 20,000 square feet of space for the electrical display from Great Britain at the "World's Fair" at Chicago. *Electricity*, the new weekly journal published at Chicago, remarks that this application should "set at rest all doubts in regard to the extent of the exhibit to be made by British manufacturers of electrical apparatus."

MR. C. E. KELWAY is now showing at the Royal Naval Exhibition an invention which promises to be of great practical value. It consists of an apparatus for marine and general electrical signalling. A number of electric incandescent lamps are placed in a suitable frame, from which insulated wires are led to a key-board, similar to those used in typewriters, or compound-switch. A key is appropriated for each letter of the alphabet and for numerals. On this key being depressed the electric current is switched on to the lamps representing the corresponding letter, which is at once shown to the observer. On the pressure being removed the lights disappear, and the next letter, or numeral, is in like manner shown, the words being spelt out at a rate more quickly than by the Morse system. Mr. Kelway claims that the applications to which this invention can be put are numerous. It might, he thinks, be of great service in naval tactics, and prove invaluable for military purposes. He also points out that it would enable mercantile vessels to communicate readily with each other and with the shore.

A CORRESPONDENT asks whether there are any firms which supply magic lantern slides dealing with geological subjects.

THE marine laboratory of biology and zoology, which is to be instituted at Bergen next year, will be open to any foreign investigators who may desire to study the marine fauna of that part of Scandinavia.

THE complete list of subscribers to the memorial to Bishop Berkeley, which has just been issued, contains the names of Profs. Huxley and Tyndall, in company with the Archbishop of Dublin and a number of bishops and deans. Mr. Gladstone and Mr. Balfour meet together in the same list. The memorial is a beautiful recumbent figure by Mr. Bruce Joy, R.A., which has been placed in Cloyne Cathedral. The inscription to be placed on the monument has not yet been announced.

THE Sociedad Cientifica "Antonio Alzate," of Mexico, who have lately moved into new quarters, have just resolved to throw open their scientific library to the general public. They are appealing on this ground to all foreign professors and scientific authors to send copies of their works to the library.

THE Engineers' and Architects' Institute of Vienna have resolved to petition the Austrian Government that engineer attachés should in future be appointed to the embassies and legations in London, Berlin, Paris, St. Petersburg, Rome, Washington, and to one Oriental city to be hereafter selected.

THE Royal Horticultural Society has issued a list of fruits which might be profitably cultivated by cottagers and small farmers in this country. The list (to which are added notes on planting, pruning, and manuring) ought to be widely distributed. It contains all the information that is really necessary for the development of a most important industry.

ACCORDING to a telegram sent from San Francisco, a severe shock of earthquake was felt there on October 11, but no damage was done. At Napa, California, where a heavy shock was experienced, the chimneys were thrown to the ground, and several buildings were shattered. The State Insane Asylum is reported to have been damaged, fissures being made in the walls. The inmates were seized with panic.

WE take from *La Nature* of the 3rd inst. the following particulars respecting the destructive cyclone which visited Martinique on the 28th of August last. The curve of a Richard barograph shows that the barometer commenced to fall about 2 p.m., when it stood at 29·92 inches, while between 7 and 8 p.m. it fell from 29·72 inches to 28·70 inches. The wind at this time, too, reached its greatest violence, and continued with hurricane force for several hours, passing alternately from N.E. to South. The recovery of the barometric pressure was equally rapid, the reading being about 29·70 inches before 10 p.m. M. Sully, of Saint Pierre, writes that the lightning was constant, with varying intensity before and after the passage of the centre. The sound of the thunder was scarcely perceptible, owing to the howling of the wind and the noise caused by the falling roofs and houses. Globular lightning was seen on all sides during the hurricane; the country folks speak of globes of fire which traversed the air for several minutes, and burst about two feet above the ground. All the towns and villages were greatly damaged, the crops destroyed, and that usually verdant country presented the appearance of the depth of the most severe winter. The deaths are said to be 420 in number.

IN the review of September in the U.S. Pilot Chart, it is pointed out that the month was unusually stormy on the North Atlantic, as indicated by the storm tracks plotted on the chart. Two of these tracks, however, represent August storms, one of them being the track of the Martinique hurricane, and another the track of the hurricane that passed east of Bermuda on August 27. The Martinique hurricane, it appears, moved west-north-west along a somewhat irregular track, crossing over Puerto Rico, Turk's Island, Crooked Island, and lower Florida, finally dying out in the north-eastern Gulf. This unusual course makes it of special interest, and its failure to recurve seems to have been due, possibly, to the opposition of the Bermuda hurricane, in a manner similar to the deflection towards Vera Cruz of the Cuban hurricane of September 1888. The Bermuda hurricane appears to have originated about 300 miles S.W. of the Cape Verde Islands on August 19.

THE correspondent of the *Times* at Alexandria telegraphed on October 11 that three colossal statues, ten feet high, of rose granite, had just been found at Aboukir, a few feet below the surface. The discovery was made from indications furnished to the Government by a local investigator, Daninos Pasha. The first two represent in one group Rameses II. and Queen Hentmara seated on the same throne. This is unique among Egyptian statues. The third statue represents Rameses standing upright in military attire, a sceptre in his hand and a crown upon his head. Both bear hieroglyphic inscriptions, and both have been thrown from their pedestals face downwards. Their site is on the ancient Cape Zephyrium, near the remains of the Temple of Venus at Arsinoe. Relics of the early Christians have been found in the same locality.

WE learn from the *Brighton Herald* that a discovery full of interest to archæologists has been made in Sussex. During some excavations near the depot of the Artillery Volunteers at Southover, Lewes, the workmen uncovered as many as twenty-eight skeletons. They were all buried close to the surface, and within an area of about 130 feet by 50 feet. As there were skeletons of women as well as of men, it is concluded that the site was not that of a battle-field, but of a place of burial. A similar find was made in 1830 at Malling Hill, which is not far distant. The remains now found were accompanied by a large number of weapons and ornaments, the characteristic features of which point to the fact of their being Anglo-Saxon. The skeletons have been reinterred, but the weapons and other articles have been placed in the museum of the Sussex Archæological Society at Lewes Castle.

MR. CLEMENT L. WALKER, while carrying on geological work in South-Western New Mexico, has also been pursuing archæological researches in that most interesting region during the last two years. He proposes to publish a detailed account of his investigations, and in the meantime he briefly records some of them in the August number of the *American Naturalist*, On the east, west, and middle branches of the Gila River, in the Mogollon Mountains, there is an extremely rough, wild, and broken tract; and here, in the rugged cliffs, are found great numbers of ancient cliff-dwellings. Mr. Webster devoted considerable time to the study of these dwellings, making plans and sketches, and copying the drawings of many of the more interesting and extensive hieroglyphics painted on the rocks. One of these ancient pueblos of the cliff-dwellers is situated in a lofty cliff which forms the side of a deep, narrow cañon extending out from the west branch of the Gila. This cliff-dwellers' village is in a fine state of preservation, and consists of upwards of twenty-eight rooms. Among the relics obtained in the rooms were specimens of several kinds of cloth, all made from the fibre of the Spanish dagger, matting of bear-grass. willow-work, sandals, cords of various sizes, feather-work, a ball and large skein of twine of the same material as the cloth, human and animal bones, stone utensils, great quantities of corn-cobs, corn, squash or pumpkin rinds, seeds and stems, corn-husks, beans, gourds, pottery, braided human hair of a brown colour, &c.; and last, but by no means least, a perfectly preserved cliff-dweller mummy. This was a mummy of a small child, with soft brown hair, similar to that found braided, only finer. It was closely wrapped in a considerable amount of two varieties of coarse cloth, woven from the fibre of the Spanish dagger, then wrapped in a large nicely-woven mat of bear-grass, and tied on by cords of the same material as the cloth to a small curiously-shaped board of cotton-wood.

SOME fine caves have lately been discovered near Southport, Tasmania. At the meeting of the Royal Society of Tasmania in June, an account of them was given by Mr. Morton, who had visited them. They are situated about four miles from Ida Bay, and a fairly good road leads to them. The entrance is through a limestone formation. A strong stream flows along the floor of the chambers. The first chamber reached by Mr. Morton and those who accompanied him showed some fine stalactites, and along the floor some fine stalagmites were seen. On the lights carried by the party being extinguished, the ceiling and sides of the caves seemed studded with diamonds, an effect due to millions of glow-worms hanging to the sides of the walls and from the ceilings. Further on, several chambers were explored, each revealing grander sights. The time at disposal being limited, the party had to return after traversing a distance of about three-quarters of a mile, but from what was observed the caves evidently extended a distance of three or four miles. The only living creatures seen were the glow-worms. These caves, under proper supervision, should become, Mr. Morton thinks, one of the great attractions of the south of Tasmania.

IN the Quarterly Statement of the Palestine Exploration Fund, it is announced that the first volume of the "Survey of Eastern Palestine," by Major Conder, has been issued to subscribers. It is accompanied by a map of the portion of country surveyed, special plans, and upwards of 350 drawings of ruins, tombs, dolmens, stone circles, inscriptions, &c. It is also announced that the new map of Palestine, so long in hand, is now ready. This map represents both sides of the Jordan, and extends from Baalbek in the north, to Kadesh Barnea in the south.

MR. E. R. MORSE contributes to the October number of the *Engineering Magazine*, a periodical issued at New York, an interesting paper on marble quarrying in the United States. Within recent years the use of American marble both in cemeteries and in buildings has become very extensive. Various foreign marbles, such as the African Red, Belgium Black, and Mexican Onyx, are employed in the interior decoration of buildings; but only Italian marble can be said to come really into competition with the American product, and the importation of this stone into the United States amounts only to about one-sixth of the value of the marble produced and sold at home. The quarrying of marble is practically limited at present to Tennessee, Georgia, Maryland, New York, Massachusetts, and Vermont. Large and valuable deposits may exist elsewhere, but the expense of testing deposits is so great, and the chances that the product of new quarries may prove unsaleable are so numerous, that Mr. Morse thinks that new marble fields are not likely to be developed soon.

THE "basking shark" (*Selache maxima*, L.) is apparently no very uncommon visitor in New Zealand waters. In the new volume of the Transactions and Proceedings of the New Zealand Institute, Mr. T. F. Cheeseman, Curator of the Auckland Museum, describes a specimen, over 34 feet long, which was stranded near the mouth of the Wade River. Mr. R. H. Shakspere, of Whangaparaoa, who saw the specimen very shortly after it was stranded, has informed Mr. Cheeseman that every spring several individuals of the same species can be seen near the entrance of the Wade River, and along the shores of Whangaparaoa Peninsula. He believes that they visit these localities in search of their food, which he thinks is composed of small *Medusæ* and other pelagic organisms. They can be easily recognized from their habit of swimming on the surface of the water, a portion of the back and the huge dorsal fin being usually exposed. It is from this circumstance, taken with the fact that their motions are very often slow and sluggish, that they have received the name of the "basking shark." They are easily approached and harpooned, and on the west coast of Ireland as many as five hundred have been taken in a single season. The liver often weighs as much as two tons, yielding six to eight barrels of oil. A few years ago, when sharks' oil was of greater value than it is at present, the oil from a single full-sized specimen would often realize from £40 to £50.

AT the meeting of the Linnean Society of New South Wales, on June 29, Mr. Froggatt exhibited some living beetles (fam. *Curculionidæ*), which afford a good example of protective coloration. They were found at Wellington, N.S.W., on the trunks of Kurrajong trees (*Sterculia*), the bark of which they resemble so closely in tint and general appearance that it was quite by accident Mr. Froggatt first recognized their true character.

MESSRS. GAUTHIER-VILLARS have sent us the "Annuaire" for 1891 of the Municipal Observatory of Montsouris. It contains, as usual, a great mass of carefully selected and well arranged information. We may especially note a collection of old meteorological observations made at Paris, and the following papers: Parisian climatology, by M. Léon Descroix; chemical analysis of the air and of waters, by M. Albert Lévy; thirteenth memoir on organic dust in the air and in waters, by Dr. Miquel.

MESSRS. G. L. ENGLISH AND CO., New York, have found it necessary to issue a supplement to the catalogue of minerals which they published in June 1890. So great has been the demand for minerals that they had three collectors at work during the summer—one in Europe, another in the south-western part of the United States and in Mexico, and a third in Colorado.

THE new number of the *Journal of Anatomy and Physiology* opens with some valuable notes by Dr. R. Havelock Charles, on the craniometry of some of the outcaste tribes of the

Panjab. He presents a series of tables drawn from the measurement of fifty skulls collected by him in the comparative anatomy museum of the Medical College, Lahore. These skulls are, in Dr. Charles's opinion, from individuals of aboriginal as distinguished from Aryan progeny, with the exception of certain megacephalic examples among the group of Mohammedan male types. In these exceptional cases descent may be derived from the more recent Mohammedan invaders, who were distinct both from the Aryan possessors on the one hand, and from the dispossessed aboriginal races on the other.

THE Department of Public Instruction in New South Wales has issued a second edition of "Wattles and Wattle-Barks," by J. H. Maiden. It appears as No. 6 of the Technical Education Series. The pamphlet is intended to supply Australian farmers, tanners, merchants, and others with authentic information in regard to the value of wattles. According to the author, the demand for good wattle-bark becomes greater every year, and the supply does not cope with it. The word "wattle," we may note, has become in Australia practically synonymous with "acacia."

AN interesting experiment has been lately made by M. Chabry, of the Société de Biologie, with regard to the pressure which can be produced by electrolytic generation of gas in a closed space. While the highest pressure before realized in this way was 447 atmospheres (Gassiot), M. Chabry has succeeded in getting as high as 1200; and the experiment was broken off merely because the manometer used got cracked (without explosion). The electrolyzed liquid was a 25 per cent. soda solution. Both electrodes were of iron: one was the hollow sphere in which the gas was collected; the other an inner concentric tube. The current had a strength of 1½ ampere, and was very constant during the experiment, which was merely one preliminary to a research in which very high pressures were desired.

THE first series of lectures given by the Sunday Lecture Society begins on Sunday afternoon, October 18, in St. George's Hall, Langham Place, at 4 p.m., when Sir James Crichton Browne, F.R.S., will lecture on "Brain Rust." Lectures will subsequently be given by Mr. Frank Kerslake, Mr. Walter L. Bicknell, Mr. W. E. Church, Prof. H. Marshall Ward, F.R.S., Mr. A. W. Clayden, and Sir Robert Ball, F.R.S.

AN important paper upon persulphates is contributed by Dr. Marshall, of Edinburgh, to the October number of the *Journal of the Chemical Society*. The anhydride of persulphuric acid, S_2O_7, was obtained by Berthelot in the year 1878, by subjecting a well-cooled mixture of sulphur dioxide and oxygen to the silent electrical discharge. He afterwards found that a substance possessing oxidizing properties, and which appeared to be persulphuric acid, was formed in solution during the electrolysis of fairly strong solutions of sulphuric acid; it appeared, in fact, to be identical with the substance obtained by dissolving his crystals of S_2O_7 in water. The anhydride does not dissolve in water without partial decomposition, a considerable proportion decomposing into sulphuric acid and oxygen, and hitherto no salts of persulphuric acid have been obtained in the solid state. Dr. Marshall has now succeeded in obtaining the potassium, ammonium, and barium salts in fine large crystals. During the course of an experiment in which an acid solution of potassium and cobalt sulphates was being electrolyzed in a divided cell, it was found that a quantity of small colourless crystals separated. A solution of these crystals in water gave only a faint precipitate with barium chloride, but on warming barium sulphate slowly separated and chlorine was evolved. The solution also liberated iodine from potassium iodide. The crystals were, in fact, potassium persulphate, KSO_4. It was next sought to prepare them from hydrogen potassium sulphate. A saturated solution of this salt was submitted to electrolysis in a similar apparatus, and at the end of two days a white crystalline deposit of potassium persulphate commenced to form. The crystals were from time to time removed until a considerable quantity of them had been accumulated. These, when recrystallized from hot water, yielded large tabular crystals, and sometimes long prisms when formed at the surface of the liquid. Analyses of pure samples yielded numbers agreeing perfectly with the formula KSO_4. From determinations of the conductivity of dilute solutions it would appear that the correct molecular formula is KSO_4 and not $K_2S_2O_8$. On ignition of the salt, oxygen and sulphuric anhydride are evolved and potassium sulphate is left. The crystals are not very soluble in water, 100 parts of water at 0° dissolving 1·77 part of KSO_4. The aqueous solution gradually decomposes, hydrogen potassium sulphate being formed and oxygen liberated. The pure freshly prepared solution is neutral to test paper. The solution yields no precipitate with any other salt by double decomposition, the persulphates of most other metals appearing to be more soluble than potassium persulphate. A solution of lead hydrate in potash yields a precipitate of lead peroxide on boiling. With silver nitrate no immediate precipitate is formed, but the liquid gradually acquires an inky appearance and after some time a black precipitate of silver peroxide, AgO, is deposited. It would appear that silver persulphate is dissolved by water. Fehling's solution gives a red precipitate of copper peroxide. Ferrous sulphate is rapidly oxidized to ferric with considerable rise of temperature. Organic colouring matters, such as litmus, are bleached. Alcohol is oxidized to aldehyde in presence of water, but absolute alcohol has no action on solid potassium persulphate. The pure crystals have a cooling saline taste, which leaves a peculiar after-taste. The impure salt evolves ozone slowly. Freshly prepared crystals have no odour, but after a time they emit a peculiar pungent odour quite different from that of ozone, and which appears to be due to persulphuric anhydride. When warmed with concentrated nitric or sulphuric acids the oxygen is liberated largely in the form of ozone. With hydrochloric acid chlorine is evolved. The ammonium salt NH_4SO_4 has been prepared in a similar manner; it crystallizes in long prisms and much resembles the potassium salt. The barium salt crystallizes in beautiful large interlocking prisms containing four molecules of water of crystallization.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mr. J. Barratt Lennard; a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Miss Corrie Chisholm; two Common Marmosets (*Hapale jacchus*) from South-east Brazil, presented by Mrs. Frederick Betts; two Bernicle Geese (*Bernicla leucopsis*), two Brent Geese (*Bernicla brenta*), European, presented by Mr. Cecil Smith; a Gamet (*Sula bassana*), British, presented by Dr. Davis; eleven Gold Pheasants (*Thaumalea picta* ♂s), two Amherst Pheasants (*Thaumalea amherstiæ* ♂ ♂), two Silver Pheasants (*Euplocamus nycthemerus* ♂ ♀) from China, a Common Pheasant (*Phasianus colchius* ♂), British, four Ruddy Sheldrakes (*Tadorna casarca*), European, presented by Mr. Edwin J. Poyser; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. F. Manners; a Macaque Monkey (*Macacus cynomolgus*) from India, deposited.

OUR ASTRONOMICAL COLUMN.

MEASUREMENTS OF LUNAR RADIANT HEAT.—Numerous measurements of lunar radiant heat have been made at Birr Castle Observatory by Lord Rosse and Dr. Copeland, and the results obtained have been published from time to time. During the total lunar eclipse of October 4, 1884, Dr. Otto Boeddicker, Lord Rosse's present assistant, carried out a series of observations for the purpose of testing the striking result previously arrived at by Dr. Copeland, viz., that "the maximum of heat seemed to occur somewhat before full moon." It was then found that "The heat as before diminished, and increased again nearly proportionally to the light, becoming inappreciable on reaching the limits of totality. The minimum of heat apparently fell later than that of illumination. But the most remarkable thing was that while during the short interval between the first contact with the penumbra and the commencement of total phase, all appreciable radiation vanished, between the end of total phase and the last contact with the penumbra, and even forty minutes later, the heat had not returned to the standard for full moon, being deficient by about 12 per cent." These facts are remarked upon by Lord Rosse in an introduction to a paper by Dr. Boeddicker, giving the results obtained during the lunar eclipse of January 28, 1888 (Transactions of the Royal Dublin Society, Series III., vol. iv., Part ix., 1891). The measurements of radiation were commenced about an hour before the first contact with the penumbra, and a decrease of heat seems even then to have set in. But excluding this diminution of heat exhibited by the curve connecting the observations, there is indisputable evidence that the decrease had definitely commenced about three minutes before the eclipse began, and probably fifteen minutes before. This indicates, therefore, that the terrestrial atmosphere extends to a height of not less than 190 miles, and intercepts the sun's rays before any part of the moon has entered the earth's shadow. In 1888, as in 1884, the anomaly of the heat not returning to its standard value even 1 hour 40 minutes after the last contact with the penumbra, was observed. Dr. Boeddicker enumerates the series of observations required to elucidate these interesting points, and hopes soon to publish some further results of his investigations.

Two New Variable Stars.—The Rev. T. E. Espin has found two new variable stars in Cygnus, viz. D.M. + 36°·3852 and D.M. + 49°·3239. They are both of a strong red colour. The first has a Type III. (Group II.) spectrum, and the second belongs to Type IV. (Group VI.).

A New Asteroid.—The asteroid observed by Dr. Palisa on August 12 turns out to be Medusa (149), as was suggested by Dr. Berberich. On this account, the asteroids from (313) to (318) must be numbered from (312) to (317), and the one discovered on September 24 by Charlois will be (318).

A New Comet.—A bright comet was discovered on October 2, by Mr. E. E. Barnard, at Lick Observatory, in R.A. 7h. 31m. 24s., and Decl. − 27° 54′. It was moving to the south-east.

THE IRON AND STEEL INSTITUTE.

THE autumn meeting of the Iron and Steel Institute was held on Tuesday the 6th inst. and Wednesday the 7th inst., under the presidency of Sir Frederick Abel. After the excitement of last year's meeting in the United States, the gathering of last week fell rather flat. As our readers are aware, it is the custom of this Society to hold two meetings each year—the first, in the spring, being in London, and the second, in the autumn, either in the provinces or abroad. This year it was proposed that Birmingham should be the place of meeting, but the great town of the Midlands does not appear to have responded to the overtures made, and, no other invitation being forthcoming, the Council was thrown back on the metropolis. In one point, at any rate, the meeting was a success, as on Tuesday a larger number of members travelled down to Woolwich, where a visit had been arranged to the Royal Arsenal, than perhaps have ever been got together before on an excursion.

The excursions are generally the leading feature of the autumn meetings, but there was but one organized for the meeting just past—namely, that to Woolwich Arsenal. The following is a list of the papers read:—On the constitution of ordnance factories, by Dr. William Anderson, F.R.S., Director-General of Ordnance Factories; on the measuring instruments used in the proof of guns and ammunition at the Royal Arsenal, Woolwich, by Captain Holden, R.A., Proof Officer at Woolwich; on the manufacture of continuous sheets of malleable iron and steel direct from fluid metal, by Sir Henry Bessemer, F.R.S.; on illustrations of progress in material for shipbuilding and engineering in the Royal Naval Exhibition, by W. H. White, C.B., Chief Constructor; on the forging press, by W. D. Allen, Sheffield; on an undescribed phenomenon in the fusion of mild steel, by F. J. R. Carulla, Derby; on the elimination of sulphur from pig-iron by J. Massenez, of Hoerde, Germany; on the Metallurgic Department, Sheffield Technical School, by B. H. Thwaite, Liverpool.

The first two papers were read at the Literary Institute, Woolwich. Dr. Anderson's contribution was taken first. It is a curious fact that the Director-General of Ordnance Factories, whose admirers used to claim, before he occupied his present position, that he was too scientific to be a successful business man, should have contributed what is perhaps the least scientific paper to be found within the Transactions of the Institute. The paper was what its title indicated, strictly a description of the constitution of the Royal Ordnance Factories. It told how they comprise the Laboratory, Gun Factory, and Carriage Department at Woolwich, the Gunpowder Factory at Waltham Abbey, and the Small Arms Factories at Enfield Lock and Birmingham. These establishments are, the author said, "supposed" to be worked on commercial principles. Dr. Anderson is an accurate and careful man, as has been proved by much good scientific work in the field of mechanical engineering which he has done, and there is much virtue in his "supposed." If ever a manufacturing establishment were worked with a view to profit after the manner of Woolwich Arsenal, the profits probably would be very small. The paper tells us that £400,000 is invested in stores, £557,945 in buildings, and £718,949 in machinery. By far the larger part of the work is done on the piece, or on the fellowship system. The number of hands employed is about 17,000, of which 13,000 are at Woolwich. In the financial year 1889-90, the value of completed work issued amounted to £2,259,126. The expenditure on all services, complete and incomplete, was £2,590,053, of which wages were responsible for £1,339,045, and materials for £1,005,224. The average wage earned per week per man and boy is 32*s.*, and about £19,000 a year is spent in medical attendance, which the men receive free.

Captain Holden's paper was on an interesting subject, but was far too brief to treat it in anything approaching an adequate manner. In addition to which illustrations are necessary to make clear the working of the various delicate instruments used in the measurement of the velocity of projectiles, but no wall diagrams were exhibited. It is true that some of the actual machines were shown, but these are a very poor substitute for sectional drawings, as one can see nothing but the outside. The Novez-Leurs chronoscope, Prof. Bashforth's chronograph, Schultz's revolving drum, together with the various modifications of it which have been introduced, were all briefly referred to. Most of these instruments are fairly well known, although not in general use. The Le Boulongé instrument, which is the one now universally used for determining the velocity of projectiles outside guns, was shown and its action illustrated. The author mentioned that when the Le Boulongé instrument was first introduced the highest normal muzzle velocities of guns were about 1000 feet per second. "Now," Captain Holden said, "they are double that amount; and it is probable they will reach 3000 feet per second." As an instance of the accuracy required in instruments of this nature, the author gave the following example: "The case of a shot whose mean velocity between two screens placed 180 feet apart is 1800 feet per second. A variation of one foot above or below 1800 feet per second is represented by a decrease or increase in time of only ·0005 of a second approximately." In order to work within such narrow limits the greatest care has to be taken to eliminate all sources of error in the instrument, and the precautions taken are briefly outlined in the paper.

After the reading of these two papers the members were conducted round the Arsenal, but such official wrath was threatened against any person who wrote for printing about anything he saw that we are too frightened to make further reference to this part of the proceedings.

On the second day of the meeting the members assembled at the Institution of Civil Engineers, Sir Frederick Abel, the President, again occupying the chair. The first paper taken was a contribution by Sir Henry Bessemer, in which he described an invention of his, devised nearly half a century ago. This consisted of the rolling of steel sheets direct from the molten metal as tapped from the furnace or converter. The process is simple in the extreme, and one can only marvel that the present complicated and costly methods should have stood so long, considering that Sir Henry Bessemer's patents have long since expired, and his direct process is open to anyone to adopt. The metal, as tapped from the furnace, in place of being run into ingots, to be afterwards rolled into slabs or billets, is just poured on to the top of a pair of water-cooled rolls placed with their axes in the same horizontal plane. The rolls are caused to revolve, and the molten metal finds its way down between the space left between them, and is thus rolled out into a continuous plate or sheet; the chill received in passing through the rolls being sufficient to solidify the metal. That the process is possible Sir Henry proved over forty years ago; that it may be made commercially successful appeared to be the unanimous opinion of the many competent critics who spoke in the discussion. Under these circumstances it would seem that the only reason why there should not be a radical change in the way of manufacturing steel plates is that the process is open to every one, and, as there are no patent rights to be acquired, it may be worth no one's while to go to the initial expenses of starting a new process just to show competitors how to do the same thing.

Mr. W. H. White's paper on the shipbuilding material at the Naval Exhibition was a useful and interesting contribution, although not so exhaustive as might have been desired. It would, however, be too much to expect so important a public servant as the Director of Naval Construction to devote his time to writing treatises for technical Societies. What Mr. White has written is of interest. He points out how the work of shipbuilding has been simplified and cheapened by the steel manufacturer, who now rolls many special sections, such as Z bars, channel bars, H bars, T bulbs, and angle bulbs, thus saving a vast amount of building up and riveting in the actual construction of the ship. The increase in the size of plates, both for ship and boiler work, was also pointed out by the author. Two specimens of boiler plate are shown in the Exhibition, which are both 1¼ in. thick and respectively 42 ft. long by 6¼ ft. wide, and 31 ft. long by 7½ ft. wide. Another way in which the steelmaker and founder has helped the shipbuilder is in producing complete parts of ships, such as stern frames and stems, especially the spur stems of war vessels, which necessarily have to be of massive construction. In old days, when such parts were made of wrought iron, the forging had to be machined to form the recesses or "rabbets" necessary for the attachment of plating. That was excessively costly work, and in the case of such heavy articles was most difficult to accomplish at all. With steel castings little or no machining is required. Mr. White exhibited a large hull diagram of a ram bow for a recent battle-ship. The part is made hollow, or rather recessed, and shelves are cast on to receive the plating of the decks, and the attachment of breast hooks, &c. The author also referred to the exhibits of armour plate made at the Exhibition, but the subject is too lengthy for us to go into here, excepting to say that nickel steel has been proved by test to show such good results for armour that some of the secondary armour plating for five first class battle-ships is now being made of that material.

Mr. W. D. Allen, in his paper, described a forging press, which, although it has been at work for some years at the Bessemer Works in Sheffield, is so ingenious, and so new to most people, that we shall attempt to describe it. The press has the appearance of a steam hammer, and, indeed, there is a steam cylinder at the top, just as in a hammer. The use of the steam, however, is only to raise the tup when the hydraulic pressure is released. The press consists of an anvil block below and a ram above, the work being in a vertical direction. The ram works in a hydraulic cylinder, and is carried through the top end of the latter in the shape of a stout shaft or shank, which may be described as a tail rod to the ram. Attached to this is the piston rod of the steam piston, the latter of course working in its own cylinder. The steam cylinder and hydraulic cylinder are therefore placed tandemwise, the latter being underneath. The hydraulic cylinder is supplied with water at pressure by a suitable pump, the barrel of the pump being in direct communication with the hydraulic cylinder, there being no valve of any kind between the two. If we have made our explanation clear, it will be seen that the ram will descend and ascend stroke for stroke with the pump plunger[1] (the same water flowing backwards and forwards continuously), it being remembered that the steam cylinder has always a tendency to lift the ram. Thus, upon the pump making a forward stroke, the water in its barrel is forced into the hydraulic cylinder; the ram is thus forced down, and gives the necessary squeeze to the work on the anvil. The pump plunger then starts on its return stroke, and so, by enlarging the space in the pump barrel, enables the hydraulic ram to rise and press the water out of the cylinder and back into the pump. The rising of the ram is caused by the lifting action of the steam under the piston; the latter, it will be remembered, being attached to the ram. Of course the water pressure is sufficient to overcome the steam pressure on the downward stroke. The chief use of this press is to produce work of any given thicknesses within the range of the machine. This end is attained by regulating the volume of water used. The action may be explained as follows. We will suppose, merely for simplicity sake,[1] the content of the pump barrel to be one cubic foot, and that of the hydraulic cylinder, when the ram is at the full extent of its stroke, to be two cubic feet. We will neglect the connecting pipe between the two, as that is not a variable and does not affect the principle. If there be admitted to the pump but one cubic foot of water as the plunger moves forward, it will drive all this water (omitting clearance) into the hydraulic cylinder, and the ram would therefore only descend one half its stroke. If the stroke were two feet the travel would be 12 inches, whilst there would be 12 inches of space between the anvil and the lower side of the squeezing tool on the end of the ram. Objects of 12 inches, or above 12 inches in thickness, could therefore be forged. If, however, an article 6 inches thick had to be worked, another half cubic foot of water would have to be admitted. As the pump barrel would only accommodate one cubic foot of water, the extra half cubic foot would remain permanently in the hydraulic cylinder, and the ram would therefore not go, by six inches, to the top of its stroke; in other words, the traverse of the ram would be carried six inches nearer the anvil. It will be remembered that the upward movement of the ram is effected by the steam cylinder, which is powerful enough to lift the dead weight of the ram, but is overcome by the hydraulic pressure. It will be seen that by regulating the volume of water in the machine, the ram—although always making the same length of stroke—can be kept working at any given distance from the anvil: the ram and pump-plunger making stroke for stroke as the water flows backwards and forwards between the barrel of the pump and hydraulic cylinder. The device is no less important than ingenious. In ordinary forging, reliance has to be placed for accuracy of work on the skill of the workman. It is surprising how near perfection a good forgeman will arrive by constant practice. Such men are necessarily scarce, and as a consequence very highly paid, but even the nearest approximation of eye and hastily applied callipers, with the chance of getting a little too much work on at the last minute, cannot equal the absolutely correct results of this automatic system. There is a very ingenious valve for regulating the admission of water to fine gradations, so as to get work accurately to gauge, but we have, perhaps, given enough description of mechanism for one article.

Mr. Carulla's paper was interesting and suggestive. He was engaged in melting Bessemer scrap in pots when a crucible gave way in the furnace just as fusion was nearly complete, the greater part of the contents flowing out into the fire. The melter was just bringing the crucible out, and, instead of finding an empty broken crucible in the tongs, he discovered a number of shells corresponding in shape with the pieces originally charged, but quite hollow. This was Mr. Carulla's unaccounted for phenomenon, upon which he invited an explanation. This discussion was not satisfactory, and it was evident that those who spoke had not prepared their ideas. This was not the fault of the speakers, but of the way in which the business of these meetings is carried on. The remark applies not only to the Iron and Steel Institute, but to most of the technical Societies of the same class. When a meeting is held, a mass of papers are brought forward and read more or less hurriedly, and members get up to make such remarks as may occur to them on the spur of the moment. It is needless to point out that no satisfactory discussion of matters involving scientific principles can be carried on in this way. Mr. Carulla's paper is, as we have said, suggestive, and a complete explanation of the facts he states would doubtless lead to most important discoveries in metallurgical science. In such cases as this we think it would be wise to read the paper and then postpone discussion until the next meeting; or, by preference, to have the paper printed in the Journal of Proceedings, and at a meeting subsequent to its appearance call for discussion. It would appear evident that the interior of the pieces of scrap had a lower melting-point than the exterior parts which formed the shells obtained, and the explanation of the variation in melting-point was the point requiring consideration. Liquation of the elements is naturally the first suggestion, but this only shifts the uncertainty, for liquation is itself an obscure matter. Mr. Snelus would explain the matter by decarbonization at the surface, which would render the interior parts more easily fusible. He had, in raking out a furnace, found pigs of which only the outer skin remained as metal, the case thus formed being filled with graphitic carbon. Mr. Galbraith attributed the phenomenon to the surface of the metal pieces having absorbed an infusible oxide when at a high temperature. There was, however, more in the circumstances described than the meeting was prepared to explain off hand, and it would be well if the discussion could be reopened at the spring meeting or brought on again by another paper.

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[1] There are actually two plungers, the pump being of the duplex type; but this is a detail which does not affect the principle.

[1] The press ram makes a stroke of 2⅞ inches, and its diameter is 30 inches. The total pressure at 3 tons per square inch would be 1700 tons.

The contribution of Mr. Massenez was in many respects the most valuable of the meeting. It is a pleasing thing to see a foreign steelmaker putting his experience so unreservedly at the disposal of his English fellow-workers, and the thanks of the Institute are doubly due to the author for his valuable and practical paper. There is also an economic lesson in this matter, for the apparatus described owed its introduction to the German colliers' great strike of two years ago. Since then there has not only been a diminution in the amount of coal wrought, but the quality has also fallen off, so that the proportion of sulphur in the coal has much increased. This necessitated a desulphurization process, the method of which forms the subject of the paper. Manganiferous molten pig, poor in sulphur, is added to sulphuretted pig iron, poor in manganese; the result being that the metal is desulphurized, and a manganese sulphide slag is formed. The mixer in which the process is carried on is a large vessel in appearance, to judge by the drawings shown, like a converter. The apparatus in use at Hoerde will hold seventy tons of molten pig, but it has been shown that a vessel of about twice the size would be advisable. Details of the working are given by the author, and will be of great use to steelmakers working with phosphoric pig. In the discussion which followed several speakers bore testimony to the value of the invention, Sir Lowthian Bell intimating that a saving of 2*s*. 4*d*. per ton could be made by this method over the process of re-melting pig in the cupola; a step which has to be taken when it is desirable to combine the product of different blast furnaces. In the large mixer, metal from two or more furnaces can be brought together.

The only remaining paper was a contribution by Mr. B. Thwaite, in which particulars were given of the metallurgical department of the Sheffield Technical School, which was read in brief abstract by one of the clerical staff; after which the meeting was brought to a conclusion by the usual votes of thanks.

CARL WILHELM VON NÄGELI.

THE death of Carl Wilhelm von Nägeli, on May 10, 1891, removes the last survivor of that distinguished group of botanists who, side by side with zoologists such as Schwann and Kölliker, laid, half a century ago, the foundations of modern histology. The career of Nägeli is of special interest for the history of botany. During a period of fifty years he held a leading position in the advance of the science; and, while his activity began in the early days of Schleiden's predominance, his most recent work is in touch with those latest developments of biology which are connected with the name of Weismann. His work reached every side of the science. Systematic botany, morphology, anatomy, chemical and physical physiology, the theory of heredity and descent, as well as histology, all bear lasting traces of his influence.

Nägeli was born on March 27, 1817, at Kilchberg, near Zürich, and was the son of a country doctor. As a child he was devoted to books, but he soon showed a taste for natural history, which appears to have been in some degree inspired by his sister. His education as a boy was begun at a private school, of which his father was one of the founders, and was completed at the Zürich Gymnasium, where he did well. He then matriculated at the recently-established University of Zürich, with the view of studying medicine. As a student, he is said to have been strongly influenced by the "Naturphilosophie," as taught by Oken. He soon lost his taste for medical studies, and, owing to his mother's influence, was allowed to migrate to Geneva, where he devoted himself to the study of botany under De Candolle.

Nägeli took his doctor's degree at Zürich in 1840; his dissertation on the Swiss species of *Cirsium* was dedicated to Oswald Heer, and was his first contribution to that minute investigation of species which formed so characteristic a part of his life's work.

Subsequently Nägeli spent a short time at Berlin, studying, among other things, the philosophy of Hegel. A metaphysical tendency marks his writings all through life, and indeed favourably distinguishes his work from that of many less cultivated scientific writers; but Nägeli, in one of his later papers, expressly denies that he was ever himself an Hegelian.

Nägeli's next migration was to Jena, and here he came under the influence of Schleiden, by whom he was initiated into microscopic work. It was not long before the association of these two great men bore fruit. In 1844 appeared the first number of the *Zeitschrift für Wissenschaftliche Botanik* under the editorship of Schleiden and Nägeli. The connection of the former with the new venture was only a nominal one, and, indeed, all the papers but two are the work of Nägeli himself. The influence of Schleiden however, is manifest throughout, sometimes in an injurious degree, though the independence of Nägeli gradually asserted itself. To this brilliant, though short-lived publication we shall return presently. In 1845 Nägeli married, and on his wedding tour he spent a long time on the south-west coast of England, and there collected much material for his important work on "Die neueren Algen-systeme," published in 1847.

On his return to the Continent he became a *Privatdocent* at Zürich and lecturer at the veterinary school, and soon afterwards he was appointed Professor Extraordinarius. In 1850 his association with Cramer, so fruitful of good work, began. His colleague says of this time: "Es war eine schöne zeit! da wurden nicht bloss Staubfäden gezählt und Blattformen beschrieben; es ging in die Tiefe, ans Mark des Lebens!" It was the microscopic practical work with Nägeli which made the deepest impression on his distinguished pupil; his lectures, though clear and full of matter, do not appear to have been specially brilliant, but he possessed the highest qualification of a teacher in being himself a great maker of knowledge.

After declining a "call" to Giessen, Nägeli in 1852 became Professor at Freiburg im Breisgau, where most of the work was done for the "Pflanzenphysiologische Untersuchungen," published in conjunction with Cramer in 1855–58. In 1855 Nägeli accepted the post of Professor of General Botany in the new Polytechnic at Zürich; his work at this time was hindered by the temporary failure of his eyesight, owing to too much microscopic work.

In 1857 Nägeli was summoned to the Professorship of Botany at Münich, where King Maximilian II. was striving to render his capital as distinguished in science as it already was in art. This post Nägeli continued to hold to the time of his death. At first somewhat distracted from his original work by practical duties in connection with the organization of the institute and gardens, Nägeli soon resumed his proper activity, and continued for thirty years more to produce a magnificent series of researches on the most varied subjects. Unfortunately, Nägeli's work was excessive, and from the age of sixty onwards, his health began to suffer, so that he was ultimately compelled to give up teaching. An attack of influenza during the epidemic of 1889-90 seriously shattered his already failing strength, and from the effects of this he never completely recovered. He lived long enough to celebrate in great honour the jubilee of his doctor's degree, and thus to look back on half a century of continuous work for the advancement of science, a retrospect such as few savants can have enjoyed.[1]

Without attempting to give an adequate account of Nägeli's scientific work, a task which would far exceed both the limits of this article and the powers of the writer, some idea may be given of the salient points in his career as an investigator.

Nägeli's first histological paper, so far as we are aware, is on the development of pollen (1841). This already marks a decided advance on Schleiden's theory of free-cell formation, for Nägeli maintains that the special mother-cells are not formed directly around a cytoblast (nucleus) but around the whole granular contents, in the middle of which a free cytoblast lies. It was long, however, before Nägeli completely freed himself from the influence of Schleiden's histological theories. It is interesting that in this paper he described and clearly figured the two nuclei in the pollen-grain of an Œnothera, though he did not know that this was a constant phenomenon. The importance of this observation was not appreciated until Elfving, Strasburger, and Guignard, investigated the subject in our own day.

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[1] The details of Nägeli's life are taken from the funeral address delivered by his colleague, Prof. Cramer, and published in the *Neue Zürcher Zeitung* for May 16, 1891.

Nägeli's "Botanische Beiträge" contributed to the volume of *Linnæa* for 1842, include some important papers. In those on the development of stomata and on cell-formation in the root-apex, he endeavoured to reconcile his own accurate observations with Schleidenian theories, and was thus led to oppose Unger, who had already recognized that vegetative cell-formation is a process of division. A paper on Fungi in the interior of cells is interesting, because the existence of such endophytic forms was at that time regarded as establishing a presumption in favour of spontaneous generation.

The *Zeitschrift für Wissenschaftliche Botanik*, 1844–46, is a very remarkable publication. It never got beyond its first volume, but it may be doubted whether any book of its size has been more important for the progress of the science. Nägeli's introductory paper, "Ueber die gegenwärtige Aufgabe der Naturgeschichte, insbesondere der Botanik," is very metaphysical in tone, and is not free from a certain youthful pedantry. Great stress is laid on the absolute difference of species—a conception which, as Nägeli tells us in one of his later works, did not prevent his believing even then in the origin of species by descent. The study of development is treated as a philosophical necessity, and anatomy, or the study of *mature* structure, is denied to be a science. This is perfectly just; no one did more for anatomy than Nägeli himself, but he recognized that it only becomes scientific in union with development and physiology. He further insists that the knowledge of development as a whole is the only sound basis for classification—a principle which still remains to be carried out. The highest importance is attached to the cell theory, which was expected to do as much for botany and zoology as mathematics had done for physics, or atomic formulæ for chemistry—an expectation which cannot be regarded as unjustified. Nägeli severely criticized the theories then current, according to which cell-formation is a process of crystallization. Some of the most doubtful of his own later generalizations, however, were affected by the same source of error—namely, too great eagerness to find a simple physical explanation for biological phenomena.

Nägeli, in this paper, devotes much space to the distinctions between animals and plants. He decisively rejects the idea of a transition between the two kingdoms, on the ground that this would contradict the "Absolutheit der Begriffe"—an argument which now seems strangely out of place in natural science.

The whole paper is of great interest as showing the point of view from which biological questions were regarded at that time by a brilliant and philosophical naturalist just entering on his life's work.

The two papers in the *Zeitschrift*, on the nuclei, formation and growth of vegetable cells (1844 and 1846), are of the greatest importance to histology, finally establishing the constant occurrence of cell-division as the one mode of vegetative cell-formation. This conclusion was only reached in its completeness in the second of the two papers. Although Unger's and Mohl's views of the details of the process were in some respects the more correct, still Nägeli established the main facts of the division of the nucleus and of the cell on a broad basis of observation. These papers, as well as one on the utricular structures in the contents of cells (nuclei, nucleoli, chlorophyll granules, &c.) were translated by Henfrey for the Ray Society, to the great benefit of English students, as the writer of this article can testify.

In the same journal there are several algological papers, the most important of which is the complete and admirable account of *Caulerpa prolifera*, the extraordinary histological structure of which and its relationship to the other Siphoneæ Nägeli already thoroughly understood. It is interesting that in this paper he describes both the cell-wall and the cellulose rods as growing by apposition, a view to which we have now returned, owing to the observations of Strasburger and Noll, in opposition to Nägeli's own later theory of intussusception propounded in 1858.

The paper on *Delesseria hypoglossum* contains an elaborate account of the cell-divisions by which the thallus is built up. Nägeli here characteristically attributes great importance to the introduction of ideas of absolute mathematical form into physiology and systematic botany.

The discovery of spermatozoids in the Ferns is one of the most important recorded in this volume. The essential points in the structure and development of the antheridia are described rightly, and the movements of the spermatozoids very accurately traced. Nägeli calls attention to the nuclear reactions of the substance of the spermatozoids. He demonstrates the homology of these bodies with those of the mosses and Chara and of animals. Nägeli was at that time necessarily completely in the dark as to the relation of the spermatozoids to spore formation, for the archegonia and the process of fertilization were first discovered by Suminski four years later.

Among other papers of fundamental importance may be mentioned that on the growth of mosses, in which the apical cell-divisions and the development of the protonema are clearly made out; that on the growth of the stem in vascular plants, a work which laid the foundation of our knowledge of the distribution of vascular bundles, and that on the reproduction of the Rhizocarps. This last is especially interesting. It is directed, though very cautiously, against the Schleidenian theory of fertilization as applied to these plants. It is singular how this theory, according to which the end of the pollen-tube, after penetrating the embryo-sac, itself became the embryo, took possession of the minds of botanists at that time, and led sometimes to the strangest confusions, sometimes to a chance recognition of homologies, which could only be legitimately proved at a later period of research. In the case of the Rhizocarps, the Schleidenian theory assumed that these plants were really Phanerogams. Hence we find that he and Nägeli agree in calling their microspores pollen-grains, their microsporangia anthers, their macrospores embryo-sacs, and their macrosporangia ovules, a terminology which very nearly expresses our present view of their homologies as established by Hofmeister. Nägeli discovered the spermatozoids of these plants as well as the prothallus and archegonia, but he shows the greatest reserve in correcting Schleiden's extraordinary mistakes.

It is worth remarking that at this early period the homology of pollen-grains with spores was generally admitted, and at first we wonder how this true result could have been arrived at so prematurely. Here again the Schleidenian theory affords the explanation. The pollen-grain was regarded as a spore, which on germination produced the embryo-plant, not as do the spores of Cryptogams in the open air, but within the embryo-sac of the ovule. This conclusion was of course strengthened by a more legitimate argument drawn from a comparison of the mode of origin of pollen-grains and spores.

A less fortunate result of the same theory appears in a paper in the *Zeitschrift*, "Ueber das Wachstum und den Begriff des Blattes." Nägeli here erroneously attributes to the stem and its branches an endogenous origin. That this holds good for the *primary* axis, he proves by stating that it is derived from the pollen-grain, which itself arises endogenously within the anther!

We have dwelt long on this *Zeitschrift*, as it affords a remarkable insight into the state of botanical questions during the earlier part of the most brilliant period of progress which the science has known. The very name, *Journal for "Scientific" Botany*, is characteristic, expressing the somewhat arrogant claims of the enthusiastic naturalists of the new school of that day.

The next period in Nägeli's career is marked by the publication of two important algological works: "Die neueren Algensysteme und Versuch zur Begründung eines eigenen Systems der Algen und Florideen," 1847, and "Gattungen einzelliger Algen," 1849. It cannot be said that Nägeli was altogether happy in his generalizations on algological subjects, though his special work was often of the greatest value. At that time he included the Lichens among the Algæ and excluded the Florideæ. The Algæ in his sense were distinguished from the Fungi, not only by the presence of chlorophyll and starch, but also by the absence of spontaneous generation, while they differed from the Florideæ and all the higher plants in being destitute of sex. The Florideæ, on the other hand, he regarded as sexual and as closely allied to the Mosses. He recognized their antheridia as the male organs, but regarded the tetraspores as the product of a female organ, on account of their superficial resemblance to the spore-tetrads of the higher Cryptogams. The carpospores, which are the real sexual products, he regarded as gemmæ like those of *Marchantia*, with the *cups* of which he compared the cystocarps. Such views were excusable at that time, but Nägeli, as we shall see, adhered to them later on with excessive pertinacity.

Nägeli was perfectly acquainted with the conjugation of Desmids and Zygnemaceæ and imperfectly with the fertilization of Vaucheria, but he imagined that these processes were too inconstant to be regarded as sexual.

Nägeli was at that time much more successful in dealing with the vegetative organs of the Algæ, and he rightly protested against the generalization current down to our own day, that all Algae are destitute of leaves.

His conviction that the Algæ are without exception sexless led him in 1849 to reject Decaisne and Thuret's discovery of the spermatozoids of Fucus, which he regarded as spores. Of his later algological papers, the most important is that on the Ceramiaceæ, published in 1861. In this the procarpia and trichogynes, the true female organs, are described and accurately figured; but Nägeli failed to recognize their true nature, and still maintained his old view of the sexuality of the tetraspores. The whole credit of the discovery of the real state of the case thus belongs to the French botanists Thuret and Bornet.

The "Pflanzenphysiologische Untersuchungen" of Nägeli and Cramer (1855–8) contain among other papers of importance Nägeli's huge work on starch grains (about 600 quarto pages!), which is of great general value as embodying his views on the growth of starch and cell-wall by intussusception and on the molecular structure of organized bodies. For many years this micellar theory, as it was afterwards called, was regarded as Nägeli's greatest achievement. Sachs, in 1875, said in his "History of Botany": "Nägeli's molecular theory is the first successful attempt to apply mechanico-physical considerations to the explanation of the phenomena of organic life." More recent research has shown that this attempt, like its predecessors, was premature, and though Nägeli's ingenious and carefully elaborated hypotheses must still arouse our admiration, we can scarcely now regard them as having added much to our knowledge either of the growth or structure of organized bodies. The book on "Starch Grains," however, quite apart from theoretical considerations, will always remain a marvellous monument of research. It contains a vast mass of systematic and descriptive matter in addition to the speculations which have made it famous. The micellar theory was further developed in subsequent papers "on the behaviour of polarized light towards vegetable organisms" (1862); "on crystalloid protein bodies" (1862); and "on the internal structure of vegetable cell-membranes" (1864). It is presented in its perfected form in the important work on the microscope, published by Nägeli and Schwendener in 1877.

The papers in the "Physiologische Untersuchungen" bear the name of Nägeli or of Cramer respectively, but it appears that they mutually assisted each other throughout; hence it is not out of place to mention here Cramer's fine researches on the apical growth of Equisetum, which to this day serve as a model (rarely approached) for all such investigations.

No sooner were these investigations with Cramer completed than another great undertaking was commenced in the publication of the "Beiträge zur Wissenschaftlichen Botanik" (1858–68). This began with the great paper "On the Growth of Stem and Root in Vascular Plants and on the Arrangement of the Vascular Bundles." This is the most important of Nägeli's purely anatomical works, and is of the greatest permanent value. It is not too much to say that the bulk of our knowledge of the distribution of vascular tissues in plants still depends on this work. Other valuable papers in the "Beiträge" are those on the use of the polarizing microscope, on the growth in thickness of the Sapindaceæ (another ideal pattern of anatomical research), and on the origin and growth of roots, in which last Leitgeb cooperated. Until the quite recent work of Van Tieghem and Douliot, this was undoubtedly the most important investigation on the subject.

Among Nägeli's later works there are two which have had a lasting influence on our views as to the biology and physiology of the simplest plants. In "Die niederen Pilze" (1877) he treats of moulds, yeasts, and bacteria in relation to infectious diseases and hygiene. In this work an excessive scepticism is displayed as to the existence of definite species among the lowest organisms, such as bacteria. There is no longer any doubt that species are neither more nor less distinct among these simple beings than among the higher plants, but Nägeli did a real service in showing that each of these species may appear in a number of morphologically and physiologically different forms.

Nägeli's "Theorie der Gährung" (1879) demonstrated the relation between the processes of fermentation and respiration, and established the modern view of fermentation, according to which, to use the words of Prof. Vines, "living protoplasm, besides undergoing decomposition itself, can induce decomposition in certain substances which are brought within the sphere of its influence."

It remains to consider briefly an aspect of Nägeli's work, which is from some points of view the most interesting of all—namely, his relation to the theory of descent. The elaborate observations on variable species, especially in the genus *Hieracium*, which Nägeli carried on throughout his whole life, side by side with his histological and physiological work, specially qualified him to take up an independent position with reference to the problems of evolution.

In his paper "Die Entstehung und Begriff der naturhistorischen Art" (1865), Nägeli for the first time discusses this question in the light of Darwin's work. His belief, however, in the origin of species by descent was no new thing, but had been tacitly held by him throughout his whole scientific career, and had been definitely expressed in his paper on individuality in Nature, published in 1856. In his work of 1865 he gave an admirably clear exposition of natural selection, but was unable to accept it as affording a sufficient explanation of evolution. He believed that variation has a definite direction, always tending towards the greater complexity and perfection of the organism (Vervollkommnungstheorie). On this view the development of the race, like that of the individual, has a definite course assigned to it beforehand. He protests that there is nothing supernatural involved in this doctrine, and that it does not necessarily require *sudden* transformations. On this latter question, however, he speaks very uncertainly, and states that transitions between certain morphological types appear to be unthinkable and impossible. One seems to catch here an echo of his older teaching about the "Absolutheit der Begriffe."

The perfecting process, he says, knows no rest; hence all plants would have become Phanerogams by this time were it not that spontaneous generation takes place at all periods. Thus the flowering plants of our own day have, on this view, the longest family history, and trace their descent from the first-formed "Urzellen," while the vascular cryptogams had a somewhat later origin, and have, consequently, not had time to advance so far, the mosses again arose more recently still, and so on with all the groups of plants. According to this singular hypothesis, there is no actual blood relationship between the higher and lower forms of any one epoch. They have had a similar but not a common origin. This remarkable, but, as it seems to us, retrogressive theory was maintained by Nägeli to the close of his career.

But, whatever view may be taken of this speculation, it must be admitted that Nägeli saw clearly the great fact—since brought home to us by the works of Weismann and his school—that the causes of variability are internal to the organism. This important doctrine, based on original experiments and observations, is maintained in a paper entitled "Ueber den Einfluss äusserer Verhältnisse auf die Varietätenbildung im Pflanzenreiche" (1865). He shows that "the formation of the more or less constant varieties or races is not the consequence and the expression of external agencies, but is determined by internal causes"; while the modifications directly produced by external influences are inconstant, and do not give rise to varieties. We think it must be allowed that, on this essential point, Nägeli was at that time somewhat in advance of Darwin himself.

Other works of that period deal with the laws affecting the distribution of species, and with the phenomena of hybridization. In the "Theorie der Bastardbildung" (1866) the peculiarities of hybrids are explained as due to the favourable or unfavourable changes produced by crossing, in the internal coadaptation of the organs of the offspring.

A paper on the social origin of new species (1872) results in the conclusion that groups of new forms are likely to arise simultaneously, rather than isolated new species.

Finally, something must be said of the great work published in 1884, "Die mechanisch-physiologische Theorie der Abstammungslehre," which states at great length Nägeli's final conclusions as to evolution and heredity. The fundamental idea of this weighty work is the conception of the Idioplasm, namely, of a definite portion of the general protoplasm, to which alone is committed the transmission of hereditary characters. This idea, as Weismann points out, is a fruitful one, and will live, and is indeed incorporated in all recent theories of heredity. Nägeli's speculations, however, as to the details of the distribution and molecular structure of this idioplasm are of much more doubtful value, and rest on no firm basis of actual observation.

Nägeli rightly argues that the character of the fertilized egg must be determined by a minute amount of idioplasm and not by the cytoplasm generally, because the characters of the male and female parent are on the average equally represented in the offspring in spite of the enormous difference in the bulk of the cytoplasm of spermatozoid and ovum.

It was only, however, after the idioplasm had been identified by Weismann and Strasburger with a definite constituent of the nucleus that the theory acquired a positive basis.

Nägeli in the "Abstammungslehre" points out that fertilization can only consist in the direct union of solid idioplasmic bodies, and thus on theoretical grounds arrives at a conclusion which has been fully confirmed by the observations of Van Beneden, Strasburger, and Guignard. He also shows that while in the higher organisms idioplasm alone is necessarily transmitted from parents to offspring, in the increase of the lower plants and animals by division, the descendants acquire a share of the nutritive protoplasm also. Hence in the latter the conditions of culture may directly affect the descendants, as Nageli found in his observations on bacteria. These views are in essential agreement with those of Prof. Weismann on the continuity of the germ-plasm, as brought forward a year later, though on other points there is a wide divergence of opinion.

Nägeli insists in his preface to this book, that the subject of heredity can only be authoritatively treated by a physiologist, and he no doubt regarded his micellar theories as an important contribution to the question. In this his view is somewhat one-sided, and as a matter of fact all recent advance in our knowledge of the essential points in reproduction has come from the morphological side.

Nägeli's attitude towards the question of spontaneous generation is interesting. In his early days he had no doubts as to the spontaneous origin of many Fungi, and thought that this could be experimentally demonstrated. In 1865 he gave up the experimental evidence, but believed in the origin *de novo* at all epochs of simple vegetable cells. In the "Abstammungslehre" he still maintains that spontaneous generation is constantly in progress, but no longer holds that even the lowest known organisms can arise in this way. His supposed primitive living things (*Probien*) are as much more simple than bacteria, as these are more simple than the highest animals or plants.

As regards the causes of evolution, Nägeli in his great work appears to limit the field of natural selection even more narrowly than in his earlier essays. Its function, according to his later views, consists in the separation and definition of races by the elimination of ill-adapted forms, rather than in determining the origin of the races themselves. In a brilliant illustration he pictures natural selection as pruning the phylogenetic tree, though powerless to cause the putting forth of new branches. He still regards evolution as a necessary progress towards perfection determined by the constitution of the organism itself, and more especially of its idioplasm.

This view is only needed if we assume with Nägeli the existence of purely morphological characters—of characters, that is, which are not, and never have been, of the nature of adaptations. It appears to us to have been sufficiently shown by Prof. Weismann and others that the existence of such characters is an unnecessary assumption. As biology advances, we learn every day the function of characters which had before appeared to us to be useless, and the whole tendency of investigation is to prove that all characters whatsoever are either of direct use to their present possessors or have been inherited from ancestors, to whom, at the time when they were acquired, they were equally advantageous. It would be difficult to cite a stronger instance of a "morphological character" than the alternation of generations which so clearly characterizes the higher cryptogams. Yet it has been lately shown by Prof. Bower that this may well have been an adaptive character at its first origin, the sporophyte being adapted for taking possession of the dry land, while the oophyte, owing to the mode of fertilization, was compelled to retain a lowly and semi-aquatic habit.

We have given a very incomplete and imperfect sketch of the life-work of one of the most illustrious of that illustrious band of botanists to whom the chief advances of our science are due. Much of his work has of necessity been left quite unnoticed. But on even a cursory glance through the writings of Nägeli the conviction is forced upon us that he was a man not only of exceptionally wide scientific and philosophical training and of great literary power, but also one of real genius, and as far removed as possible from that narrow specialism which is the besetting sin of so much modern scientific effort. The judgment of Nägeli's colleague, Prof. Cramer, that he was "a truly great man," cannot be dismissed as the exaggerated language of personal affection, but expresses a truth. Though some of his theories may be abandoned, a vast sum of permanent achievement will always remain, and the influence of Nägeli on the future of our science will be powerful and lasting.

D. H. Scott.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Oxford.—Full term commences on Saturday, October 17. The following lectures in science generally have been advertised:—

The Savilian Professor of Geometry (J. J. Sylvester) will lecture on surfaces of the second order, illustrated by the models with which that department has been supplied at the request of the Professor.

The Professor of Astronomy (Rev. C. Pritchard) proposes to lecture on the methods of determining astronomical constants, and offers practical instruction with the transit circle and solar spectroscope.

Rev. Bartholomew Price (Sedleian Professor of Natural Philosophy) lectures on hydromechanics.

The Professor of Experimental Philosophy (R. B. Clifton) will lecture on electricity; and instruction in practical physics is offered by Mr. Walker and Mr. Hatton at the Clarendon Laboratory. Lectures on mechanics and experimental physics are offered by Rev. F. J. Smith, at the Millard Laboratory.

The Waynflete Professor of Physiology (J. S. Burdon-Sanderson) will lecture on the subjects required for the final examination in the School of Physiology, and Mr. Dixey will lecture on histology. Practical instruction on this latter subject will be given by Mr. Kent.

In the subject of Chemistry, the Waynflete Professor (W. Odling) will lecture on animal products, while the Aldrichian Demonstrator (W. W. Fisher) will give a series of lectures on the non-metallic elements. Mr. J. Watts lectures on organic chemistry, and the instruction in practical work is under the supervision of Mr. Watts, Mr. Veley, and Mr. J. E. Marsh.

The Deputy Linacre Professor of Human and Comparative Anatomy (E. Ray Lankester) offers a course of lectures on comparative anatomy and embryology. This course is intended for seniors. There will also be a junior course for beginners and candidates for the preliminary examination in animal morphology conducted by the Deputy Linacre Professor and Dr. W. B. Benham. This last-named gentleman will also lecture on the Chætopoda.

The Professor of Geology (A. H. Green) offers two courses of lectures, one on physical, the other on stratigraphical geology.

The Reader in Anthropology (E. B. Tylor) will lecture on the origin and development of language and writing.

The Sherardian Professor of Botany (S. H. Vines) lectures, this term, on elementary botany.

The Hope Professor of Zoology (J. O. Westwood) lectures and gives informal information upon some of the orders of Arthropoda.

In the department of medicine, Sir H. W. Acland, Bart., gives informal instruction on modes of medical study. This instruction is given at the Museum, where arrangements will be made for one or more demonstrations in illustration of subjects bearing on public health. Dr. Collier and Mr. Morgan give demonstrations for the Professor on Medical and Surgical Pathology. The Lichfield Lecturer in Clinical Medicine (W. Tyrrell Brooks) will lecture on the physical signs of disease, and the Lecturer in Clinical Surgery (A. Winkfield) offers instruction on the treatment of fractures, &c.

The Lecturer in Human Anatomy (A. Thomson) offers a course of lectures on human osteology, and a series of demonstrations will be arranged to meet the requirements of those working in the department. The dissecting-room will be open daily for practical work and instruction.

The Rev. H. Boyd, Principal of Hertford College, has been nominated Vice-Chancellor for the ensuing year.

A mathematical fellowship has been awarded at Merton College to Mr. Arthur Lee Dixon, B.A., formerly scholar at Worcester College. Mr. Dixon was placed in the first class both at Moderations and in the final Mathematical Schools. He obtained the Junior Mathematical Scholarship in 1887 and the Senior Mathematical Scholarship in 1891. Also at Corpus Christi College a mathematical fellowship has been awarded to Mr. Arthur Ernest Jolliffe, scholar of Balliol College. Mr. Jolliffe was placed in the first class by the Mathematical Moderators in 1889, and in the first class by the Examiners in Scientiis mathematicis et physicis in 1891. He also obtained the Junior Mathematical Scholarship in 1889.

CAMBRIDGE.—The erection of the Newall telescope is nearly completed. Prof. Adams was able to use it for the first time last week, and took an observation of Neptune.

Prof. Ewing announces that the new Engineering Laboratory is ready for use, and will be occupied this term.

Mr. F. Blackman, of St. John's College, has been appointed Demonstrator of Botany.

By the return of Prof. Jebb, the University enjoys the distinction of being represented in Parliament by a Senior Classic (Dr. Jebb) and a Senior Wrangler (Sir G. G. Stokes).

Sixty-four candidates entered for the examination in sanitary science held last week. Of these forty-three have passed both parts of the examination, and receive the diploma in Public Health.

The Lecturer in Geography (Mr. Buchanan, F.R.S.) will this term lecture on physical and chemical geography, with especial reference to land surfaces and their development under climatic and other agencies.

The vote in the Senate on the question whether a syndicate shall be appointed to consider alternatives for Greek and Latin in the Previous Examination will be taken on Thursday, October 29, at 2 p.m.

University Extension.—It is announced that Mr. T. D. Galpin, of the firm of Cassell and Co., Limited, has offered to the Dorset County Council the sum of £1000 to be invested for the purpose of providing scholarships to send natives of Dorset to the Summer Meetings of Oxford and Cambridge. The scholarships will be awarded to the writers of the best essays, and it is proposed that the examination should be conducted by the University Extension Committee of the Oxford Delegates of Local Examinations. The scholarships are to be awarded without distinction of sex, or any political, sectarian, or social distinction whatever.

SCIENTIFIC SERIALS.

THE *American Journal of Science*, October 1891. Some of the possibilities of economic botany, by George Lincoln Goodale. This is the Presidential address delivered before the American Association for the Advancement of Science, at Washington in August last.—On the vitality of some annual plants, by T. Holm. The author enumerates several species of plants which show a tendency to vary from annual to biennial or perennial.—A method for the separation of antimony from arsenic by the simultaneous action of hydrochloric and hydriodic acids, by F. A. Gooch and E. W. Danner.—Notes on allotropic silver, by M. Carey Lea. The blue form of allotropic silver is mainly considered. The action of light on this form is remarkable, for its effect is first to increase the sensitiveness to reagents and then to completely destroy it. This reversing action is analogous to that which light exerts upon silver bromide. Mr. Lea has also examined the point as to whether in the reduction of silver, the allotropic or the normal form is produced, and he finds that when the silver passes from the condition of the normal salt or oxide to that of the metal, the reduced silver always appears in the ordinary form. But when the change is first to sub-oxide or to a corresponding sub-salt, the silver presents itself in one of its allotropic states.—Structural geology of Steep Rock Lake, Ontario, by Henry Lloyd Smyth.—On the so-called amber of Cedar Lake, North Saskatchewan, Canada, by B. J. Harrington. The resin or "retinite" examined by the author had a hardness of about 2·5, and a specific gravity 1·055 at 20° C. An analysis gave for its composition, carbon 80·03, hydrogen 10·47, and oxygen 9·50.—Geological horizons as determined by vertebrate fossils, by O. C. Marsh. The method of defining geological horizons by vertebrate fossils was first used by the author in 1877, and appears to afford the most reliable evidence of climatic and other geological changes. It is now extended and revised. A section accompanies the paper representing, in their geological order, the successive strata at present known with certainty from characteristic vertebrate fossils.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 3.—M. Duchartre in the chair.—On the variations of composition of Jerusalem artichokes from the point of view of mineral matters, by M. C. Lechartier. The author gives the results of some investigations made at the Rennes Agricultural Station, on the culture of artichokes in soils differently treated. He has also studied atmospheric influences as indicated by cultures on similar plots for three consecutive years.—Observations of Wolf's comet made with the great telescope of Toulouse Observatory, by M. E. Cosserat. Observations for position were made and are recorded, extending from August 13 to September 28.—On the value of electrostatic tension in a dielectric, by M. L. de la Rive.—On the simultaneous existence, in cultures of *Staphylocoque pyogène*, of a vaccine substance capable of being precipitated by alcohol, and of a substance soluble in alcohol, by MM. A. Rodet and J. Courmont.—On some parasite Copepods, by M. Eugène Canu.—Observations of the fall of a solar prominence into a spot, by M. E. L. Trouvelot. The observations relate to some remarkable luminous filaments occurring in a group of spots from August 6 to August 10.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Physical Geology and Geography of Ireland: E. Hull, 2nd edition (Stanford).—On Surrey Hills, a Son of the Marshes (Blackwood).—By Seashore, Wood, and Moorland: E. Step (Partridge).—An Introduction to Human Physiology: Dr. A. D. Waller (Longmans).—Guide to the Examinations in Physiography, and Answers to Questions: W. J. Harrison (Blackie).—Journal of the Chemical Society, October (Gurney and Jackson).—London and Middlesex Note-book, vol. i., No. 3 (E. Stock).—Botanischer Jahrbücher für Systematik, Pflanzengesschichte und Pflanzengeographie, Vierzehnter Band, 3 Heft (Leipzig, Engelmann).—Quarterly Journal of the Royal Meteorological Society, July (Stanford).—Meteorological Record, vol. x. No. 40 (Stanford).—Himmel und Erde, October (Berlin).

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THURSDAY, OCTOBER 22, 1891.

RUDOLF VIRCHOW AND HIS COUNTRYMEN.

THE German people are to be congratulated on the brilliant way in which the seventieth birthday of Prof. Virchow was celebrated last week in Berlin. We say the German people, because the entire nation associated itself with the scientific societies in doing honour to the illustrious investigator of whose achievements it has for many a day been so justly proud. Everyone who devotes the slightest attention to science is aware that Prof. Virchow occupies a prominent place among the foremost intellectual leaders of the present age. As the *Times* has said, "So much has he done, and so thoroughly has he done it, that it is difficult for this generation to apprehend the full magnitude of his work. Open a book on medicine, and especially any volume on pathology, composed, it matters not much where, before Virchow began his observations, and compare it with one composed with the light of his endless investigations to guide the author: a veritable revolution in conceptions and terminology has taken place; at every turn you read, 'All this is understood since Virchow wrote,' or words to that effect; and you are referred to his multifarious 'epoch-making' articles scattered through many professional and technical periodicals." By his great principle, "Omnis cellula ex cellula," he made a contribution of the highest importance to biological science; and his conception of cellular processes introduced wholly new and most fertile ideas as to all the phenomena of disease. The science of pathology as it is now understood and taught we owe, indeed, mainly to his insight and labour, and the recent advances which have been made in it by other explorers have been made on the lines he has traced. If Prof. Virchow had done nothing else for science, this alone would have secured for him imperishable fame; but his energies are so varied that it has been impossible for him to content himself with one department of research. As a student of archæology, ethnology, and anthropology, he is hardly less eminent than as a pathologist. In all these sciences he has marked an era by his writings, and by the personal influence he has exerted on the Berlin Gesellschaft für Anthropologie, Ethnologie, und Urgeschichte, which he founded in 1869. In practical life, too, as a member of Parliament and of the Municipal Council of Berlin, Prof. Virchow long ago made himself a great power in Germany. He has missed no opportunity of expounding the laws of public health, and of insisting upon their importance; and a striking testimony to the value of his work in this direction may be seen in the improved sanitary condition of the German capital.

To the Germans it seemed perfectly natural that, when so illustrious a man of science completed his seventieth year, the nation should offer its congratulations on the splendid results he had accomplished. Would an English man of science of corresponding intellectual rank have received similar tokens of popular gratitude and respect? Unfortunately, the question answers itself; and it would be well worth the while of Englishmen to consider carefully the causes which have led to the contrast in this respect between them and their German kinsfolk. It may be said that Germans are more demonstrative than Englishmen, but this by no means accounts for the very different ways in which scientific discoverers are treated in the two countries. The real root of the difference lies in the fact that the importance of science is much more highly estimated in Germany than in England, and especially by the Governments. For several generations, the various German Governments have done everything in their power to foster scientific investigation. With this object in view, they have spent money freely and wisely, allowing themelves to be guided, not by impulse or caprice, but by the advice of men of wide experience and knowledge. They were quick to note the influence which might be exerted on industrial development by technical education; and the result is that Germany has for some time had as many technical schools and colleges, adequately equipped, as are necessary for her wants. We need scarcely say how very different is the spirit that has hitherto animated our own Government. The idea of most English statesmen about science seems to be that it is a bore and a nuisance, and that the less they have to do with it the better for themselves and the public. Even for technical instruction they declined to make provision, until, by an accident, the present Government found itself in possession of a fund which it did not know how to get rid of except by giving the County Councils authority to use it for the establishment of technical schools and classes. Is it surprising that when their rulers act in this way the mass of the British people should be utterly indifferent to scientific progress? The Germans have been accustomed all their lives to see science encouraged, and all classes learn therefore to regard it as an essential factor in the evolution of their national life. This week they have had a fresh example of the respect in which science is held, the Emperor having appointed Prof. Helmholtz a member of the Privy Council, with the title of Excellency. In the telegram announcing to Prof. Helmholtz the honour conferred on him, the Emperor took occasion to refer with pride to the lustre shed on Germany by his scientific achievements. Nothing of the kind is ever done here.

The influence of education must also, of course, be taken into account. There is still some dispute in Germany, as in other countries, about the exact place which properly belongs to science in general education; but there is no dispute at all as to the importance of training children to recognize the benefits which science in all its branches has conferred on mankind. Moreover in the "Realschulen" an excellent scientific training is provided for those who either have little power of appreciating classical literature, or who are likely to be best fitted for their future work by the study of science. And in elementary schools an effort is everywhere made to interest children in the facts and laws of nature, and to give them some conception of the objects and methods of scientific inquiry. How far we lag behind the Germans in these respects all true "educationists" know. We have made only a beginning in the use of science as an instrument of popular culture, and many years, we fear, may pass before we shall have applied it sufficiently to render scientific conceptions a really vital element in the intellectual life of the community.

It is not for the sake of men of science that we desire to see more widely diffused an intelligent appreciation of their work. A celebration like that of last week necessarily brings with it sad as well as happy reflections. "After all," said Bluntschli, the famous jurist, on a like occasion, "it is an end, not a beginning." Prof. Virchow is fresh and vigorous, and the world may still reasonably expect from him much sound work; but we may be sure that, in responding to congratulations, he had a little of Bluntschli's feeling; and it is possible that, if he had consulted his own wishes only, he would have preferred to celebrate his seventieth birthday more quietly. But it is good for a nation to express on such occasions the admiration and reverence excited by a long and great career. The mere fact that men desire to honour one whose title to distinction is that he has advanced human knowledge proves that they have interests higher than those of a material character; and it inevitably tends to deepen and strengthen the best and most enduring of their impulses. We should be glad, therefore, if Englishmen had as strong a wish as Germans to display a hearty appreciation of the triumphs achieved by their great scientific thinkers. That would be the most effectual of all proofs that they had begun, as a people, to understand how momentous is the part which science has played, and must continue to play, in the modern world.

ELECTRIC LIGHT FITTING—GOOD AND BAD WORK.

Electric Light Fitting: a Hand-book for Working Electrical Engineers. By John W. Urquhart. (London: Crosby Lockwood and Son, 1890.)

THIS book is exactly what it professes to be—a practical book for practical men—and is vastly superior to "Electric Light," by the same author. The detailed instructions given in the first 42 pages, on the erecting, managing, and repairing dynamos, are admirable, and are not to be found in any other book in the English language. The young electrical engineer will find just the information he needs: how to fit up a large dynamo when received in parts from the makers; how to prevent the commutator becoming rough in use; exactly what to do if it be rough; how to prevent sparking at the brushes; how to attach a new commutator and make joints in the armature wires; what to do if the dynamo heats; and how to get over the various other difficulties met with in the dynamo-room.

The author, in these early chapters, and indeed throughout the book, uses the expression "constant current" for direct current; and although the action of the regulators of the Brush and of the Thomson-Houston constant current dynamos is correctly described, and clear illustrations given of their construction, the reader is left in the dark as to the exact use of these regulators. Or, rather, the only definite statement as to the function of the Thomson-Houston regulator, that it is "for causing the machine to evolve more or less current as required," is certainly much more likely to lead the reader wrong than right. Further, to say that "in Siemens's alternator, or the Ferranti dynamo, 'lead' must be given to the brushes" (an instruction, of course, quite impossible to carry out, as alternate machines have no commutators, but only collecting rings), will probably destroy the correct impression about lead which the practical man may have derived from reading the previous page.

In spite of these defects, however, chapter i. is excellent, but we cannot speak quite as highly of chapter ii., "On Localizing Dynamo Faults, and Observations respecting Accumulators." In describing the test for the existence of leakage between the iron framework and the earth, the author makes an error that we have met with before, in stating that a deflection of a galvanometer whose ends are connected respectively with the iron framework and the earth indicates leakage between these two. This is equivalent to saying that a conductor not having the potential of the earth proves that it is in connection with the earth. In the "Hints to Accumulator Attendants" there are some very useful suggestions, but the instructions for deciding when an accumulator is charged confirm the impression we gave when reviewing the author's "Electric Light," that the author had not derived his knowledge of storage cells from a practical acquaintance with them. For he says that they must not be so much discharged that they cease to give any current; and in the chapter on "Switch Board and Testing Work," that the E.M.F. of accumulators, in discharging, should never be allowed to fall below 0·5 volt per cell. Such instructions are about as useful as saying that a horse should not be worked until he dropped, for if accumulators were to be regularly discharged until their E.M.F. fell to a value even three times as great as the limit prescribed by Mr. Urquhart, they would be speedily ruined.

Why these two statements about the discharge limit of storage cells should be given in different parts of the book, with information about "Running Dynamos in Parallel," the "Periodicity of Alternators," &c., inserted between, we do not know. In a somewhat similar way, the author returns again and again in different parts of the book to the subject of insulation resistance. Each time, no doubt, valuable information is given; but why not have put it all together, so that the working electrical engineer could have at once read up the subject, without having to turn up a number of references? This sort of scattering of information runs through the whole book, and rather suggests the idea that no very serious attempt was made to sort out information written down by the author as it occurred to him at different times.

We do not think that the explanation on p. 54. "alternators work according to a 'phase,'" is very lucid, Further on, the author says the number of phases per second is the periodicity, and later that periodicity and phase are the same thing. On p. 51 we are told "a fall of five volts in a hundred affects the brightness of the lamps," from which a person might easily obtain the wrong impression that a fall of two or three per cent. was not observable, and be astonished when he read, on p. 72, "that a fall of five volts in a hundred in the working pressure will cause lamps which burn brightly at a hundred volts to become very dull." He would also not be able to reconcile the statement, "upon well-conducted systems the pressure upon the mains is never allowed to vary more than one-half per cent.," with the variation of 2 per cent. up and 2 per cent. down, which is allowed by the Board of Trade. Nor is it possible to understand the rule with reference to the wiring of a house, "It should show an insulation resistance of at least ·1 megohm per lamp," since this would make the insulation of an installation the higher the greater the number of lampholders, whereas of course, as a matter of fact, the very reverse is the case.

Chapter iv., on "Arc Light Wiring and Fitting," is full of practical suggestions; the instructions on the trimming of arc lamps, and the precautions that ought to be adopted in order to keep arc lamps in good working order, will greatly help the young engineer when he is first put in charge of arc lamps. It is a pity, however, that when the author is speaking of supplying constant current to a variable number of arc lamps running in series, he should say, "but the shunt or compound-wound machines are supposed to regulate themselves, which they very often fail to do." For we never heard of a compound-wound machine, still less of a well-made shunt machine, which professed to produce a constant current when the external resistance was varied. And this mistake is emphasized in the next section, on running arc lamps in parallel, since, although it is quite rightly said of the attendant, that "his chief care is to keep the *potential difference* between the leads the same," Mr. Urquhart states, "This is usually effected in part by the dynamo itself when a shunt-wound machine is used, or by regulating the speed"; and he makes no reference here to the use of a compound-wound machine, as if it were not the special function of this type of machine to keep the potential difference between the mains constant.

There is a good illustration on p. 107 of the Thomson-Houston lightning arrester, with an explanation of its construction, but no hint is given that the electric arc produced by the lightning flash is magnetically blown out and thus extinguished. And in the large perspective illustration of a Thomson-Houston transformer, given in this chapter, the thickly-insulated leads are shown with a thick copper conductor inside them, while the lightly-insulated leads have a thin conductor, and since, in the description of a transformer, it is not stated that, besides transforming from a high to a low *potential difference*, this apparatus also transforms from a small to a large *current*, it would be quite possible for a beginner to read this book, and wonder why people went out of their way to construct dynamos to produce one or two thousand volts, and then had to employ special apparatus at the consumers' premises to lower this high potential difference. "It is usual to put the secondary circuit to earth," probably expresses the author's view (as it also does the reviewer's) of the proper way to guard against accidents being produced by a contact between the primary and secondary circuits of a transformer, but it certainly does not represent the ordinary practice.

The name "impedance coils" is suggested for inductive coils used to diminish a varying or an alternating current; but the necessity for this name arises from the expression "choking coils," which is commonly used in this sense, having been wrongly employed by the author for any kind of resistance coils, such as, for example, a non-inductive resistance used with a steady current.

Chapter v., on "Wiring for Incandescent Lamps," abounds in useful hints, and is illustrated with several well-executed woodcuts. Admirable, however, as may be the switches, fuses, &c., constructed by Messrs. Woodhouse and Rawson, the succession of illustrations with the names of that firm underneath tends to give the impression that there are no other manufacturers of such apparatus. Surely the weighted fuses made by the Acme Works, the switches of Messrs. Siemens—which provide a metallic circuit for the current but expend the flash, produced by opening the circuit, on carbon contacts—and the S switches of Messrs. Crompton, were worthy of a reference.

If the well thought out precautions detailed in "Methods for Running Wires" had been followed in all the wiring of houses that has been carried out during the past few years, we should not have heard of those very justifiable complaints of occupiers who, after taking the lease of a house, temptingly described in the agent's list as fitted throughout with the electric light, find that they have to entirely re-wire the house before the insurance office will allow the current to be turned on. We thoroughly agree with the author that "There is one leading maxim for a contractor putting in electric light, and it is to avoid contracts that do not allow of the best class of material and labour being used throughout." We should also like to impress on the general public that the plummer, or the carpenter's handy man, is not, as they seem to think he is, any more capable of fitting up an electric installation than he is of setting a broken leg.

We do not understand why, as a definition of "cleat wiring," Mr. Urquhart says, "This means uncovered wires run &c."; surely cleats are frequently employed to hold down covered as well as uncovered wires. On p. 185 the temperature is not stated at which "the ohm is the resistance offered by a column of mercury 1 square millimetre in cross-section and 106 centimetres long." Power and work are said to be synonymous, and foot-pounds said to be analogous with volt-amperes. The output of 1000 watts "is called under the Board of Trade regulation a *kilowatt*," whereas the late Sir William Siemens, and not the Board of Trade, originated this name. "As lamps are now made, each would probably give a light of 20 candle-power, the watts per candle-power being 2·5." Would that we could buy glow lamps which had a decent life, while needing only 2·5 watts per candle.

Sir William Thomson's rule about the right sectional area to give to a conductor "is only a suggestion made for the protection of buildings from fire." We thought everyone knew that it was a rule for settling the thickness of the conductor with which maximum economy could be obtained.

The rules about jointing leads are exact and valuable; we do not, however, like the general rule of using the body of a chandelier itself to serve as the return, and we think this rule ought to be followed only when the return wire is throughout the installation an uninsulated one.

Chapter vi. gives a good *résumé* of the *pros* and *cons* regarding the use of the body of an iron ship as the return for ship lighting; while chapter vii. gives the substance of the rules issued by the Institution of Electrical Engineers, in connection with fire risks and danger to life.

MORE SUGGESTIONS FOR COUNTY COUNCILS.

County Councils and Technical Education. By J. C. Buckmaster. (London: Blackie and Sons.)

UNDER the above title Mr. Buckmaster, who for many years has been connected as teacher, lecturer, and organizer with the Science and Art Department, gives some statistics relating to technical education, and his views on the best way of utilizing the funds in the hands of County Councils. We need hardly say that, backed as they are by so long an experience, his opinions deserve the most careful and respectful consideration.

Briefly stated, Mr. Buckmaster believes in class teaching as opposed to lectures, and in utilizing as far as possible existing elementary and science and art teachers. "Unless," he says, "the sympathy of teachers and other educationists can be enlisted, the most carefully considered schemes of County Councils can only end in partial or complete failure." Again,

> "Lectures by themselves are never to be highly valued as a means of education. In a lecture on science, to create and sustain an interest, you must be popular, and to do this you avoid the complex difficulties of the science, which are often the only intellectual parts of it. . . . Lectures, unless followed up by thought and reading on the part of those who hear them, fail as a means of education, &c., &c."

All this is excellent, and the warning is useful. But when Mr. Buckmaster comes to the application of these principles he is not quite so happy. For example, he is unjust to the University Extension system, which he does not clearly understand, and treats as though it were mere popular lecturing, like the work of the old Mechanics' Institutes. Now, though we have no belief that the University Extension machinery can fill the place of elementary class teaching, we cannot accept the implied suggestion that courses of ten or twelve lectures (often arranged in sequences of two or three sets of twelve lectures), each lecture followed by a class for the more serious students, and by written paper work corrected by the lecturer, and the whole course tested by independent examination, form an engine of instruction scarcely above the level of a clever conjuror's performance.

His constructive suggestions are, first, to use elementary teachers to give object-lessons in simple science—a most useful proposal, about to be carried out in various counties as soon as the teachers themselves can be properly trained for the work; and secondly, to multiply science and art classes. "The best technical instruction for some time will be a wider development and extension of the educational work of the Science and Art Department by means of night classes and continuation science and art schools." This depends, of course, on the meaning to be attached to "development." If it merely means multiplication, the statement is open to serious question. No one can know better than Mr. Buckmaster the special dangers attaching to the system which he advocates—the abuses which grow up round a system which makes the financial success of the class, and usually the salary of the teacher, depend on the result of an examination. In our opinion, the machinery of the Science and Art Department will long continue to be a most useful and important factor (though not to the exclusion of other agencies) in the development of technical instruction. But the present is the great chance to consolidate and improve, rather than merely extend the work. If the County Council funds are so granted as to correct the evils which inevitably arise out of such a system of payments on results as is adopted by the Department—if its control is used to render more effective the ***inspection*** as opposed to the mere examination of science and art classes—then the portion of the grant given to promote the work aided by the Science and Art Department will be well spent. But no claim on the part of this or any other single agency to a monopoly of all technical instruction above the rank of that which can be given by the village teacher can be conceded. Mr. Buckmaster does not in so many words make the claim, but he sometimes seems to imply it by minimizing the value of most other experiments which County Councils are attempting. It is virtually a plea for educational bureaucracy against local experiment. But we have not yet reached the stage, if, indeed, we ever do so, when variety of experiment can be dispensed with. Some of the experiments will probably fail. But it is only by wide and free experimenting that the "fittest" will be discovered. Mr. Buckmaster has confined himself, probably on purpose, to the elementary branches of technical instruction, and is silent on its higher developments. Manual work he only just mentions, and not with much sympathy. His criticisms on the wood-carving taught by ladies in villages is not, perhaps, too severe; but it is strange that he does not give a hint that systematic manual training may be (as it has been for a long time in other countries, and lately in our own) made of real educational value. Not a word is said of the worst defect of all in our educational system—the want of good, cheap, secondary schools, which the present grant may do so much to remedy.

Though, however, Mr. Buckmaster takes a rather cramped and narrow view of the outlook, his pamphlet is full of valuable, if rather partial, ideas.

The pamphlet opens and concludes with some useful statistical and other information taken from various publications of the National Association for the Promotion of Technical and Secondary Education. Readers who do not know the source from which these pages are derived may be puzzled by a reference to "the Committee" (p. 41), which by some error in editing has been left still standing, without explanation, in Mr. Buckmaster's pamphlet.

THE MISSOURI BOTANICAL GARDEN.

Missouri Botanical Garden: Second Annual Report. By William Trelease. Pp. 188; Plates 48, reproduced Photographs 5, and Plan of Garden. (St. Louis, Missouri: Published by the Board of Trustees, 1891.)

THE Board of Trustees of the Missouri Botanical Garden have instructed the Director to edit for publication each year a volume setting forth the objects of the Garden and the School of Botany, and the results accomplished by each. The first volume of this series was issued in December 1890, and contained an account of the Garden and School. The present volume, therefore, really begins the series of annual reports, and together with the reports we have a revision of the North American species of *Epilobium*. In the earlier part of the book details are given of the appointment of six garden pupils to scholarships in accordance with a resolution adopted by the trustees at a meeting held in November 1889. Each scholarship conferred may be held by the recipient for a period not exceeding six years, subject to certain conditions. The holders of scholarships are repaid for their services to the Garden, and at the expiration of the six years are entitled to examination by the Garden Committee. On passing such examination to the satisfaction of the Committee and Director, they receive a certificate of proficiency in the theory and practice of gardening. The only scientific paper in the volume is, as we have just mentioned, a revision of the genus *Epilobium*, the American species occurring north of Mexico being those studied. This genus differs from all the other capsule-bearing *Onagraceæ*, except the Californian *Zauschneria*, in having its seeds provided with an ample coma at the apex. While it reaches great development in New Zealand, *Epilobium* is essentially a genus of temperate and cold climates, and the most widely distributed species are those of Arctic and Alpine regions. In Alaska a few such species occur, which are otherwise confined to the adjacent part of Asia. More widely distributed Arctic-Alpine immigrants from the Old World to the New are *E. spicatum*, *E. latifolium*, *E. palustre*, *E. alpinum*, &c. *E. hirsutum*, *E. parviflorum*, and *E. adnatum*, also occur as accidental waifs. The genus passes into South America along the backbone of the continent; few members of this family extend very far across the Mexican boundary in either direction. The most interesting biological features of the genus are those connected with the means of vegetative propagation, pollination, and dissemination. The contrivances by which species survive the winter, and are vegetatively propagated, in this respect attain an extreme degree of differentiation, one in particular having acquired aërial bulblets. The large-flowered species appear to be regularly proterandrous, the duration of the dichogamy being brief in most of them, and the smaller-flowered seem to be always synacmic and self-fertile, although with the probability of frequent intercrossing by aid of insects attracted by the nectar which is secreted within the calyx tube. The genus is of no striking economic value. The North American Epilobia have been mostly described by De Candolle, Torrey and Gray, Haussknecht and Barbey; the more notable works of more limited range being Hooker's "Flora Boreali-Americana," and Brewer, Watson, and Gray's "Botany of California." Prof. Trelease in his revision enumerates 38 species, which number includes the following novelties: *E. holosericeum*, *E. delicatulum*, and *E. clavatum*. The well-known sections *Chamænerion* and *Lysimachion* are still adhered to, the latter, of course, being by far the larger. In the analytical key the main divisions depend on whether the stigma is deeply 4-lobed or 4-cleft, or entire or only notched. Subdivisions are founded on whether the seeds are smooth, or papillately roughened. The name *E. spicatum*, Lam., is used instead of *angustifolium*, the typical *angustifolium* of Linnæus being, according to Prof. Haussknecht, what is commonly known as *E. Dodonæi*, Vill. We are glad to see that Prof. Trelease differs from Prof. Haussknecht in not adopting a new name for what is left of the original *E. alpinum* The *E. alpinum* of Linnæus included with this *E. Hornemanni* and *E. anagallidifolium*, but we think that the name may well stand for one of the segregates. The genus *Epilobium* has always proved a difficult subject; and Prof. Trelease is to be congratulated on his careful treatment, and successful arrangement, of the North American members. The 48 plates will be found of great help to students of these plants; they are not quite of uniform merit, but, taken as a whole, they give the essential details, stress being laid on the varied form of the stigma and seed. Additional illustrations are some well-reproduced photographs taken in the Garden, and a plan of the grounds (scale $\frac{1}{720}$) in five sections.

E. G. B.

OUR BOOK SHELF.

The Story of the Heavens. By Sir Robert Stawell Ball. Eighteenth Thousand. (London: Cassell and Company, 1891.)

In the preface to this edition, Sir Robert Ball remarks that he has taken the opportunity to "revise the work in accordance with the progress of astronomy during the last four years," and, generally speaking, new facts and theories are briefly referred to. A few points, however, are hardly brought up to date. For example, the spectrum of the Andromeda nebula is said to be "a faint continuous band of light" (p. 462), although it is now definitely known that this continuity does not exist. We also find no reference to the many stars now known to have bright lines in their spectra. The author thus misses a chance of exercising his well-known descriptive ability in an account of the connection between such stars and nebulæ; the similarity of the two being so considerable that Pickering has followed Lockyer in arranging them in a single group. Dr. Huggins's old view as to the coincidence of the nebula line with nitrogen is mentioned merely to be dismissed as erroneous. Why, therefore, is no notice taken of the suggested magnesium origin of the line—for, on any published evidence, the edge of the magnesium fluting is nearer the proper position than the nitrogen double? We would also point out that, according to recent observations, the apex of the sun's way is much nearer Lyra than Hercules. Telescopic changes in comets are fully described, but the accompanying changes in their spectra are not touched upon. Motions of stars in the line of sight are considered; but not those of nebulæ, although Mr. Keeler's observations have been published for some time. In fact, it may be said that there is a tendency to eschew spectroscopic questions, and hence much of the most beautiful part of the story of the heavens is left untold.

Notes on Elementary Physiography. By Horace C. Martin. (London and Manchester: John Heywood, 1891.)

The author has collected a lot of scraps of information from standard writers on physiographical matters, and has strung his gleanings together to form this book. And if he were an adept at compilation, and knew how to best arrange and connect facts, this plan of printing extracts *verbatim* might be commended. But when Mr. Martin selects notes which by themselves are incorrect, and interpolates in others crude statements which render them ridiculous, he does an injustice to the authors to whom he acknowledges his indebtedness, and he shirks responsibility by saying that "these notes do not lay claim to originality." Could anything be more misleading than the following description of sun-spots on p. 148? "They seem to rise suddenly to a great height, cool, and then sink back into the photosphere. They are due to uprushes of incandescent hydrogen, and are identical with the red flames seen during an eclipse." And the figure that accompanies this text cannot be a sun-spot at all, but must be something else inserted by mistake. Another blunder occurs on p. 59, where a section of an intermittent spring is shown upside down. The figures are mostly very coarse and poor, especially the moraines on p. 62, the section through a cinder cone on p. 89, and one of a volcano on p. 90; whilst the two figures of ocean bottoms on pp. 102 and 103 give a very wrong idea of their nature. There is, of course, a deal of information in the book, but no attempt is made to give it interest. In fact, although the author is a teacher of physiography, it is very evident from his work that he has not paid attention to the practical side of his science, or verified any of the phenomena he essays to describe. As a book of reference the work before us is untrustworthy; and as a work for students of elementary physiography it is useless and much to be condemned.

Thomas Sopwith, M.A., C.E., F.R.S.; with Excerpts from his Diary of Fifty-seven Years. By B. Ward Richardson, F.R.S. (London: Longmans, Green, and Co., 1891.)

MR. SOPWITH died in 1879 at the age of seventy-six. He was not eminent as an original scientific investigator, but he was a man of great vigour and freshness of mind, and had won the affection of a wide circle of friends by his genial and happy temper. For many years he resided at Newcastle as an engineer and railway surveyor. Afterwards he removed to Allenheads, where he served as the chief agent of Mr. T. W. Beaumont's lead-mines in Northumberland and Durham. Dr. Richardson's book will recall Mr. Sopwith vividly to the minds of his friends, and it contains many things which will be of interest even to readers who were not personally acquainted with him. During the long period of fifty-seven years he kept a diary regularly; and of this, of course, Dr. Richardson has made liberal use. The extracts show that Mr. Sopwith studied closely the currents of scientific opinion, and formed his own judgment about them in a shrewd and independent spirit.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Electric Transmission of Power.

YOUR article of the 1st inst. on the International Electrical Exhibition (p. 522), says: "In those days (before 1879) two wrong notions misled people—the one, that the maximum efficiency of a perfect electromotor could be only 50 per cent.; the other, quoting the remarks of Sir W. Siemens, 'in order to get the best effect out of a dynamo-electric machine, there should be an external resistance not exceeding the resistance of the wire in the machine.'"

These two notions are really one: the first follows by immediate inference from the second.

Your article says a little further on: "At the British Association in 1879, Prof. Ayrton exposed the fallacy of assuming that 50 per cent. was the maximum efficiency theoretically obtainable from an electromotor. . . . This was perhaps the first time that it had ever been suggested that the efficiency in electric transmission of power could be more than 50 per cent."

This is a mistake as to historical fact. Many years ago, I am not sure of the date, but it was long before the dynamo was invented, I had some conversation with the late Prof. Joule about mechanical equivalents and motive power, in which he told me that an electromotor (worked, of course, by a voltaic battery) had shown a very high percentage of efficiency—I think he said 79 per cent., and I am sure it was far above 50. I said, "How is that compatible with Ohm's demonstration that the efficiency of an electric circuit is at a maximum when the resistance of the battery is equal to that of the rest of the circuit?" to which he replied, "The maximum effect, in Ohm's theorem, does not mean the maximum work done by the oxidation of a given quantity of zinc, but the maximum effect obtainable from a given surface of zinc plates." "I see," said I, "just as in the case of the steam-engine, the problem of getting the maximum of useful effect from a given weight of coals is a different one from that of getting the maximum of power from a given area of piston."

This appears to be an instance of a truth being grasped by one of the great masters of science long before it passed into general teaching. And it is also an instance of a truth being so mistaken as to mislead: Ohm's law was evidently understood to bear a significance that it did not really bear.

Belfast, October 13. JOSEPH JOHN MURPHY.

[That Joule had clear and correct views regarding the efficiency of an electromotor driven by a voltaic battery was pointed out some years ago, being mentioned, for example, by Prof. S. P. Thompson in his book on "Dynamo-Electric Machinery." But in the paragraph quoted by Mr. Murphy from NATURE of October 1, the expression "electric transmission of power" had reference to the combination of apparatus exhibited at the lecture in question—had, in fact, the meaning usually attached to this expression, viz. the employment of a dynamo to convert mechanical energy into electric energy at one end of a pair of wires of some length, and the employment of a second dynamo at the other end of the wires to convert the electric energy back again into mechanical energy.

Now, not only would it have been somewhat difficult to foretell what would be the combined efficiency attainable by the employment of two dynamos as generator and motor, at a period "long before the dynamo was invented," but even down to 1879 no one had succeeded in practically transmitting power by means of this combination with an efficiency of as much as 50 per cent. over a distance of even one mile.

The only direct-current dynamo in common use at that date was the series-dynamo, and that machine, as is well known, differs radically in its behaviour from a voltaic battery. For while it is when a voltaic battery is developing a very small current that it gives power most economically to the outside circuit, the series-dynamo, when only a very small current is passing through it, develops practically no electromotive force, no power, and therefore has a very low efficiency. Hence, although electricians were undoubtedly mistaken in fancying that there was a theoretical limit of 50 per cent. in the efficiency when two dynamos were employed in the transmission of power, neither the error, nor its correction, were of that obvious character in 1879 that one might imagine from reading Mr. Murphy's letter.—W. E. A.]

Rain-making.

IN 1883 I published in NATURE (vol. xxviii., p. 83) an account of some experiments which I made to explain the curious phenomenon commonly seen at the Bocca of the Solfatara of Pozzuoli: paper or brushwood is kindled near the fumarole, and the action of the flame, even when its duration has been very brief, is observed for some time after in the relatively great increase of cloudy vapour that appears to roll out of the Bocca and to rise from the surrounding minor fumaroles. According to Prof. Arcangelo Scacchi, this increasd condensation of vapour is due to the carbon dioxide produced in the combustion; this gas causing condensation from the highly saturated medium in the same way as fumes become visible when concentrated hydrochloric acid is exposed to ordinary air. My experiments of 1883 tend to show that not only carbon dioxide, but (in accordance with the views of Dr. Aitken on the formation of cloud or mist) the increase of solid corpuscles made to float in the vapour-laden air inside or near the fumarole, might be the cause of a rapid and continuous condensing of the invisible vapour. I noticed that the "powdering" of the air with any kind of dust increased the cloudy column issuing from the Bocca of the Solfatara. I am therefore led to believe that the action of a paper- or faggot-flame in causing the increase of visible vapour from the Bocca of the Solfatara is due both to the production of carbon dioxide and to the increase of solid particles of soot and of light unburnt fragments made to rise and float in the air.

These experiments may help in explaining the action of explosives in causing a downfall of rain. Not only does the explosion produce a certain amount of carbon dioxide, but dust is widely scattered in the air, and carried upwards by the hot gases produced in the explosion. If the results of the experiments in Texas and Kansas by General Dyrenfurth and Prof. Curtis be confirmed, it would be interesting to see if the condensation of vapour in the atmosphere could be better insured by purposely increasing the quantity of dust produced in each explosion. The effect would perhaps be enhanced if the dust were of a markedly hygroscopical nature: the scattering in high air of very minute particles of calcium chloride should help in the making of cloud and rain. ITALO GIGLIOLI.

Laboratory of Agricultural Chemistry,
Royal Agricultural College, Portici, near Naples,
October 12.

Weather Cycles and Severe Winters.

THE following view of the relations of severe winters is one which I do not remember to have seen stated.

Consider the 79 years 1812–90 (at Greenwich), and let us take, as a measure of winter cold, the mean temperature of the three months December, January, and February. Divide the series of years at 1860; giving a first series of 48 years (1812–59), and a second of 31 years (1860–90).

Now consider the first series. The coldest winter in it is 1813 (meaning, by that, 1813–14). The coldest of the following winters is 1829; the coldest of the following, 1840; then come (reckoning similarly) 1844 and 1846 (equal), 1854, 1859. The absolute order of decreasing severity is to some extent the same, but at certain points the order of time is reversed.

Next take the second series. The coldest winter in this is 1890 (*i.e.* 1890-91); the coldest of those preceding, 1878; the coldest of those preceding, 1870; then come (similarly) 1864 and 1860.

Thus we have a succession of severe winters of decreasing severity, and another, after it, of growing severity.

We may tabulate the data:—

Severe winters with lessening severity.	Mean temperature.	Severe winters with growing severity.	Mean temperature.
1813	31°·9	1860	37°·4
1829	33·2	1864	37·1
1840	33·9	1870	36·4
1844	34·9	1878	34·6
1846	34·9	1890	34·1
1854	35·6		
1859	37·4		

These data, put into the form of a graphic diagram, give a wave whose crest (mildest of the severe winters) we seem to have passed in the sixties. And it would appear judging by the past, that we have not yet reached the bottom of the hollow; but that after some years' interval we may have a winter even more severe than last, possibly we may have more than one, of growing severity.

It is right to state that, as far as 1856, the values of mean temperature used are those of Mr. Belleville, reduced to sea-level, as given in a paper by Mr. Eaton to the Royal Meteorological Society (Quarterly Journal, January 1888); after that date, those of Greenwich Observatory, published annually. The slight difference in kind does not materially affect the result.

In the *Meteorologische Zeitschrift* for September, M. Woeikof considers the question whether winters in Russia have been growing warmer, and his examination of the St. Petersburg records, from 1744 to 1890 (noting the number of cold days), leads to an affirmative answer. The number of very cold days has, on the whole, fallen off considerably in the later sixty-three years compared with the earlier, and in the second half of our century, as compared with the eighteenth and the earlier half of the nineteenth.

This, he finds, corresponds with popular opinion for Northern and Central Russia, according to which intense frosts have become more rare; but in the south, in the Crimea, the Caucasus, and Turkestan, there have been complaints of colder winters of late.

Mr. Glaisher some time ago expressed the view that our winters had been becoming milder. I have seen a criticism of this view, to the effect that the proximity of Greenwich to such a rapidly growing city as London might have to do with such a result. If the facts are as I have suggested above, a growing severity has taken the place of growing mildness, and the criticism referred to would fail to apply. A. B. M.

A Lunar Rainbow.

ON the evening of Saturday, October 17, at about 6.30 p.m., the rare and interesting phenomenon of a lunar rainbow was observed from Patterdale, Westmoreland. On the south-east, the moon, which had just risen, brightened the sky behind the mountains, while on the north-west there hung a uniformly dark and unbroken screen of haze or rain-cloud, which lightened off somewhat and was more scattered on the extreme west. With its highest point lying almost exactly north-west, a semicircle of pale whitish light was projected against this vapoury curtain. The bow was quite complete, but much brighter and sharper on its northern arc than on that falling south. The brighter portion fell over weird and clear into Glenridding (a favourite haunt of sun-painted rainbows), and as seen striped against the dark hill-sides of that valley, appeared to emit a pale-blue phosphorescent glare. At one time a shred of the dark smoky haze scudded over, but did not completely obscure the highest reaches of the spectral light. The radius appeared smaller than in the case of an ordinary solar rainbow, and the breadth of beam was about one-half thereof, or perhaps rather less. The spectacle having lasted for about eight minutes, light rain began to fall, and then the sky in a very short time became quite clear and star-lit, and all was over.

P. Q. KEEGAN.

Patterdale, Westmoreland, October 17.

The Destruction of Mosquitoes.

THE recent mention of this subject in your pages reminds me that I was told a few years ago by an English gentleman who has a most beautiful place on the Riviera that he had freed his property from this pest.

The property in question is a peninsula, and for that reason is exceptionally open to separate treatment. On the Riviera, as many of your readers will know, fresh water is a somewhat rare commodity, and all of it that the inhabitants can lay hold of is stored for future use in tanks or small receptacles.

The larva of the mosquito lives, as I understand, only in fresh water. Consequently, on the Riviera he is found in the tanks I have named.

The carp is, I am told, passionately fond of the larva of the mosquito, and the Englishman I refer to had extirpated the insect by putting a pair of the fish in every tank.

The plan is not one that could be adopted everywhere, but it is worth bringing under the notice of those whose circumstances are like those of the Riviera. S. A. M.

Law of Tensions.

POSSIBLY many science teachers find some little difficulty in satisfactorily demonstrating to a class the "law of tensions" for vibrating strings. In practice, unless the sonometer is fixed vertically, the error introduced by friction at the pulley (especially with heavy weights) is so great that the real tension is very different from that represented by the weight attached. Even if the apparatus be thus fixed, the changing of the weights occupies time, and a comparison wire is necessary, which must first be tuned to exact unison. The following admirable and very simple method was suggested to me by one of my students, and possibly there are some teachers to whom the idea is new.

Instead of applying tension by attaching weights, the result may be effected much more readily by means of an ordinary spring suspension-balance, such as is often used for weighing parcels. By this method the tension may be regulated to within half a pound, and increased or decreased so rapidly that the heightening of pitch is clearly recognized without the use of an auxiliary wire. H. G. WILLIAMS.

Congregational School, Caterham.

The Koh-i-Nur: a Reply.

IT is a far from pleasant task for me to set about replying to Prof. Maskelyne's criticism of my history of the Koh-i-Nur. I desire to say what must be said with all respect for him, but the tone of some of his remarks renders this a task of exceeding difficulty. All I care about is to get at the truth, and in order to do so I have spared neither time nor labour. I cannot suppose that you would grant me space sufficient for answering in detail all the statements in Prof. Maskelyne's article; nor do I seek for such space, because I deem it to be sufficient for those, several of them experts, who have accorded my views their hearty support and approval—Firstly, to state here in a general way that having very carefully studied Prof. Maskelyne's long article it has not, in my opinion, in the very smallest degree shaken the facts I have quoted, and the deductions from them which are to be found in my appendix to "Tavernier's Travels," and in the article published in the April number of the *English Illustrated Magazine* of the present year. Indeed, I might go further, and say that this attack very materially confirms the strength of the position upon which I have taken my stand. Secondly, I shall select a few points only which afford clear issues without any mystification, as to which side the balance of evidence lies upon, and invite readers to draw their own conclusions.

Before going further I think I should recall to notice the review of my edition of "Tavernier" which appeared in NATURE last February (vol. xliii. p. 313), and the *English Illustrated Magazine* for April, from which it will be seen that a suggestion made in the review has since been acted upon, with the result that was anticipated.

Prof. Maskelyne states that there is an absence of novelty in my facts. Just so, it is the old facts that I rely upon, not the misquoted and distorted variants which are to be found in so many writings. In my earliest allusions to this subject, many years ago, I made some mistakes, from blindly following authorities whom I now know to have been misled as to their facts. Since then I have learnt how necessary it is to check all statements as of fact in reference to this subject, and not to place too implicit a trust on quotations, no matter how eminent the authority who makes them may be.

Is it conformable to the judicial position which Prof. Maskelyne claims to occupy, to say that I dismiss Prof. H. H. Wilson, and what he narrates, "by the somewhat flippant remark that 'it has afforded sundry imaginative writers a subject for highly characteristic paragraphs'"? the facts being these—I never referred to Prof. H. H. Wilson; I did not even know before that he was the writer of the anonymous note in the official catalogue; and more than that, I had not that particular contribution to the subject in my mind when writing the above words.

Still further, with regard to the judicial position, I do not think it is apparent in any of Prof. Maskelyne's subsequent remarks. They are those of an advocate who smites his opponent in season and out of season, and seeks to disparage him by implying that he has assaulted the reputation of men (whom all must honour), when he has merely pointed out misquotations in their writings and expressed dissent with their conclusions.

I yield to no one in my admiration for the late Mr. King's work, but this cannot and should not restrain me from pointing out misquotations and misprints in his books when treating of the subjects with which he has dealt. To justify this I shall quote but a few instances which I have noticed, out of many. On pp. 78 and 82 ("Natural History of Precious Stones," Bohn's edition, 1870) the weight of the Mogul's diamond is stated as on Tavernier's authority to have been 240 carats and on the plate 208 carats, instead of 279$\frac{9}{16}$ carats.

The Koh-i-Nur is stated on p. 82 to have weighed 184 carats instead of 186$\frac{1}{16}$, and, strangest of all, when recut, that is to say in its present condition, its weight is given, pp. 75 and 347, as 102½ and on the plate as 102¼ carats, whereas its true weight is 105$\frac{1}{16}$ carats.

On p. 68 he deduces an argument from the note by Clusius, which is referred to by Prof. Maskelyne, and given in the original in my paper; the whole force of his argument depending, however, on the change of the word Belgium of the original to Europe in his, Mr. King's, own rendering of it.

I might add to this list, but sufficient has been stated to show that such statements require the most careful scrutiny, by whomsoever they may have been made.

On pp. 81–82 will be found Mr. King's dissent from Prof. Maskelyne's theory about the identity of Babar's diamond with the Mogul's; the difference of opinion between them being very wide indeed, though Prof. Maskelyne does not think it necessary to refer to it in his article.

With reference to what Prof. Maskelyne writes about De Boot and Garcia de Orta, I shall only say that I am very well acquainted with both authors' works, and that I assert again that the statement wrongly attributed to Monardes, and quoted as from Mr. King by Prof. Maskelyne, was an unsound and dangerous link in the chain by which it was proposed to connect Babar's diamond with the Koh-i-Nur.

It was a statement convenient to use, but what if I had used it first, and had also misquoted the authority? Would the terms Prof. Maskelyne employs about my aberration, &c., have been considered strong enough? There was, however, no aberration whatever on my part, and Prof. Maskelyne has himself now fully demolished, as anyone may read, the authenticity of the link he formerly used as a very material element in his chain. How can he, then, still cling to the fragments of this shattered link, while he dismisses so peremptorily Malcolm's statement about the weight of the Darya-i-Nur? Will he ever again use that link, or quote Monardes as his authority? (*Edinburgh Review*, vol. cxxiv., 1866, p. 247.)

I still venture to think that my conclusion as to the kind of carat used by Tavernier is a legitimate one. At the end of chapter xviii., book ii., he says, where computing from their weights the values of diamonds to a *liard*, "le Diamant du Grand Mogol pese 279$\frac{9}{16}$ carats" (*sic*); and in the very next paragraph, "le Diamant du Grand Duc de Toscane pese 139½ carats."

True it is, as pointed out by Prof. Maskelyne, that Tavernier in some other passages defines the carats as "nos carats"; he does not say, however, "carats de France," and the meaning therefore I take to be the carats employed by himself and his confraternity as contrasted with Indian measures of weight.

The value of the *abbas* or pearl *ratti* of 2·66 grains, or seven-eighths of the Florentine carat, has also been approximately arrived at by other relations given by Tavernier; conversely, therefore, it proves his carat to have been the Florentine.

I know of several early writers who have written about the Grand Duke's diamond, and by them Tavernier is referred to as *the* authority for its weight, which, as even Prof. Maskelyne admits, was given in Florentine carats. I think all the circumstances justify the belief that it was probably weighed by Tavernier himself with his own weights and scales. Now as to the weighment of the Mogul's diamond, in one passage Prof. Maskelyne (p. 557) states that Tavernier does not say he weighed any of the stones, and, in another, on the same page, "The diamond Tavernier saw, weighed, he said (was he merely told so or did he really weight it?), 319½ ratis."

The pages of Tavernier give the following very explicit answer to this query. He says, "Ce diamant appartient au Grand Mogol, lequel me fit l'honneur de me le faire montrer avec tous ses autres joyaux. On voit la forme où il est demeuré étant taillé, *et m'ayant esté permis de le peser jay trouvé qu'il pese 319½ ratis qui sont 279$\frac{9}{16}$ de nos carats.*"

This is precise evidence enough that he did weigh the stone himself, and if the carats were French instead of the lighter Florentine carats, which I believe them to have been, the stone was so much the heavier, and therefore still more removed in weight from Babar's stone.

Tavernier, I must remind the reader, besides Bernier, is our only authority for what is known about the Mogul's stone, as such, and what I have protested against and still protest against is, the suppression or rejection of such precise statements as the above, while others of his which fit in with particular theories are accepted.

In various directions I have been enabled to show Tavernier's minute accuracy about matters not connected with his trade as a jeweller, and when he speaks as an expert, in the practice of his own profession, he deserves, and proves that he deserves, a very different treatment from that which he has received. It is for this reason, and not because I am blind to his faults, that I give him my loyal support. I have already, in vol. ii. of "Tavernier's

Travels," stated that some corrections of values given in vol. i. are required in consequence of the identification, made too late for their correction, of the value of Tavernier's carat, but the present discussion as to the Koh-i-Nur is quite independent of that.

With regard to the mutilated condition of the Koh-i-Nur, I have nothing to add; the statement as to its condition, quoted by me, and the figures and models of the stone appear to be sufficient proof that portions had been removed by cleavage, which would account for the difference between its weight and the Mogul, as described by Tavernier, and I still retain that opinion.

It is not of the least importance as regards the main question, whether my suggestion should prove correct or not, that if Babar's stone has survived it may be identical with the Darya-i-Nur, to which Malcolm attributed a weight of 186 carats. Prof. Maskelyne, upon a system of calculation which I cannot admit as applicable to the case, as we do not know the thicknesses of the stones which he compares, gives to the Darya-i-Nur an estimated weight of 210 carats. For the present, therefore, I prefer Malcolm's definite statement to Prof. Maskelyne's theory about the attributed weight being the "echo associated with the Koh-i-Nur."

I shall have something to say about the Golconda table diamond, and about a great many other diamonds and other precious stones too, on a future occasion. In that work I shall be as careful to give, as I have hitherto been, chapter and verse for every statement of fact quoted, and I shall trust the histories so supported will find acceptance from those who care to investigate the evidence in favour of the conclusions connected therewith.

I am not quite sure that I appreciate the full force of the phrase "verisimilitude of a true history"—the last words of Prof. Maskelyne's article—but of this I am certain, that if ever I should see a history of the Koh-i-Nur following the lines of that article, I shall feel bound to make another and special "incursion" into the subject in defence of Tavernier if not of myself.

Dublin, October 12. V. Ball.

THE NAUTICAL ALMANAC.

It has been known for some little time that Dr. John Russell Hind, F.R.S., who for many years past has been responsible for the production of the national ephemeris, would soon seek that retirement to which his long services and his distinguished career entitle him. At the end of the year, he will relinquish the office of Superintendent of the "Nautical Alamanac," and the good wishes and kindly sympathy of the astronomers of many nations will follow him in the retirement he is seeking.

His successor has been appointed, and in Mr. A. M. W. Downing we have not the slightest doubt that the Admiralty have made a happy selection, and that under his auspices the high character and reputation of the "Nautical Almanac" will be fully maintained. Mr. Downing has long been associated with meridian astronomy in its best traditions; and in his position of greater responsibility and greater freedom we entertain the hope that his astronomical reputation will be fully maintained and extended. He may be said to enter on his office at a time when the "Nautical Almanac" is on its trial. The arrangement of the book, and the information it conveys, were practically settled by a Committee some sixty years since. How efficiently that Committee performed its task is shown by the fact that so little alteration has been needed for so long a period. But the outcry for change has gone forth: new committees are deliberating and reporting, and it will be among Mr. Downing's first duties to give shape, alike to the suggestions of irresponsible authorities, as well as to incorporate the recommendations of recognized committees in a new and improved "Nautical Almanac."

One great difficulty which has to be encountered, and of which it is not easy to see the proper solution, is due to the fact that the "Nautical Almanac" seeks to supply the wants of two very different classes of persons—namely, astronomers properly so called, and nautical men. The former demand very considerable detail in the exhibition of the several computations, the latter are satisfied with a very few final results. The former class is a small one, and a very moderate edition would satisfy their demands. The latter class is a very large one, and necessitates the printing, it may be, of thirty or forty thousand copies. The first question therefore, it seems, which must claim the attention of any Committee, or of any Superintendent, is, whether it be desirable to separate the "Nautical Almanac" into two, or it may be more, sections—one circulating among astronomers, the other among mariners. Private enterprise, anxious to minister to the wants of a rapidly increasing mercantile marine, has long supplemented the "Nautical Almanac" with a smaller and pirated edition, valuable to sailors, but detrimental to the circulation of what may be considered the legitimate ephemeris. Would it not be better if the Admiralty could see their way to publish an ephemeris with other nautical information, entirely for the use of the marine? Such a course is followed by the Governments of other countries. The German Government publish at Berlin a compact "Nautisches Jahrbuch," admirably adapted for naval purposes. This example is followed in Austria and in America, and we believe that the sale of our "Almanac" to the naval men of those countries has fallen off in the last years, or at least has not kept pace with the increase of foreign tonnage.

Such questions are of importance, as concerning not only the financial position of the work, but its influence in our own and foreign navies. There are, however, others touching the scientific and purely astronomical side of the compilation. Such, for instance, is the vexed question of the introduction of empirical terms in the final positions of the moon. Astronomical purists will maintain that the position of the moon should be that assigned by a purely gravitational theory, to facilitate the comparison of that theory with observation. Others demand that the place of the moon should coincide, as accurately as possible, with observation; and looking at the large portion of the "Nautical Almanac" devoted to "lunar distances," it would seem (if this section is ever used) that it is desirable that the distances given should represent observed facts. After a naval man has been at the trouble of observing and reducing a lunar distance, to ask him to apply a correction for the error of moon's place seems wanton and irritating. And if the amount of the empirical correction is clearly ascertainable, it can be easily removed before instituting a comparison between observation and that theory from which the moon's place has been computed. But to satisfy the demands of both classes of astronomers will try the tact and ability of the new Superintendent to the utmost.

The section devoted to the apparent places of the stars has also been submitted to considerable criticism. No doubt here enlargement is needed, and possibly improved places of the stars, particularly of circumpolar stars in the southern hemisphere, are much wanted. But on this point the new Superintendent is himself a weighty authority. He has worked much and successfully in the determination and removal of systematic differences from star catalogues, and their reduction to known and recognized standards. So that, under his influence, we may hope that this section will take and maintain a foremost position.

Mr. Downing has undertaken a very important duty, of great national importance, at a very critical period. We fully believe that he will grapple with this task successfully, and that, in his efforts to improve our ephemeris, he will have the assistance and support of all classes of astronomers.

RAIN-MAKING IN TEXAS.

IN NATURE of September 17 (p. 473), Mr. H. F. Blanford has discussed at considerable length the rain-making experiments in Texas, on the basis of such information as was attainable from newspaper reports. Inasmuch as these telegraphic reports have not only been inadequate, but also frequently inaccurate and misleading, the writer, who was the meteorologist of the Expedition, is led to give the following brief summary of the experiments and their results.

The experiments, which have been quite independent of the direction or patronage of the Weather Bureau, have been carried on by the Hon. R. G. Dyrenforth, Special Agent appointed by the Department of Agriculture. The plan of exploding oxy-hydrogen balloons was adopted as one of the principal methods to be employed, and several months were spent in preparing the necessary materials and apparatus. Preliminary experiments made in Washington demonstrated that a tremendous concussion could be produced by the explosion of balloons 10 feet in diameter, filled with a mixture of hydrogen and oxygen in the ratio of two to one. In addition to the explosion of balloons, preparations were made to fire sticks of dynamite carried up in the air by kites, and to explode rackarock (an explosive consisting of three parts of potassium chlorate to one part of nitrobenzol) and dynamite on the ground.

With materials for carrying out these three lines of experiment, the party went to an isolated ranch twenty-three miles north-west of Midland, Texas (lat. 32° 14′, long. 102° 12′). The inauguration of the experiments attracted great attention throughout the whole south-western section of the country, and, locally, people went from all the surrounding counties to witness the operations. Actual trial in the field soon developed the fact that the preparations for the balloon experiments were entirely inadequate. Accidents occurred to the furnaces for generating the gas, which took much time to repair, windy weather prevented the filling of the balloons, and a combination of other sources of delay rendered this line of experiment a practical failure. One or two balloons were exploded on several days, but these were too few in number and too infrequent to serve the purpose of an adequate experiment. Similarly it was found impossible with the small available force to operate the kites to advantage, and in windy weather they were quite unmanageable; so that, although, in all, quite a number of dynamite sticks were fired in the air in this way, yet as a line of effective experiment this also proved a failure. The only explosions that were made on a scale even approximately commensurate with the requirements were those of rackarock, and it may be stated that all the effective operations essential at Midland can be duplicated in every essential particular with 1500 pounds of rackarock together with 500 feet of wire and a small portable dynamo.

The first rain that occurred after the party reached Midland began shortly after noon on August 10, and continued at intervals until evening. The amount of rainfall was not measured, but it was stated in the language of the country to be a good "grass rain." The writer, who was *en route* to Midland, met similar sharp showers in the latter part of the afternoon near Sweetwater, 100 miles to the eastward. On the preceding evening some preliminary explosions had been made, but only on a small scale, and no result was anticipated. In the telegraphic despatch that was sent reporting the rainfall, no causative action was claimed—in fact, such action was explicitly disclaimed in the telegraphic report, which stated "we do not think the explosions actually produced the storm, as they were not on a large enough scale. The preliminary trial was made simply to test the efficiency of the special blasting powder." The firing, which was not over half-a-dozen blasts, was, then, simply a preliminary trial of material, and not in any sense an experiment to produce rain.

On August 16, 17, 18, and 20, cloudy weather very largely prevailed, and numerous thunderstorms were seen on the horizon that did not visit the ranch. On each of these days blasts of rackarock and of dynamite were fired while heavy cumulus or dense storm-clouds were in the field. In several instances, when a dense threatening cloud was overhead, a sharp detonating explosion of rackarock or of dynamite was followed at an interval of 30 to 40 seconds by a spatter of rain, or, if it was already sprinkling, the blast was followed by a very noticeable increase of the drops. This interesting result occurred a sufficient number of times to indicate that the phenomenon was a real effect of the explosions. On none of these days, however, was the amount of rainfall appreciable, except on the 18th, when it was two-hundredths (0·02) of an inch. The 18th opened cloudy, and old settlers predicted rain for the afternoon, whether the experiments should be made or not. To what extent, therefore, the explosions that were made were influential in producing the 0·02 inch that fell is obviously very difficult to determine, and as an evidence of the efficacy of the explosions it is practically valueless.

The next explosions were on the evening of August 21, when 156 pounds of rackarock were fired in 14 blasts. During the night a genuine norther came on, the wind blew from the north, the barograph curve rose rapidly, the temperature fell rapidly, and during the next forenoon a fine mist prevailed. This change of weather was quite extraordinary and unexpected, and with its accompanying mist was attributed to the heavy firing of the evening previous; but the norther had been on its way for several days, and the fine mist was evidently due to the uplifting by the cold north wind of the warm moist air of the plains. At numerous points in the State where the air was more humid a heavy rainfall occurred.

The last experiment, which in magnitude was the greatest of all, took place on the evening of August 25, after the writer had departed. The conditions were thought to be extremely unfavourable for rain, and the party was advised to wait for a more propitious occasion. The firing, however, was carried on until 11 p.m., when the party retired for the night. It is reported that "at 3 a.m. the heavy rolling of thunder disturbed the sleepers, heavy banks of clouds were seen advancing, almost constantly lighted by most brilliant lightning. An hour later the rain began to fall in torrents on the ranch, and did not cease till 8 a.m." Unfortunately, records of the *amount* of rainfall have not yet been received, but I am informed, by a gentleman who was present, that "it was nothing but a sprinkle." Further light is thrown on this rainfall by the weather map for 8 p.m. eastern time, of August 25. Rainfall is shown in New Mexico to the north-west of Midland, Texas, and the forecast officer made the following prediction: "For Eastern Texas, generally fair, except *local showers* on the extreme south-east coast *and the north-west*." Here we have an official prediction made in Washington City of probable showers over the district in which the experimenters were operating, and for the very night in which the thunderstorm followed the last of the explosions to produce rain.

In view of these facts, it is scarcely necessary for me to state that these experiments have not afforded any scientific standing to the theory that rain-storms can be produced by concussions. But, if the adherents of the theory maintain that "no experiment has been tried that is worthy of the name, and that no results ought to be looked for," it will be difficult to take opposite ground.

GEORGE E. CURTIS.

Smithsonian Institution, October 9.

COLOUR-BLINDNESS GENERALLY CONSIDERED.

COLOUR-BLINDNESS has now passed from the category of ailments denominated interesting, and is recognized as a visual infirmity the importance of which cannot be over-estimated. Before entering upon a discussion of the subject it will be well to lay down a definition of colour-blindness that shall run on all fours with the latest scientific findings in the matter. Colour-blindness is merely the inability of the eye to recognize the quality of the light that falls upon it, *i.e.* to discriminate between ether waves of varying refrangibility, the impingement of which upon the retina conveys to us the sensation of colour. Total colour-blindness is the inability to distinguish any colours. To a person so afflicted all bodies are either black, or white, or grey, according to the intensity of the light reflected from them. This form of the disease is very rare. Colour-blindness in ordinary is merely a question of degree, no two persons having exactly the same colour perception. A popular, but erroneous, belief respecting human vision is that good eyesight, *i.e.* accurate perception of form and distance, carries with it a keen perception of colours. This belief is deeply rooted, the impression that colour perception is an integral part of *good* eyesight being of almost universal adoption. The eye, however, that has the most perfect appreciation of form and distance *may* utterly fail to discriminate between two differently coloured objects of the same shape, and placed at equal distances from the observer. In this case a variation in the intensity of the light reflected from the objects under view would enable the colour-blind to discriminate between them, for along with colour ineptitude there generally exists the most delicate sense of discrimination as to the relative intensities of two sources of light.

The majority of people are undoubtedly afflicted with a mild form of colour-blindness. They are physically incompetent to differentiate exactly between the nicer shades of the more composite colours, such as browns, greys, and neutral tints. Yellow would appear to be the colour that gives least trouble to the colour-blind, and blue, if strongly illuminated, is readily recognized.

Red would appear to be the colour the want of the sense of which may be said to be characteristic of colour-blindness; and as a person blind to red is usually blind to its complementary colour, green, ordinary colour-blindness may thus be defined as the inability to discriminate between red and green. The normal eye would appear capable of analyzing white light into three coloured elements, one of which is red; the colour-blind eye, on the other hand, analyzes white light into two elements, neither of which is red. Why this visual defect should manifest itself in inability to distinguish that part of the spectrum which is the result of the slowest of the series of ethereal undulations is by no means clear. Physiological knowledge as to the exact relationship between external colour factors and our mental idea of colour is yet in its infancy. A consensus of opinion would, however, appear to obtain, that, of the rods and cones to which the nerve terminals of the retina are generally compared, the latter are responsible for the processes of analysis by which a compound ether wave is decomposed into its constituent elements, each of which produces an influence upon a corresponding nerve fibre. That the rods and not the cones are least responsible for our sensations of colour would appear to be borne out by the fact that among predatory animals, to whose nocturnal habitat a colour percipient apparatus would be an unnecessary adjunct, the cones are wanting, while the rods are very highly developed. Which theory may be ultimately accepted as best explaining the varied phenomena of colour-blindness is at present matter of speculation.

Authorities on colour-blindness are, however, agreed that in the majority of cases it is congenital; that to a great extent it is amenable to the same laws that govern the transmission of other hereditary tendencies; and while in some very few cases where it is induced by accident, such as concussion of the brain, or is the residual product of some malady or alcoholic excess, it may be palliated, yet colour-blindness is absolutely incurable.

The knowledge that something like 3 to 4 out of every 100 of our adult population are afflicted with colour-blindness is of serious importance, and statistics show that this is no over-estimation of the case. The following table shows the percentage obtained from a large number of cases:—

Examiner.	Number Examined.	Number Colour-blind.	Percentage.
Holmgren	32,165 men ...	1,019 ...	3·168
Dr. Joy Jeffries ...	18,556 ,, ...	764 ...	4·117
London Committee ..	14,846 ,, ...	617 ...	4·156
Total ...	65,567 men	2,400	3·66%

The percentage of female colour-blinds is much less. My own findings show 0·162; Dr. Joy Jeffries, however, found a lower proportion than this, as among 14,557 females tested only 11 were colour-blind.

This great disparity between the numbers of the colour-blind in the two sexes has been long known, and various causes have been assigned as accountable for it. As far back as 1855, Prof. Wilson, while admitting the superiority of colour perception in the female, could not believe that the number of colour-blind women were so few as compared with the number of men similarly afflicted. He took up the view that women were not so willing to be tested as men, so that unless they were members of some public institution it was quite a voluntary matter whether they were tested or not. He argued, too, that women attached greater importance to perception for colour than men do, and would consequently strive to screen their defect from others. Thus the only women who would voluntarily submit to be tested for colour would be those who had no doubt but they were possessed of perfect colour-vision.

Unfortunately, however, this method of reasoning is based upon an hypothesis altogether fallacious. Colour-blind people do *not* of themselves realize their condition. They cannot tell that there is any difference between red and green as they see them, and red and green as viewed by the normal eye.

The fact that females have more practice in handling colours than males is not sufficient to account for this disparity, unless we assume that the present condition of the female colour percipient is the resultant of the gradual development and training transmitted through ages of time. The superior colour percipience of the female must, we believe, be regarded as an inherent quality of the sex, which no amount of individual artificial training and practice can attain to.

There is just one thing, however, which may discount a little this feminine superiority. As colour-testing was first applied, too much importance was attached to the correct *naming* of colours, and as this is a province in which the masculine section of humanity is decidedly inferior, the ratio of male to female colour-blinds may have been increased in consequence. Assuming that the percentage of 3·66 of adult male colour-blinds is correct, we are confronted with the fact that there are over 4000 colour-blind seamen in the British mercantile marine. This number is exclusive of pilots, canal or lightermen, and firemen. Now, all of the 4000 are liable to be called upon to officiate as look-outs, *i.e.* they may be placed in circumstances where it is necessary they should distinguish instantaneously between the colours of the regulation side-lights of an approaching vessel. As far back as 1877, the Board of Trade, acting in accordance with the recommendation of the ophthalmic section of the British medical profession, came to the conclusion "that all candidates for masters' or mates' certificates shall pass a test examination as to their ability to distinguish the following colours, which enter largely into combination of signals by day or night used at sea; viz. black, white, red, green, yellow, and blue"; and they state that "the Board have been led to this decision because of the serious consequences which might arise from an officer of any vessel being unable to distinguish the colour of the lights and flags which are carried by vessels."

So far so good. But there the matter stopped. An officer failing to pass in colours is not deterred from going to sea; his certificate is simply endorsed "*failed to pass in colours*," and then it is optional with the owners, *if* they know of a man's colour imperfectness, to engage him or not. In the majority of cases they do not know. Wishing to obtain accurate information as to the views of the Liverpool shipowners upon this subject, I submitted to them the following queries:—

(1) Do you consider a colour-blind officer, mate, or captain, competent to have command of a vessel, steam or sailing?

(2) Would you consider a colour-blind man fit to be a look-out man?

In reply, 110 firms answered both questions in the negative, while *one* answered both in the affirmative.

Six said "Yes," to the first query, and "No," to the second.

Six expressed the opinion that no colour-blind officer should have command of a vessel; but that colour-blindness was not a barrier to a seaman officiating as look-out.

The language of the firms that answered both questions in the negative was such as to show that there was not the slightest hesitancy in the minds of the writers as to the utter undesirability, not to say danger, of employing a colour-blind man in any capacity in which he was responsible, in part or whole, for the safe navigation of the vessel.

Such expressions as "emphatically no," "absolutely unfit," "not fit to serve on a ship," "very unsuitable," &c., show in unmistakable terms the views held by Liverpool shipowners on the subject.

Liverpool shipowners certainly seem alive to the dangers of colour-blind *employés*. The practice of private examination would seem to be coming into common practice among first-class firms. But the Board of Trade have still to realize that look-out men, as well as officers, should not suffer from colour-blindness. If shipowners themselves deem it necessary for their own interests, and the safety of the *voyageurs* and property intrusted to their care, to debar colour-blind seamen from their service, it is surely incumbent upon the Board of Trade, in the interests of the travelling community over whose welfare they are supposed to preside, to make perfect colour-vision a *causa sine quâ non* that shall apply to *all* seamen of our mercantile marine. It is but fair, however, to that complex and overburdened instrument of government to add that they have introduced a so-called voluntary test, whereby a seaman, on payment of a fee of 1*s*., may be tested as to the perfectness of his vision for colour. Such a test must, from the very necessities of the case, be absolutely worthless. What A.B. would be likely, had he the slightest suspicion of his colour-blindness, to seek that confirmatory evidence which would debar him from following his calling? Sailors may be pardoned if they prefer to remain in a state of blissful ignorance as to their colour-vision, since they have nothing to gain, and possibly everything to lose, by undergoing an examination in colours. It must be admitted, however, that there are not wanting those who aver most positively that colour-blindness is not responsible for maritime disaster of any description whatever.

Rear-Admiral P. H. Colomb is of this opinion. In discussing the action of the Washington International Maritime Conference relative to colour-blindness, he stated, "I never knew myself a case of collision where colour-blindness was in question. The statements were generally perfectly clear that wrong helm was given deliberately in the face of the colour seen, and as no authoritative teaching had existed to show that it mattered what colour was seen as long as danger was denoted, I have never been able to lay stress on the colour-blind question."

Again, Admiral Colomb expressed the opinion "that collisions at night occurred through the helm being ported to the green light, and starboarded to the red light."

Undoubtedly this is a fertile source of disaster, but seamen, unless we assume them wilfully negligent, or astoundingly nervous, could hardly fail to act correctly at the critical moment in so many instances, if there were not some other factor at work which brought them to grief. I admit the truth of Admiral Colomb's statement as to collisions at night occurring through the helm being ported to the green light, and starboarded to the red. But I would go further, and inquire why such a wrongful procedure should be adopted in so many cases. I cannot believe it is done wilfully with the intent of causing collision, I cannot accept nervousness on the part of men, many of whom have spent a lifetime at sea, as the sole, or even a likely cause. I believe that in many cases the reason why the helm is ported to the green light and starboarded to the red light is that the persons responsible for the porting and starboarding are visually incapable of differentiating between one colour and the other.

Admiral Colomb's cause is undoubtedly the immediate means of effecting the collision; but that cause traced to its original source will, in the majority of cases, show neither negligence nor nervousness, but will stand revealed as the inevitable resultant of eyesight that cannot distinguish red from green. Pronouncements such as those quoted above, coming from those in high places, and pregnant with the weight of authority that usually attaches to such utterances, are mainly responsible for the general laxity and half-heartedness which are so characteristic of the Board of Trade's officials in respect to colour-blindness. A perusal of the records of inquiries into collisions at sea, or of the courts which settle questions of maritime and commercial law arising therefrom, reveals an astounding amount of conflictory evidence as to the relative positions of the colliding vessels as judged by their side-lights. It would be more charitable to suppose that the witnesses examined were colour-blind, rather than guilty of wilful and deliberate perjury. In such cases the question of a look-out's colour percipience is never discussed. An examination of the witness *on the spot*, as to his capability of discriminating between the port and starboard lights of a ship, would set at rest the question of his physical competence to assist in elucidating the problems under consideration.

The Dutch Government has long been alive to the dangers accruing from *induced* colour-blindness—I use the term induced in contradistinction to congenital—and adopt the most drastic measures to prevent a colour-blind officer from holding a position in their mercantile marine. Among other qualifications necessary to procure a warrant empowering a man to act as mate in the merchant marine, the royal order requires:—

"Colour perception perfect for transmitted light in one eye, and at least one half in the other, according to Donders's method."

Also that "the report and declaration of the expert, as required in the above, shall be considered valid for one month only from the time the test is made."

In Holland the tests are made by experts. In England they are applied by persons who, however well they may be qualified to examine candidates in navigation and seamanship, have certainly no *locus standi* in the matter of reporting upon the perfectness, or otherwise, of a man's visual organs.

The tests themselves that these navigation examiners have to apply are far from being perfect. They are established upon a wrong principle. Candidates are made to name colours, and according to the Parliamentary Report of 1887 "the only reasons for which they are reported as having failed are inability to distinguish red from green, and either from black by daylight, and red from green and either from ground glass by artificial light."

Candidates are first required to give correct colour names to a series of eight cards coloured black, red, green, pink, drab, blue, white, and yellow, respectively. A candidate is passed, however, if he names correctly the first three.

The second test consists in naming the colours of glasses some eleven in number, viz. ground glass, standard red, pink, three shades of green, yellow, neutral tint, two shades of blue, and white. The candidate need, however, only name the ground glass, the standard red, and the standard green.

Clearly, with such tests as these, the colour-blind may easily escape detection.

The Board of Trade return relative to colour tests for the year ending May 31, 1891, shows that out of 4688 candidates who presented themselves for masters' and mates' certificates, 31 were rejected on account of deficient colour sense. That these should be rejected after serving an apprenticeship to the sea, is manifestly unfair. The test should be applied at the commencement of their nautical career, and not when the initial stage is passed. Four of the 31 were reported as passing on subsequently undergoing examination, although medical expert opinion is emphatic in stating that colour-blindness is absolutely *incurable*. Perhaps it may be that the examiners were disposed, by their leniency in passing young men whose previous "failure in colours" proved them colour-blind, to atone in some slight form for the bad system which allows lads to spend the best years of their life in mastering the irksome details of a profession, before it informs them that they are visually unfitted for it. It is to be hoped that the investigation into the whole system of colour-testing at present being conducted by a committee appointed by the Royal Society, may lead to thorough and effective reforms.

T. H. Bickerton.

ON VAN DER WAALS'S TREATMENT OF LAPLACE'S PRESSURE IN THE VIRIAL EQUATION: A LETTER TO PROF. TAIT.

MY DEAR PROF. TAIT,—I gather from your letter of September 28 (Nature, October 8, p. 546) that you admit the correctness of Van der Waals's deduction from the virial equation (1) when the particles are infinitely small, in which case

$$\left(p+\frac{a}{v^2}\right) v = \tfrac{1}{2}\Sigma m V^2 \quad . \; . \; . \; . \; . \quad (1)$$

a representing a cohesive force, whose range is great in comparison with molecular distances; and (2) when, in the absence of a cohesive force, the volume of the particles is small in comparison with the total volume *v*, in which case the virial of the repulsive forces at impact gives

$$p(v-b) = \tfrac{1}{2}\Sigma m V^2 \quad . \; . \; . \; . \; . \; . \quad (2)$$

For hard spherical masses, the value of *b* is four times the total volume of the sphere. But you ask, "How can the factor $(v-b)/v$, which Van der Waals introduces on the left (in the first case) in consequence of the finite diameters of the particles, be justifiably applied to the term in K (or a/v^2) as well as to that in *p*?"

In my first letter I desired to avoid the complication entailed by the consideration of the finite size of the particles; but it appears to me that the argument there given (after Van der Waals) suffices to answer your question. For, if the cohesive force be of the character supposed, it exercises no influence upon any particle in the interior, and is *completely* accounted for by the addition to *p* of a/v^2. In so far, therefore, as (2) is correct when there is no cohesive force, the effect of such is properly represented by

$$\left(p+\frac{a}{v^2}\right)(v-b) = \tfrac{1}{2}\Sigma m V^2 \quad . \; . \; . \; . \quad (3)$$

in which *b* is to be multiplied by a/v^2, as well as by *p*.

Yours very truly,

October 13. Rayleigh.

NOTES.

At the Royal College of Physicians, on Monday, when the Harveian Oration was delivered by Dr. W. H. Dickinson, the Baly Medal was given to Prof. Michael Foster for distinction in physiology; the Morgan Medal to Sir Alfred Garrod for distinction in clinical medicine.

Dr. Dickinson, in the Harveian Oration, presented an admirably clear and vigorous account of Harvey's great discovery, and of the scientific results to which it has led. The earliest and most important of these results was the completion of Harvey's work by the discovery of the capillary system by Malpighi, who was born in the year in which Harvey published his famous treatise. "Harvey," said Dr. Dickinson, "had never seen a capillary, nor did the state of the microscope in his time allow of it. He was fain to conclude that the blood passed from the arteries to the veins partly by anastomoses but mainly by percolation, as water, to quote his own illustration, percolates the earth and produces springs and rivulets. Had it been possible, we may imagine the delight with which he would have witnessed the completion by vessels of his circular route." Dr. Dickinson also referred, among other results of Harvey's discovery, to embolism, and to our knowledge of inflammation, or at least as much of it as concerns the capillaries. In conclusion, he said:—"Knowledge has been advancing since Harvey's time in many and independent lines; the achievements of Bell, Bright, and Addison had no direct connection with his, but it is not too much to assert that the medicine of to-day is scarcely less permeated with the results of Harvey's discovery than is the human body with the circulation he discovered. It does not make him small to say that what he found out must have come to light had he never lived. If Columbus had not discovered America some one else must have done so before now. The law of gravity might even have been revealed in the fulness of time to another if not to Newton. But the discoverer is before his time; in this lies one measure of his praise; another, and a more important one, is in the results of his discovery."

The Electrical Exhibition, to be opened at the Crystal Palace on January 1 next, promises to be one of great interest and importance. The requests for space—which already exceed a total of 200—include electric lighting plants for country and town houses, for mines, for steamships, for railway trains, and even for private carriages. There are also included the newest forms of motors, generators, accumulators, and other machinery employed for producing and storing electricity. Several of the more important exhibits at the Frankfort Exhibition will be transferred to the Crystal Palace. The apparatus section will include a complete set of Sir William Thomson's standard electric instruments, new electro-medical and electro-thermic apparatus, the latest improvements in telephony and telegraphy, and also the most recent electrical appliances for war purposes, blasting, signalling, &c. Special buildings are now in course of erection for boilers and other heavy machinery.

The Municipality of Genoa has voted the sum of 15,000 lire in aid of the International Botanical Congress which is to be held in that city in September 1892 to celebrate the fourth centenary of the discovery of America.

The French Association for the Advancement of Science will meet at Besançon in 1893.

The Russian Geographical Society has awarded its great Constantine Medal to Prof. Sludsky for his researches into the figure of the earth and his geodetical work generally. Another Constantine Medal has been given to Prof. Pontebnya for his researches into the ethnography and the languages of the Great Russians, the Little Russians, and other Slavonians. His two works on the Russian grammar far surpass all previous works of the kind, not only in the number of examples but in the novelty and importance of his conclusions as to the structure of the Russian and other Slavonian languages; while his works on Great and Little Russian folk-lore are full of new and profound observations. The Count Lütke's medal has been awarded to S. D. Rylke for an elaborate work on the determinations of longitudes in Russia by means of the telegraph; the probable error of the chief determinations does not exceed 0·016 of a second of time. Another work of the same geodesist deals with the possible errors of levellings, as dependent upon temperature; they appear considerably to exceed those admitted in the best treatises on this subject. We also learn from Mr. Rylke's researches that the level of the Baltic Sea, as deduced from long series of observations, regularly sinks in the direction from north to south. Other gold medals have been awarded to Rovinsky, for a work on the geography and history of Montenegro; to M. Filipoff, for researches into the changes of the level of the Caspian Sea; to M. Obrutcheff, for a geological and orographical sketch of the Transcaspian region; and to M. Priklonsky, for a work on the Yakutes. Some silver medals have been awarded for works, chiefly in ethnography, of minor importance.

Dr. A. R. Forsyth, F.R.S., and Dr. M. J. M. Hill have been nominated to fill up the vacancies caused by the retirement of Dr. Hirst, F.R.S., and Mr. Lachlan from the Council of the London Mathematical Society.

Mr. Joseph Thomson has returned to England from South Africa, where he has been at work on behalf of the British South Africa Company. Accompanied by Mr. Grant, a son of Colonel Grant, he crossed the plateau between Lake Nyassa and Lake Bangweolo, and we learn from the *Times* that he has been able to make important rectifications in the geography of the Bangweolo region. The lake, as shown in our maps, is incorrectly laid down, mainly because the one definite and precise observation taken by Livingstone has not been adhered to. The lake is really only a backwater of the Chambeze (the source of the Congo), which enters from the east, and issues from the west of the lake as the Luapula. The lake, in fact, lies in a very slight depression of the plateau to the north of the Chambeze-Luapula. Even in the rainy season Mr. Thomson believes the lake does not exceed 20 feet at its deepest. The southern shores are clothed with forests, and, as a matter of fact, Mr. Thomson encamped far within the bed of the lake as it is laid down in most maps. In the rainy season the water of the lake spreads out, and covers for some distance the ground on which the forest stands.

Mr. W. L. Sclater, the Deputy Superintendent of the Indian Museum, Calcutta, will proceed to Upper Assam in December next, upon a collecting expedition for the benefit of the Museum. From Makum he will ascend the Dibing river in boats to the mouth of the Dapha, one of its confluents from the north, and establish his camp at some convenient spot in the Dapha valley. At the head of the Dapha valley rises Dapha Búm, a mountain of some 15 000 feet in altitude, on the frontiers of Chinese territory, so that there is a good prospect of the occurrence of Chinese forms in the district. The Dapha valley has been described geographically by Mr. S. E. Peal, who visited it in 1882 (see J.A.S.B., lii., pt. 2, p. 7), but has not been much explored zoologically. Mr. Sclater will pay special attention to mammals and birds.

Mr. Frank H. Bigelow, who has been acting as assistant in the U.S. Nautical Office, has been appointed to a newly-created professorship in the American Weather Bureau. His work will relate to terrestrial magnetism and solar physics, especially in their relation to meteorology.

News has been received of M. Paul Maury, who started in March last year for a botanical expedition in Mexico, and of whom nothing had been heard since his departure. He appears to have made a successful exploration of the province of Huasteca.

Dr. S. Winogradsky, of Zürich, has been appointed director of the scientific bacteriological section of the new Bacteriological Institute at St. Petersburg.

A notice which will be read with interest by owners of gems has been issued by Dr. A. Brezina, the Director of the Mineral Department of the Natural History Museum at Vienna. It relates to the doings of a young man who, on September 26, contrived to conceal himself in the Department just before the time for the closing of the Museum. He was caught, and found to be armed with a revolver, and to have in his possession files and other implements. He had also in his possession nearly 600 gems, some of them cut, but the majority in their natural state. He has a passport, in which he is described as Hugo Kahn, of Berlin; but he has also called himself Krony, Kronek, Kornak, Kronicsalsky. His age is twenty-four; he measures in height 170 cm.; he is slender, has a longish, handsome face, is of a brownish complexion, has dark hair, grey eyes, and a light-brown beard, which is of feeble growth. Upon the whole, he is an attractive-looking person. He has made several journeys in Germany, France, Switzerland, and Italy; and between the middle of July last and the beginning of September he travelled through Pyrmont, Ems, Strasburg, Basel, Milan, Genoa, Nice, Monaco, Genoa, Venice, to Vienna. Most of the gems (the names of which, with the exception of a rock crystal, he does not know) he professes to have bought from a barber in Marseilles. As it is important that the former owner or owners should be known, Dr. Brezina prints a list of the gems, with the request that anyone who has information about them will communicate with him.

On Monday the centenary of the Royal Veterinary College in Great College Street, Camden Town, was celebrated by a luncheon given in a tent which had been erected in front of the new buildings. The Duke of Cambridge, President of the College, took the chair, and the Prince of Wales was among the guests. In proposing the toast, "Success and continued prosperity to the institution," the Prince of Wales contrasted the important position of the College at the present day with its humble beginnings a hundred years ago.

We regret to record the death of the Rev. Percy W. Myles, of Bright's disease, at the comparatively early age of forty-two, at Ealing, on October 7. He was a man of great ability both in literary and scientific pursuits. He was a good botanist, and proved himself a most able editor of *Nature Notes*, the journal of the Selborne Society. The work with which his name will be identified is the "Pronouncing Dictionary of Botanical Names," appended to Nicholson's "Dictionary of Gardening"; it is now recognized as a standard work by botanists. Unfortunately his professional duties did not enable him to leave a margin; so that it is proposed to raise a "Myles Memorial Fund" on behalf of his widow; and any contributions will be thankfully received and at once acknowledged by the Rev. Prof. G. Henslow, Drayton House, Ealing, London, W.

THE Council of the Institution of Civil Engineers has issued a list of subjects on which it invites communications. The list is to be taken merely as suggestive, not in any sense as exhaustive. For approved papers, the Council has the power to award premiums, arising out of special funds bequeathed for the purpose. A detailed list is given of the awards made for original communications submitted during the past session.

MORE than ten earthquake shocks were felt in the island of Pantellaria, between Sicily and the Tunisian coast, between 5.30 p.m. and 4 a.m. on October 14–15. Some of the shocks were rather violent, and nearly all the inhabitants left their houses and passed the night in the streets or in the open country. According to intelligence received at the Central Meteorological Bureau, Rome, from Pantellaria on October 18, shocks of earthquake continued to be felt in the island. A remarkable phenomenon is announced in connection with these seismic disturbances. A new volcano has risen from the bed of the sea, not far from the coast of the island, and has been throwing up masses of stones and rubbish to a considerable height. A "slight eruption" from it was referred to in a telegram sent from Rome on October 20.

LAST winter there were some reports that sunset phenomena had greatly increased in brilliancy, as if something similar to the optical disturbance following the Krakatão eruption had occurred. Herr Busch has remarked (*Met. Zeit.*) how difficult it is to recognize gradual variations in such phenomena, or to say where they pass beyond the normal. Even the brown-red Bishop's ring may be regarded as quite normal in winter. A much more sure method of finding an optical disturbance of the atmosphere is measurement of the polarization of light. Herr Busch has carried this on systematically for some years with a Savart polariscope, and a simple instrument for measuring angles, determining the height of the two neutral points (Babinet's and Arago's) at sunset. Now, the values for this height, in February and May last, considerably exceed those obtained in the three previous years, and come near those in 1886, when the last traces of the great atmospheric disturbance were still everywhere perceptible. It would seem, then, that some optical disturbance has been really present, the beginning, extent, and cause of which, however, are in obscurity. The desirability of systematic observations in different places is pointed out.

IN our issue of October 8 (p. 549) we drew attention to three atlases issued by the Chief Signal Officer of the U.S. Army. We have now to record the publication, dated June 15 last, of an atlas containing seventy-two charts showing the normal temperature conditions in the United States and Canada by decades, three decades to each month, for 8h. a.m. and 8h. p.m., Washington time. Although the Signal Service has been in existence upwards of twenty years, it had not before been able to accumulate sufficient actual observations at any one hour, or set of hours, from which normal values could be derived. The values and isotherms contained in the present atlas are based upon nine years' observations, 1881–89. The charts have been carefully prepared, for the work of the Forecast Division, and will also be very useful in furnishing general information upon the average temperature of North America. The work has been prepared under the supervision of General Greely, although issued by the new Weather Bureau.

THE Ealing *Middlesex County Times* (October 17) prints the following account of an incident which occurred at "The Grange," the residence of Mr. Yates Neill, Ealing, on Wednesday, October 14:—"It appears that during Tuesday night a large branch of one of the magnificent chestnut trees standing in the ground was broken off by the force of the wind, and fell on two stripling chestnut trees near the wall. On Wednesday morning, the gardener, a man named Parker, was engaged in sawing the detached bough, Mr. Delancey Neill and Mr. Vertie Neill watching the operation. Just before noon, the first-named gentleman saw what appeared to him to be a ball of fire fall, and striking the tree in an oblique direction, alight on the ground within two or three yards of where the three were standing, whence it rebounded and exploded with a sound like dynamite. Although neither of them was struck, the shock was so great that for a time all three were dazed, Mr. Vertie Neill, indeed, being thrown down, and rolling over two or three times. His brother was the first to recover from the shock, and promptly went to his help, and he was removed to the house, where the feeling of dizziness speedily wore off; and beyond somewhat severe headaches, which lasted for some hours, neither of the gentlemen nor the gardener appeared to have suffered any ill effects. The trunk of the tree struck by the meteor presents the appearance of having been burned in a zigzag direction for a distance of some 20 or 30 feet."

MOST people who visit Greece devote their attention mainly to the remains of ancient art. Dr. Philippson, of Berlin, is of opinion that they might also with advantage spend some time in climbing the mountains of Greece. In the *Zeitschrift des Deutschen und Oesterreichischen Alpenvereins* he deals with the subject in a capital paper, which has been issued separately. He gives an attractive account of his own experiences in climbing Mount Chelmós, in the Peloponnese, describing admirably the impression produced upon him by the Styx. Dr. Philippson shows that in the Highlands of Greece there is still much good work to be done in topography, geology, and meteorology; and he sees no reason why some of it should not be accomplished by tourists.

MESSRS. W. H. ALLEN AND CO. have published a second edition of the late Mr. R. A. Proctor's "Other Suns than Ours."

THE new number of the *Internationales Archiv für Ethnographie* opens with a most interesting paper (in German) by Dr. J. Zemmrich on "The Islands of the Dead, and related Geographical Myths." The author shows how widely diffused is the belief that there are far-off happy islands, where all sorts of enjoyments are in store for the dead; and he suggests that Atlantis, about which so much has been written, was originally one of these mythical realms. Dr. J. Jacobs concludes his critical examination (in Dutch) of Dr. Ploss's view of the significance of circumcision.

MR. G. J. SYMONS, F.R.S., contributes to the current number of the Quarterly Journal of the Royal Meteorological Society a learned paper on the history of rain-gauges. It was read before the Society on March 18, in connection with the annual exhibition, and is one of the series in which hygrometers, anemometers, instruments for travellers, thermometers, sunshine recorders, barometers, marine instruments, apparatus for studying atmospheric electricity, solar radiation instruments, and the application of photography to meteorology, have been successively dealt with. Among the remaining contents of the number are papers on the following subjects: meteorological photography, by A. W. Clayden; on the variations of the rainfall at Cherra Poonjee, in the Khasi Hills, Assam (plate), by H. F. Blanford, F.R.S.; some remarkable features in the winter of 1890–91 (four illustrations), by F. J. Brodie; the rainfall of February 1891, by H. S. Wallis; "South-east Frosts," with special reference to the frost of 1890–91, by the Rev. F. W. Stow.

In the latest record of the proceedings of the Philosophical Society, Philadelphia, Dr. Daniel G. Brinton gives some vocabularies from the Musquito Coast. He obtained them from the Rev. W. Siebärger, a missionary of the United Brethren, now resident in that region. The most important of the vocabularies is a list of words from the language of the Ramas tribe, the only specimen of their tongue Dr. Brinton has ever secured. These people live on a small island in Blomfield lagoon. There are at present about 250 of them. All of them have been converted to Christianity, and, with the exception of a few very old persons, are able to speak and read English. Their native language is rapidly disappearing, and in a few years, probably, no one will use it fluently and correctly. They are large and strongly built, and are described as submissive and teachable. Their language has always been regarded as wholly different from that of the Musquito Indians, who occupy the adjacent mainland; and this is shown to be correct by the specimen sent to Dr. Brinton. It bears no relation, he says, to any other tongue along the Musquito Coast. It does not, however, stand alone, constituting an independent stock, but is clearly a branch, not very remote, of a family of languages once spoken near Chiriqui lagoon, and thence across—or nearly across—to the Pacific.

The Penang Administration Report for 1890 contains some interesting observations on the little-known aborigines of the Malay Peninsula. Observations made during the course of the year go to show that the Sakai (as distinguished from the Semang, or Pangan, as the Negrito tribes are called by the Malays of Perak and Pahang respectively) are far more numerous than was formerly supposed, and the President is of opinion that there may be more than 5000 men, women, and children in the district of Ulu Pahang alone. The country on both sides of the mountain range, which forms the watershed of the Jelai, Selom, Bidor, and Kampar rivers, is thickly inhabited by Sakai, who, although one or two large villages exist, live for the most part in groups of from two to three families. These Sakai are divided into two distinct tribes, called by themselves Sen-oi and Tem-be respectively, the former being the more civilized and more accessible tribe, while the latter are but little known to the Malays. Both the Tem-be and Sen-oi dialects, however, resemble one another so closely that it would seem to be evident that they originally sprang from the same source. Words to express any numerals higher than three are not found in the dialect of either tribe.

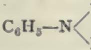
The mareograph in the harbour of Pola, according to Lieut. Gratzl (*Met. Zeitsch.*), often shows, in addition to the ordinary tidal curve, certain more or less regular oscillations, generally with a period of about 15 minutes (some with one of 7 minutes). These appear to be of the nature of *seiches*, and to be caused by squalls, which drive water from the open sea into the partly inclosed basin of the harbour, where it rises as a wave, retires, rises again to a less height (as only part of the surplus water escapes), and so on. Thus, in the evening of July 6, 1890, after a stiff west-north-west squall, there were eight pronounced oscillations, the strongest showing about 1·4 inch difference of level in 16 minutes. In another case, the harbour level rose higher than it had done for 15 years. The latter squall (a strong south-west one) affected also the Trieste mareograph, which showed nine wide oscillations with a mean period of 1 hour 46 minutes. Lieut. Gratzl suggests observations as to whether sudden impulses of "bora" against the Italian coast might not heap up the water there, so that a return wave might affect the Austrian mareographs; also whether certain sudden currents which injure fishermen's nets in the Dalmatian canals may not be connected with those waves.

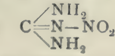
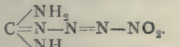
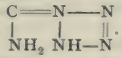
A cat born with only two legs (the fore-legs being absent from the shoulder-blades) has been recently described by Prof. Leon of Jassy (*Naturw. Rundsch.*). It is healthy, and goes about easily, the body in normal position. When startled, or watching anything, it raises itself to the attitude of a kangaroo, using the tail as a support. This animal has twice borne kittens; in both cases two, one of which had four feet, the other only two.

We learn from Dr. Woeikof's notes of a journey in the Caucasus published in the Russian *Izvestia*, that the Russian Ministry of Ways and Communications has issued a very interesting work on the snow-slips of the Kazbek glaciers, accompanied by an atlas of maps and plans. Careful measurements of the variations in the position of the lowest end of the Devdorak glacier since 1878 have been made, and the results are given in the atlas. A house has been recently built close to the glacier, and it is connected by a road (available for horses) with the villages beneath. An experienced guide, who is bound to accompany the men of science and tourists who may intend to visit the glacier, stays in the house.

A kind of artificial honey which has lately been produced seems likely to become a formidable rival of natural honey. It is called "sugar honey," and consists of water, sugar, a small proportion of mineral salts, and a free acid; and the taste and smell resemble those of the genuine article. Herr T. Weigle brought the subject before a recent meeting of the Bavarian Association of the Representatives of Applied Chemistry, and there is a paragraph about it in the current number of the *Board of Trade Journal.*

Rats at Aden appear to have a vigorous appetite, and to adopt remarkable ways of gratifying it. Captain R. Light, writing on the subject from Aden to the Journal of the Bombay Natural History Society, says the rats in his house—which is overrun with them—demolish skins, braces, whips, &c.; and one night he awoke, feeling a rat gnawing at his toes. This happened in spite of a dog (a good ratter) being in the room. Captain Light was lately watching his pony being shod, and noticed the hoof apparently cut away all round the coronet, wherever it was soft. He accused the "nalband" of doing this in addition to the usual rasping of the hoof to suit the shoe. The "syce" said that the rats had done it, and that they came at night and ate away not only the pony's hoofs but those of the goat and kid, and that these animals were greatly tormented by the rats. Captain Light examined the hoofs, and found beyond doubt that such was the case, the marks of the teeth being plain; moreover, he found that the horns of the kid, which had been about half an inch high, were eaten flush with the head. Next morning, too, a large rat was discovered in the bedding under the horse. It had evidently been killed by a kick from him.

Two new methods of preparing azoimide, N_3H, the hydride of nitrogen isolated last year by Prof. Curtius, of Kiel, have been discovered. As announced at the time in NATURE (vol. xliii. p. 21), Prof. Curtius prepared this remarkable compound by reacting with his previously isolated hydrazine hydrate, $N_2H_4.H_2O$, upon hippuric acid, converting the hydrazine derivative thus obtained into its nitroso-derivative, and decomposing an alkaline solution of the latter with sulphuric acid. An aqueous solution of azoimide was obtained upon distilling the product of the latter operation. In order to obtain the free compound itself, the silver salt was prepared by allowing the distillate to flow into a solution of silver nitrate, and the precipitated silver salt, after drying, was decomposed with sulphuric acid. In a subsequent communication (comp. NATURE, vol. xliii. p. 378), Prof. Curtius, in conjunction with Dr. Radenhausen, showed that the pure compound was a very volatile liquid, boiling at 37°, and of fearfully explosive properties. In the current number of the *Berichte*, Drs. Noelting and Grandmougin, of Mülhausen, publish a preliminary note, in which they describe a new, and from the point of view of its constitution most important, method of preparing the liquid. The phenyl ester of azoimide is the diazobenzene imide of Griess,  $C_6H_5{-}N\left<\begin{array}{c}N\\ \|\\ N\end{array}\right.$, just as chlorbenzene is the phenyl ester of hydrochloric acid. In view of the great stability of the esters of aromatic radicles, it was hardly to be expected that diazobenzene imide would yield azoimide upon saponification. But Drs. Noelting and Grandmougin considered that it might be possible to obtain the latter by decomposing a nitro-derivative of diazobenzene imide by means of alkalies, inasmuch as the introduction of nitro-groups generally effects a considerable increase in the mobility of the acid radicle, rendering its removal by processes of saponification much less difficult. They therefore prepared the dinitro-derivative of diazobenzene imide from dinitro-aniline by means of the usual diazo-reaction—conversion into the perbromide, and treating with ammonia. When treated with alcoholic potash, this dinitro-diazobenzene imide readily decomposes into the potassium salt of dinitro-phenol and azoimide. Upon acidifying the product of the reaction and subjecting it to distillation, an aqueous solution of azoimide passes over, which may be converted into the anhydrous liquid by the method described by Prof. Curtius. The properties of the anhydrous azoimide obtained by this new method agree completely with those detailed by Prof. Curtius.

THE second new method of preparing azoimide was communicated by Dr. Thiele, of Halle, at the Versammlung deutscher Naturforscher und Aerzte, held in that city in September last. In the course of an investigation of the compounds of guanidine, nitro guanidine was obtained,  $C\left<\begin{array}{l}NH_2\\ =N{-}NO_2\\ NH_2\end{array}\right.$. Upon treating this compound with acetic acid and zinc dust, it is reduced to amido-guanidine, a substance which forms well-crystallized salts. By boiling the latter with soda, decomposition ensues, with formation of free hydrazine, N_2H_4, which may be very conveniently prepared by this method. Upon subjecting the nitrate to the diazo-reaction, the diazo-nitrate of guanidine is obtained,  $C\left<\begin{array}{l}NH_2\\ =N{-}N{=}N{-}NO_2\\ NH\end{array}\right.$. This compound readily breaks up on warming into two compounds, one of which is azoimide, and the other a complex acid of the composition CN_5H_3, and the curious constitution  $\begin{array}{lll}C & {=}N & {-}N\\ | & \;| & \;\|\\ NH_2 & NH & {-}N\end{array}$. The azoimide may be obtained by distillation in a manner similar to that described above.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♂) from India, presented by Mr. W. Harrow; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mrs. Cotton; a Common Marmoset (*Hapale jacchus*) from South-East Brazil, presented by Mrs. Trelawny; a Gannet (*Sula bassana*), British, presented by Mr. J. Hitchman; a Smooth Snake (*Coronella lævis*) from Hampshire, presented by Mr. F. C. Adams; ten Smooth Snakes (*Coronella lævis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

DISTRIBUTION OF LUNAR HEAT.—Mr. Frank H. Very's essay on the distribution of the moon's heat and its variation with the phase, which gained the prize of the Utrecht Society of Arts and Sciences in 1890, has recently been published. A bolometer in connection with a very sensitive galvanometer was used in the research, and the plan has been to project an image of the moon about 3 centimetres in diameter by a concave mirror; and to measure, not the heat from the whole of this, but only that in a limited part of it, from $\frac{1}{25}$ to $\frac{1}{30}$ of the area of the disk, the observations being repeated at different points and at different phases. Measures made six hours after full moon show that the east limb was hotter than the west limb in the proportion of 92·2 to 88·9. In one observation, made a day after full moon, the excess of heat at the east limb was much larger. There is a regular decrement of heat in passing from higher to lower latitudes, and observations on this point appear to indicate that heat is accumulated after many days of continuous sunshine. The heat in the circumferential zone of the full moon differs from that of the centre by about 20 per cent. In this respect, therefore, the thermal image is like the visual one. There seems to be some evidence that bright regions radiate a little more than dark during the middle of the lunar day; but this is not quite proved, and with a low altitude of the sun the effect is reversed. A comparison of the curve drawn by Zollner for the moon's light with that deduced from Mr. Very's observations brings out the point that visible rays form a much larger proportion of the total radiation at the full than at the partial phases, the maximum for light being much more pronounced than that for the heat. The diminution of the heat from the full to the third quarter is shown to be slower than its increase from the first quarter to the full. This result agrees with that obtained at Lord Rosse's Observatory, and is direct evidence of the storage of heat by lunar rocks.

GEOLOGICAL SOCIETY OF AMERICA.

THE Geological Society of America met at Washington on August 24 and 25. Owing to the death of the President, Prof. Alexander Winchell, Vice-President Gilbert took the chair.

The meeting was opened with an address on the late President by his brother, Prof. N. H. Winchell. Alexander Winchell was born on December 31, 1824, in Dutchess County, N.Y., and died at Ann Arbor on February 19 last. His work was many-sided. He had studied to be a civil engineer; had a strong leaning towards theology. He also read medicine and was a fine mathematician. He loved music, wrote poetry, and modelled in clay and plaster. As a financial resource he became a teacher, and was very successful. He became famous by his arguments on "The Bible History of the Creation," and published in the *Christian Advocate* "Adamites and Pre-Adamites," an exposition of Scriptural and scientific harmony. For four years he lectured on geology at Vanderbilt University. During his long connection with the University of Michigan he wrote many scientific articles of a popular nature, and did a great deal to popularize geological science. The speaker spoke eloquently of his dead brother's long and splendid connection with the Ann Arbor University. His death was most touchingly described. Oddly enough the last words he uttered in public were these: "When I speak to you again it will be of the inhabitants of another world." He had just finished his weekly lecture, and referred in his closing sentence to the subsequent lecture that was never delivered. He discovered many new geological species, and many other geologists testified their admiration for him by naming after him species they discovered. His great work for the Geological Society was touched on, and the speaker expressed his conviction that the next generation would keenly feel the beneficent influence of his brother's work. At the conclusion of the memorial Prof. Edward Orton, Dr. C. A. White, and Mr. C. R. Van Hise were appointed a committee to draft resolutions expressive of the Society's regret at the death of its President.

Prof. Dr. Gustav Steinmann, of the University of Freiburg, Germany, read the first paper, which consisted of the description of a geological map of South America. A large copy of the map was hung up beside the platform, and small replicas were distributed among the audience. Dr. Steinmann, who is a young, bearded, spectacled, typical German student, was sent to

South America by the Strasburg University some ten years ago, and spent some two years making a most thorough research in the geology of the continent, the tangible result being the remarkably complete map exhibited. His researches in South America prove that there is a most remarkable similarity between the geology of the two Americas, and especially between the geology of the southern United States and the southern continent.

The second paper was by Dr. August Rothpletz, of the University of Munich, Germany, on the Permian, Triassic, and Jurassic formations in the East Indian Archipelago. The doctor's paper was devoted to the description of some Mesozoic and Palæozoic fossils collected in two of the Indian islands by his friend Dr. Wichmann, during a geological exploration of the islands. Dr. Wichmann being geologist of the University of Utrecht, Holland, the collections were of particular value, and Dr. Rothpletz's description and classification of them, to which he devoted his paper, was thorough and minute. He took occasion to ridicule some of the classifications of fossils which put them in one category when found in one place and in another when found somewhere else.

"Thermometamorphism in Igneous Rocks" was the title of the next paper presented. It was by Mr. Alfred Harker, of St. John's College, Cambridge, England, and dealt with the effects of high volcanic temperatures in the formation of rocks. He described the results of his researches in the lake region of England, where the volcanic forces of nature were particularly well marked.

Prof. Alexis Pavlow of the University of Moscow, Russia, presented a paper entitled "Sur les Couches Marines terminant le Jurassique et commençant le Crétace, et sur l'Histoire de leur Faune."

Another paper, also in French, presented by Prof. Max Lohest, of the University of Liége, Belgium, was entitled "Sur l'Homme contemporain du Mammouth en Belgique." The contemporaneous existence of man was supported by proofs additional to those heretofore given.

Baron Gerald de Geer, of Sweden, gave an interesting account of recent changes of level along the sea-board of the Scandinavian peninsula.

The most important new matter presented was a paper on "Fossil Fishes of the Lower Silurian Rocks of Colorado," by Mr. C. D. Walcott, of the United States Geological Survey. The discovery of the fossil fish remains is of recent date, and attracts great attention among zoologists and geologists from its carrying back into the past, over a great time interval, our knowledge of vertebrate life. They are the oldest vertebrate remains known, and appear to be the ancestral types of the great ichthyic fauna of the classic "old red sandstone" of Europe, and the Devonian group of America.

In the discussion, Profs. Von Zittel, Jaekel, and F. Schmidt compared the fish remains exhibited with those of the Devonian, and stated that the Upper Silurian types were not represented in the fauna.

SECOND DAY.—From the committee appointed to draft appropriate resolutions relative to the death of Dr. Alexander Winchell, the President of the Society, Prof. Orton made a report which was adopted. The resolutions reported paid a just and touching tribute to the character of the deceased, and fittingly acknowledged the great services which he had rendered to the science in the course of the forty years of arduous and unremitting toil which he had devoted to its investigation. To his writings and lectures were attributed in a great degree the growing liberality and enlargement of thought of the more serious-minded portion of the community in regard to the theory of organic evolution as presented by Darwin and his successors. Dr. Winchell, the report affirmed, stated and defended with marked ability and courage and persuasive power this the most characteristic and far-reaching doctrine of modern geological science. "The first enunciation of this doctrine," the report stated, "was sure to awaken distrust and even bitter hostility among a large class of people because of its apparent incompatibility with some of their most fundamental convictions and beliefs. To disregard the sincere apprehension of this great class, comprising, as it does, so much of the moral and intellectual force of the body politic, would be heartless. To mock at its fears, ill founded though they were, would be worse. What worthier service to science and the community than to disarm hostility by showing that the evolutionary philosophy, so far from degrading and dishonouring man, makes him in a peculiar sense the head and crown of the creation?"

In seconding the resolutions Dr. C. A. White paid a warm tribute to Dr. Winchell, with whom he had been on terms of intimacy for many years. As a further mark of respect the resolutions were adopted by a rising vote.

The first paper presented was by Dr. Frederick Schmidt, of St. Petersburg, Russia.

Prof. Gregoire Stefanescu, of the University of Bucharest, Roumania, presented "Sur l'Existence du Dinotherium en Roumanie," the next paper. The Professor read it in French, illustrating it by drawings on the blackboard, and after he had finished, Prof. Dr. Charles Barrois read it over again in English, so that those who did not know French might not lose it. Though quite short the paper was very interesting. It briefly described a large number of bones of the Dinotherium found widely distributed over Roumania, which indisputably pointed to the existence of this almost unknown extinct animal in that land countless years ago. This was probably the largest mammal that ever inhabited the earth, its epoch being the Tertiary period. It had enormous tusks, that curved downward and backward in such a way that it could only hurt itself with them, and probably had a massive trunk. In character it more nearly resembled the elephant and rhinoceros of modern ages than any other known animal.

Prof. A. N. Krassnof, of the Charkow University, Russia, read the next paper on the black earth of the steppes of Southern Russia, its origin, distribution, and points of resemblance with the soils of the prairies of America. The paper traced the resemblance between the Russian steppes and the American prairies to their similar origin in the layers of the vegetables of years. Their remarkable fertility was touched on generally, and a technical account of the origin of the two plains was given.

TECHNICAL CHEMISTRY.

IN his Cantor Lectures on Photographic Chemistry, delivered last spring before the Society of Arts, and just issued by the Society in a separate form, Prof. Meldola opens with some remarks on the special position of technical training in chemistry, which should be carefully conisdered in connection with the present widespread movement in the direction of technical education throughout the country. He says:—

"There are many who identify technical instruction with the teaching of some handicraft, a notion which has no doubt arisen from the identification of technical skill with manual dexterity in some mechanical industry. By the adoption, either tacitly or openly, of this narrow definition, the chemical industries have suffered to a very large extent in this country, because their progress is more dependent on a knowledge of scientific principles, and much less dependent on manual dexterity than any of the other subjects dealt with in schemes of technical instruction. Now, in order to give technical instruction in a subject like photography, which is so intimately connected with chemistry, we may adopt one of two courses. The student may become a practical photographer in the first place, and may then be led on to the science of his practice by an appeal to the purely chemical principles brought into operation. This may be called the analytical method. The other method is to give the student a training in general chemistry first, and then to specialize his knowledge in the direction of photography. This may be regarded as a synthetical method.

"In other departments of technology, and especially in those where the underlying principles are of a mechanical nature, the analytical method may be, and has been, adopted with success. It is possible to lead an intelligent mechanic from his every-day occupations to a knowledge of the higher principles of mechanical science by making use of his experience of phenomena which are constantly coming under his notice. From this it is sometimes argued by those who are in the habit of regarding technical instruction from its purely analytical side, that technical chemistry can be taught by the same method. Some teachers may possibly succeed in this process, but my own experience, both as a technologist and a teacher, has led me to the conclusion that, for chemical subjects, the analytical method is both too cumbersome and circuitous to be of any real practical use. No person engaged in chemical industry in any capacity—whether workman, foreman, manager, or proprietor—can be taught the principles of chemical science out of his own industry, unless he has some considerable knowledge of general principles to start with. No person who is not grounded in such broad principles can properly appreciate the explanation of the phenomena with which his daily experience brings him into contact, and if his previous training is insufficient to enable him to understand the nature of the changes which occur in the course of his operations, he cannot derive any advantage from technical instruction. These remarks will, I hope, serve to emphasize a distinction which exists between technical chemistry and other technical subjects, and I have thought it desirable to avail myself of the present opportunity of calling particular attention to this point, because it is one which is generally ignored in all discussions on technical education.

"The reason for this difference in the mode of treatment of chemical subjects is not difficult to find. The chemical technologist—the man who is engaged in the manufacture of useful products out of certain raw materials—is, so far as the purely scientific principles are concerned, already at a very advanced stage, although he may not realize this to be the case. The chemistry of manufacturing operations, even when these are of an apparently simple kind, is of a very high order of complexity. There are many branches of chemical industry in which the nature of the chemical changes undergone by the materials is very imperfectly understood; there is no branch of chemical industry of which the pure science can be said to be thoroughly known. For these reasons I believe that I am justified in stating that the chemical technologist is working at a high level, so far as the science of his subject is concerned, and this explains why he cannot be dealt with by the analytical method.

"The general considerations which have been offered apply to the special subject of photography with full force. A person may become an adept as an operator without knowing anything of physics or chemistry; there are thousands of photographers all over the country who can manipulate a camera and develop and print pictures with admirable dexterity, who are in this position. If we adopt the narrow definition of technical instruction, we should appoint such experts in our Colleges, and through them impart the art of taking pictures to thousands of others. But would our position as a photographing nation be improved by this process? I venture to think not. We might be carrying out the ideas of certain technical educators by adopting this method, but I do not imagine that in the long run the subject itself would be much advanced; our position in the scale of industry would not be materially raised by the wholesale manufacture of skilful operators. And so with all other branches of applied chemistry; it is technologists whose knowledge is based on a broad foundation that are wanted for the improvement of our industries. These are the men who are raised in the technical high schools of the Continent, and whose training the Continental industries have had the wisdom to avail themselves of."

AN ASTRONOMER'S WORK IN A MODERN OBSERVATORY.[1]

THE work of astronomical observatories has been divided into two classes, viz. astrometry and astrophysics. The first of these relates to astronomy of precision, that is to the determination of the positions of celestial objects; the second relates to the study of their physical features and chemical constitution.

Some years ago the aims and objects of these two classes of observatories might have been considered perfectly distinct, and, in fact, were so considered. But I hope to show that in more recent years their objects and their processes have become so interlaced that they cannot with advantage be divided, and a fully equipped modern observatory must be understood to include the work both of *astrometry* and *astrophysics*.

In any such observatory the principal and the fundamental instrument is the transit circle. It is upon the position in the heavens of celestial objects, as determined with this instrument or with kindred instruments, that the whole fair superstructure of exact astronomy rests; that is to say, all that we find of information and prediction in our nautical almanacs, all that we know of the past and can predict of the future motions of the celestial bodies.

Here is a very small and imperfect model, but it will serve to render intelligible the photograph of the actual instrument which will be subsequently projected on the screen. [Here the lecturer described the adjustments and mode of using a transit circle.]

We are now in a position to understand photographs of the instrument itself. But first of all as to the house in which it dwells. Here, now on the screen, is the outside of the main building of the Royal Observatory, Cape of Good Hope. I select it simply because, being the observatory which it is my privilege to direct, it is the one of which I can most easily procure a series of photographs. It was built during the years 1824–28, and like all the observatories built about that time, and like too many built since, it is a very fair type of most of the things which an observatory should not be. It is, as you see, an admirably solid and substantial structure, innocent of any architectural charm, and so far as it affords an excellent dwelling-place, good library accommodation, and good rooms for computers, no fault can be found with it. But these very qualities render it undesirable as an observatory. An essential matter for a perfect observatory should be the posssibility to equalize the internal and the external temperature. The site of an instrument should also be free from the immediate surroundings of chimneys or other origin of ascending currents of heated air. Both these conditions are incompatible with thick walls of masonry and the chimneys of attached dwelling houses, and therefore, as far as possible, I have removed the instruments to small detached houses of their own. But the transit circle still remains in the main building, for, as will be evident to you, it is no easy matter to transport such an instrument.

The two first photographs show the instrument, in one case pointed nearly horizontally to the north, the other pointed nearly vertical. Neither can show all parts of the instrument, but you can see the massive stone piers, weighing many tons each, which, resting on the solid blocks 10 feet below, support the pivots. Here are the counter-weights which remove a great part of the weight of the instrument from the pivots, leaving only a residual pressure sufficient to enable the pivots to preserve the motion of the instrument in its proper plane. Here are the microscopes by which the circle is read. Here the opening through which the instrument views the meridian sky. The observer's chair is shown in this diagram. His work appears to be very simple, and so it is, but it requires special natural gifts, patience and devotion, and a high sense of the importance of his work to make a first-rate meridian observer. Nothing apparently more monotonous can be well imagined if a man is "not to the manner born."

Having directed his instrument by means of the setting circle to the required altitude, he clamps it there and waits for the star which he is about to observe to enter the field. This is what he sees. [Artificial transit of a star by lantern.]

As the star enters the field it passes wire after wire, and as it passes each wire he presses the key of his chronograph and records the instant automatically. As the star passes the middle wire he bisects it with the horizontal web, and again similarly records on his chronograph the transit of the star over the remaining webs. Then he reads off the microscopes by which the circle is read, and also the barometer and thermometer, in order afterwards to be able to calculate accurately the effect of atmospheric refraction on the observed altitude of the star; and then his observation is finished. Thus the work of the meridian observer goes on, star after star, hour after hour, and night after night; and, as you see, it differs very widely from the popular notion of an astronomer's occupation. It presents no dreamy contemplation, no watching for new stars, no unexpected or startling phenomena. On the contrary, there is beside him the carefully prepared observing-list for the night, the previously calculated circle setting for each star, allowing just sufficient time for the new setting for the real star after the readings of the circle for the previous observation.

After four or five hours of this work, the observers have had enough of it; they have, perhaps, observed fifty or sixty stars, they determine certain instrumental errors, and betake themselves to bed, tired, but (if they are of the right stuff) happy and contented men. At the Cape we employ two observers—one to read the circle, and one to record the transit. Four observers are employed, and they are thus on duty each alternate night. Such is the work that an outsider would see were he to enter a working meridian observatory at night, but he would find out, if he came next morning, that the work was by no means over. By far the largest part has yet to follow. An observation that requires only two or three minutes to make at night, requires at least half an hour for its reduction by day. Each observation is affected by a number of errors, and these have to be determined and allowed for. Although solidly founded on massive piers resting on the solid rock, the constancy of the instrument's position cannot be relied upon. It goes through small periodic changes in level, in collimation, and in azimuth, which have to be determined by proper means, and the corresponding corrections have to be computed and applied; and, also, there are other corrections for refraction, &c., which involve computation and have to be applied. But these matters would fall more properly under the head of a special lecture upon the transit instrument. I mention them now, merely to explain why so great a part of an astronomer's work comes in the daytime, and to dispel the notion that his work belongs only to the night.

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[1] Friday Evening Discourse delivered at the Royal Institution by Dr. David Gill, F.R.S., Her Majesty's Astronomer at the Cape of Good Hope, on May 29, 1891.

One might very well occupy a special lecture in an account of the peculiarities of what is called personal equation—that is to say, the different time which elapses for different observers between the time when the observer believes the star to be upon the wire, and the time when the finger responds to the message which the eye has conveyed to the brain. Some observers always press the key too soon; some always too late. Some years ago I discovered, from observations to which I will subsequently refer, that *all* observers press the chronograph key either too soon for bright stars or too late for faint ones.

Other errors may, and I am sure do, arise both at Greenwich and the Cape, from the impossibility of securing uniformity of outside and inside temperature in a building of strong masonry. The ideal observatory should be solid as possible as to its foundations, but light as possible as to its roof and walls—say, a light framework of iron covered with canvas. But it would be undesirable to cover a valuable and permanent instrument in this way.

But here is a form of observatory which realizes all that is required, and which is eminently suited for permanent use. The walls are of sheet iron, which readily acquire the temperature of the outer air. The iron walls are protected from direct sunshine by wooden louvres, and small doors in the iron walls admit a free circulation of air. The revolving roof is a light framework of iron covered with well-painted *papier-maché*.

The photograph now on the screen shows the interior of the observatory, and this brings me to the description of observations of an entirely different class. In this observatory the roof turns round on wheels, so that any part of the sky can be viewed from the telescope. This is so, because the instrument in this observatory is intended for purposes which are entirely different from those of a transit circle. The transit circle, as we have seen, is used to determine the *absolute* positions of the heavenly bodies; the heliometer, to determine with greater precision than is possible by the absolute method the *relative* positions of celestial objects.

To explain my meaning as to absolute and relative positions. It would, for example, be a matter of very little importance if the absolute latitude of a point on the Royal Exchange or the Bank of England were one-tenth of a second of arc (or 10 feet) wrong in the maps of the Ordnance Survey of England—that would constitute a small *absolute* error common to all the buildings on the same map of a part of the city, and common to all the adjoining maps also. Such an error, regarded as an *absolute* error, would evidently be of no importance if every point on the map had the same absolute error. There is no one who can say at the present moment whether the absolute latitude of the Royal Exchange—nay, even of the Royal Observatory, Greenwich—is known to 10 feet. But it would be a very serious thing indeed if the relative positions on the same map were 10 feet wrong *here* and *there*. For example, if of two points marking a frontage boundary on Cornhill, one were correct, the other 10 feet in error, what a nice fuss there would be! what food for lawyers! what a bad time for the Ordnance Survey Office! Well, it is just the same in astronomy.

We do not know, we probably never shall know with certainty, the absolute places of even the principal stars to one-tenth of a second of arc. But one-tenth of a second of arc in the measure of some relative position would be fatal. For example, in the measurement of the sun's parallax an error of one-tenth of a second of arc means an error of 1,000,000 miles, in round numbers, in the sun's distance; and it is only when we can be quite certain of our measures of much smaller quantities than one-tenth of a second of arc, that we are in a position to begin seriously the determination of such a problem as that of the distances of the fixed stars. For these problems we must use differential measures—that is, measures of the relative positions of two objects. The most perfect instrument for such purposes is the heliometer.

Lord McLaren has kindly sent from Edinburgh, for the purposes of this lecture, the parts of his heliometer which are necessary to illustrate the principles of the instrument.

This instrument is the same which I used on Lord Crawford's expedition to Mauritius in 1874. It was also kindly lent to me by Lord Crawford for an expedition to the Island of Ascension to observe the opposition of Mars in 1877. In 1879, when I went to the Cape, I acquired the instrument from Lord Crawford, and carried out certain researches with it on the distances of the fixed stars.

In 1887, when the Admiralty provided the new heliometer for the Cape Observatory, this instrument again changed hands. It became the property of Lord McLaren. I felt rather disloyal in parting with so old a friend. We had spent so many happy hours together, we had shared a good many anxieties together, *and we knew each other's weaknesses so well*. But my old friend has fallen into good hands, and has found another sphere of work.

The principle of the instrument is as follows. [The instrument was here explained.]

There is now on the screen a picture of the new heliometer of the Cape Observatory, which was mounted in 1887, and has been in constant use ever since. It is an instrument of the most refined modern construction, and is probably the finest apparatus for refined measurement of celestial angles in the world.

[Here were explained the various parts of the instrument in relation to the model, and the actual processes of observation were illustrated by the images of artificial stars projected on a screen.]

Here, again, there is little that conforms to the popular idea of an astronomer's work; there is no searching for objects, no contemplative watching, nothing sensational of any kind. On the contrary, every detail of his work has been previously arranged and calculated beforehand, and the prospect that lies before him in his night's work is simply more or less of a struggle with the difficulties which are created by the agitation of the star images, caused by irregularities in the atmospheric refraction. It is not upon one night in a hundred that the images of stars are perfectly tranquil. You have the same effect in an exaggerated way when looking across a bog on a hot day. Thus, generally, as the images are approached, they appear to cross and recross each other, and the observer must either seize a moment of comparative tranquillity to make his definitive bisection, or he may arrive at it by gradual approximations till he finds that the vibrating images of the two stars seem to pass each other as often to one side as to the other. So soon as such a bisection has been made, the time is recorded on the chronograph, then the scales are pointed on and printed off, and so the work goes on, varied only by reversals of the segments and of the position circle. Generally, I now arrange for thirty-two such bisections, and these occupy about an hour and a half. By that time one has had about enough of it, the nerves are somewhat tired, so are the muscles of the back of the neck; and if the observer is wise, and wishes to do his best work, he goes to bed early and gets up again at two or three o'clock in the morning, and goes through a similar piece of work. In fact, this must be his regular routine night after night, whenever the weather is clear, if he is engaged, as I have been, on a large programme of work on the parallaxes of the fixed stars, or on observations to determine the distance of the sun by observations of minor planets.

I will not speak now of these researches, because they are still in process of execution or of reduction. I would rather, in the first place, endeavour to complete the picture of a night's work in a modern observatory.

We pass on to celestial photography, where astrometry and astrophysics join hands. Here on the screen is the interior of one of the new photographic observatories, that at Paris. [Brief description.]

Here is the exterior of our new photographic observatory at the Cape. Here is the interior of it, and the instrument. [Brief description.]

The observer's work during the exposure is simply to direct the telescope to the required part of the sky, and then the clockwork *nearly* does the rest—but not quite so. The observer holds in his hand a little electrical switch with two keys; by pressing one key he can accelerate the velocity of the driving-screw by about 1 per cent., and by pressing the other he can retard it 1 per cent. In this way he keeps one of the stars in the field always perfectly bisected by the cross-wires of his guiding telescope, and thus corrects the small errors produced partly by changes of refraction, partly by minute unavoidable errors in cutting the teeth of the arc into which the screw of the driving-shaft of the clockwork gears.

The work is monotonous rather than fatiguing, and the companionship of a pipe or cigar is very helpful during long exposures. A man can go on for a watch of four or five hours very well, taking plate after plate, exposing each, it may be, forty minutes or an hour. If the night is fine, a second observer follows the first, and so the work goes on the greater part of the night. Next day he develops his plate, and gets something like this. [Star-cluster.]

Working just in this way, but with the more humble apparatus which you see imperfectly in the picture now on the screen, we have photographed at the Cape during the past six years the whole of the southern hemisphere from 20° of south declination to the South Pole.

The plates are being measured by Prof. Kapteyn, of Groningen, and I expect that in the course of a year the whole work, containing all the stars to 9½ magnitude (between 200,000 and 300,000 stars) in that region, will be ready for publication. This work is essential as a preliminary step for the execution in the southern hemisphere of the great work inaugurated by the Astrophotographic Congress at Paris in 1887, the last details of which were settled at our meeting at Paris in April last. What we shall do with the new apparatus, perhaps I may have the honour to describe to you some years hence, after the work has been done.

We now come to an important class of astronomical work, more purely astrophysical, for the illustration of which I can no longer appeal to the Cape, because I regret to say that we are not yet provided with the means for its prosecution. I refer to the use of the spectroscope in astronomy, and especially to the latest developments of its use for the accurate measurement of the velocity of the motions of stars in the line of sight.[1]

It is beyond the province of this lecture to enter into history, but it is impossible not to refer to the fact that the chief impulse to astronomical work in this direction was given by Dr. Huggins, our Chairman to-night—nay, more, except for the early contributions of Fraunhofer to the subject, Dr. Huggins certainly is the father of sidereal spectroscopy, and that not in one but in every branch of it. He has devised the means, pointed the way, and, whilst in many branches of the work he still continues to lead the way, he has of necessity left the development of other branches to other hands.

From an astronomer's point of view the most important advance that has been made in spectroscopy of recent years is the sudden development of precision in the measures of star motion in the line of sight. The method remained for fifteen or sixteen years quite undeveloped from the condition in which it left the hands of Dr. Huggins, and certainly no progress in the accuracy attained by Dr. Huggins was made till the matter was taken up Dr. Vogel at Potsdam. At a single step Dr. Vogel has raised the precision of the work from that of observations in the days of Ptolemy to that of the days of Bradley—from the days of the old sights and pinnules to the days of telescopes. Therefore I take a Potsdam observation as the best type of a modern spectroscopic observation for description, especially as I have recently visited Dr. Vogel at Potsdam, and he has kindly given me a photograph of his spectroscope, as well as of some of the work done with it.

A photograph of the Potsdam spectroscope attached to the equatorial is now on the screen. [Description.]

The method of observation consists simply in inserting a small photographic plate in the dark slide, directing the telescope to the star, and keeping the image of the star continuously on the slit during an exposure of about an hour; and this is what is obtained on development of the picture.

If the star remained perfectly at rest between the jaws of the slit the spectrum would be represented by a single thread of light, and of course no lines would be visible upon such a thread; but the observer intentionally causes the star image to travel a little along the slit during the time of exposure, and so a spectrum of sensible width is obtained.

You will remark how beautifully sharp are the faint lines in this spectrum. Those who have tried to observe the spectrum of Sirius in the ordinary way, know that many of these fine lines cannot be seen or measured with certainty. The reason is that on account of irregularities in atmospheric refraction, the image of a star in the telescope is rarely tranquil, sometimes it shines brightly in the centre of the slit, sometimes barely in the slit at all, and the eye becomes puzzled and confused. But the photographic eye is not in the least disturbed; when the star image is in the slit, the plate goes on recording what it sees, and when the star is not in the slit the plate does nothing, and it is of no consequence whatever how rapidly these alternate appearances and disappearances recur. The only difference is that when the air is very steady and the star's image, therefore, always in the slit, the exposure takes less time than when the star is unsteady.

That is one reason why the Potsdam results are so accurate. And there are many other reasons besides, into which I cannot now enter. What, however, it is very important to note is this, that we have here a method which is to a great extent independent of the atmospheric disturbances which in all other departments of astronomical observation have imposed a limit to their precision. Accurate astrospectroscopy, therefore, may be pushed to a degree of perfection which is limited only by the optical aid at our disposal and by the sensibility of our photographic plates.

And now I think we have sufficiently considered the ordinary processes of astronomical observation to illustrate the character of the work of an astronomer at night. The picture should be completed by an account of his work by day; but to go into that matter in detail would certainly not be within the limits of this lecture. It is better that I should in conclusion touch upon some recent remarkable results of these day and night labours. It is these after all that most appeal to you; it is for these that the astronomer labours; it is the prospect of them that lightens the long watches of the night and gives life to the otherwise dead bones of mechanical routine.

Let us take first some spectroscopic results. To explain their meaning let me remind you for a moment of the familiar analogy between light and sound.

The pitch of a musical note depends on the rapidity of the vibrations communicated to the air by the reed or string of the musical instrument that produces the note, a low note being given by slow vibrations and a high one by quick vibrations.

Just in the same way red light depends on relatively slow vibrations of ether, and blue or violet light on relatively quick vibrations. Well, if there is a railway train rapidly approaching one, and the engine sounds its whistle, more waves of sound from that whistle will reach the ear in a second of time than would reach the ear were the train at rest. On the other hand, if the train is travelling at the same rate *away* from the observer, fewer waves of sound will reach his ears in a second of time. Therefore an observer beside the line should observe a distinct change of pitch in the note of the engine whistle as the train passes him, and as a matter of fact such a change of pitch can be and has been observed.

Just in the same way, if a source of light could be moved rapidly enough towards an observer it would become bluer, or if away from him it would become more red in colour. Only it would require a change of velocity in the moving light of some thousands of miles per second in order to render the difference of colour sensible to the eye. The experiment is, therefore, not likely to be frequently shown at this lecture table!

But the spectroscope enables such changes of colour to be measured with extreme precision. Here on the screen is the most splendid illustration of this that exists at present, viz. copies of three negatives of the spectrum of α Aurigæ, taken at Potsdam in October and December of 1888, and in March 1889.

The black line (the picture being a negative) represents the bright line $H\gamma$ given by the artificial light of hydrogen, the strong white line in the picture corresponds to the black absorption line which is due to hydrogen in the atmosphere of the star.

Why is it that the artificial hydrogen line does not correspond with the stellar line in all these pictures? The answer is, either the star is moving towards or from the earth in the line of sight, or the earth is moving from or towards the star. But in December the earth in its motion round the sun is moving at right angles to the direction of α Aurigæ; why then does not the stellar hydrogen line agree in position with the terrestrial hydrogen line? The simple explanation is that α Aurigæ is moving with respect to the sun.

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[1] The older methods enabled us to measure motions at right angles to the line of sight, but till the spectroscope came we could not measure motions in the line of sight.

In what way is it moving? Well, that also is clear; the stellar line is displaced towards the red end of the spectrum—that is to say, the star light is redder than it should be in consequence of a motion of recession; this proves that the star is moving away from us, and measures of the photograph show the rate of this motion to be 15½ miles per second. We also know that in October the earth, in its motion round the sun, is moving towards α Aurigæ nearly at the same rate as we have just seen that α Aurigæ is running away from the sun. Consequently, at that time, their relative motions are nearly insensible, because both are going at the same rate in the same direction, and we find accordingly in October that the positions of the stellar and artificial hydrogen lines perfectly correspond. Finally, in March, the earth, in its motion round the sun, is moving away from α Aurigæ, and as α Aurigæ is also running away from the sun, the star-light becomes so much redder than normal that the stellar hydrogen line is shifted completely to one side of the hydrogen and artificial line.

The accuracy of these results may be proved as follows:—

If we measure all the photographs of α Aurigæ which Dr. Vogel has obtained, we can derive from each a determination of the relative velocity of the motion of the star with respect to our earth.

Of course these velocities are made up of the velocity of motion of α Aurigæ with respect to the sun (which we may reasonably assume to be a uniform velocity) and the velocity of the earth due to its motion round the sun. But the velocity of the earth's motion in its orbit is known with an accuracy of about one five-hundredth part of its amount, and therefore, within that accuracy, we can allow precisely for its effect on the relative velocity of the earth and α Aurigæ. When we have done so we get the following results for the velocity of the motion of α Aurigæ with respect to the sun. You see by the following table how beautifully they agree in the Potsdam results, and how comparatively rough and unreliable are the results obtained by the older method at Greenwich:—

α *Aurigæ—Potsdam.*

Date.	Observed relative motion of earth and star. Miles per sec.	Motion of earth.	Concluded motion. Star relative to the sun.
1888.			
October 22 ...	+ 2·5	− 13·0	+ 15·5
,, 24 ...	+ 3·1	− 12·4	+ 15·5
,, 25 ...	+ 3·1	− 12·4	+ 15·5
,, 28 ...	+ 2·5	− 11·8	+ 14·3
November 9 ...	+ 6·8	− 8·7	+ 15·5
December 1 ...	+ 11·8	− 3·1	+ 14·9
,, 13 ...	+ 14·9	+ 0·6	+ 14·3
1389.			
January 2	+ 20·5	+ 6·8	+ 13·7
February 5 ...	+ 32·9	+ 14·3	+ 18·6
March 6	+ 34·2	+ 16·8	+ 17·4

α *Aurigæ—Greenwich.*

Date.	Observed relative motion of earth and star. Miles per sec.	Motion of earth.	Concluded motion. Star relative to the sun.
1887.			
January 25 ...	+ 16·4	+ 12·6	+ 3·8
February 16 ...	+ 34·4	+ 15·9	+ 18·5
October 22 ...	+ 39·8	− 13·5	+ 52·3
,, 25 ...	+ 25·4	− 13·0	+ 38·4
,, 29 ...	+ 40·6	− 12·1	+ 52·7
1388.			
December 7 ...	+ 29·0	− 1·2	+ 36·2
1889.			
February 15 ...	+ 23·8	+ 16·0	+ 7·8
March 5	+ 20·3	+ 17·1	+ 3·2
September 17 ...	+ 18·6	− 13·3	+ 33·3
,, 19 ...	+ 21·8	− 16·7	+ 38·5
,, 25 ...	+ 24·8	− 16·5	+ 41·3
November 25 ...	+ 24·5	− 4·9	+ 29·4

I believe that in a few years—at least, in a period of time that one may hope to see—we shall not be content merely to correct our results for the motion of the earth in its orbit only, and so test our observations of motion in the line of sight, but that we shall have arrived at a certainty and precision of working which will *permit the process to be reversed*, and that we shall be employing the spectroscope to determine the velocity of the earth's motion in its orbit, or, in other words, to determine the fundamental unit of astronomy, the distance of the sun from the earth.

I will take as another example one recent remarkable spectroscopic discovery.

Miss Maury, in examining a number of photographs of stellar spectra taken at Harvard College, discovered that in the spectrum of β Aurigæ certain lines doubled themselves every two days, becoming single in the intermediate days. Accurate Potsdam observations confirmed the conclusion.

The picture on the screen shows the spectrum of β Aurigæ photographed on November 22 and 25 of last year. In the first the lines are single; in the other every line is doubled. Measures and discussion of a number of these photographs have shown that the doubling of the lines is perfectly accounted for by the supposition of two suns revolving round each other in a period of four days, each moving at a velocity of about 70 miles a second in its orbit.

When one star is approaching us and the other receding, the lines in the spectrum formed by the light of the first star will be moved towards the blue end of the spectrum, those in the spectrum of the second star towards the red end of the spectrum. Then, as the two stars come into the same line with us, their motions become at right angles to the line of sight, and their two spectra, not being affected by motion, will perfectly coincide; but then, after the stars cross, their spectra again separate in the opposite direction, and so they go on.

Thus by means of their spectra we are in a position to watch and to measure the relative motions of two objects that we can never see apart—nay more, we can determine not only their period of revolution, but also the velocity of their motions in their orbits. Now, if we know the time that a body takes to complete its revolution, and the velocity at which it moves, clearly we know the dimensions of its orbit; and if we know the dimensions of an orbit we know what attractive force is necessary to compel the body to keep in that orbit, and thus we are able to weigh these bodies. The components of β Aurigæ are two suns, which revolve about each other in four days; they are only between 7 and 8 millions of miles (or one-twelfth of our distance from the sun) apart, and if they are of equal weight they each weigh rather over double the weight of our sun.

I have little doubt that these facts do not represent a permanent condition, but simply a stage of evolution in the life-history of the system, an earlier stage of which may have been a nebular one.

Other similar double-stars have been discovered both at Potsdam and at Cambridge, U.S., stars that we shall never see separately with the eye aided by the most powerful telescope; but time does not permit me to enter into any account of them.

I pass now to another recent result that is of great cosmical interest.

The Cape photographic star charting of the southern hemisphere has been already referred to. In comparing the existing eye-estimates of magnitude by Dr. Gould with the photographic determinations of these magnitudes, both Prof. Kapteyn and myself have been greatly struck with a very considerable systematic discordance between the two. In the rich parts of the sky—that is, in the Milky Way—the stars are systematically photographically brighter by comparison with the eye-observations than they are in the poorer part of the sky, and that not by any doubtful amount, but by half or three-fourths of a magnitude. One of two things was certain—either that the eye-observations were wrong, or that the stars of the Milky Way are bluer or whiter than other stars. But Prof. Pickering, of Cambridge, America, has lately been making a complete photographic review of the heavens, and, by placing a prism in front of the telescope, he has made pictures of the whole sky like this. [Here two examples of the plates of Pickering's spectroscopic *Durchmusterung* were exhibited on the screen.] He has discussed the various types of the spectra of the brighter stars, as thus revealed, according to their distribution in the sky. He finds thus that the stars of the Sirius type occur chiefly in the

Milky Way, whilst stars of other types are fairly divided over the sky.

Now, stars of the Sirius type are very white stars, very rich, relative to other stars, in the rays which act most strongly on a photographic plate. Here, then, is the explanation of the results of our photographic star-charting and of the discordance between the photographic and visual magnitudes in the Milky Way.

The results of the Cape charting further show that it is not alone to the brighter stars that this discordance extends, but it extends also, though in a rather less degree, to the fainter stars of the Milky Way. Therefore, we may come to the very remarkable conclusion that the Milky Way is a thing apart, and that it has been developed perhaps in a different manner, or more probably at a different and probably later epoch, from the rest of the sidereal universe.

Here is another interesting cosmical revelation which we owe to photography.

You all know the beautiful constellation Orion, and many in this theatre have before seen the photograph of the nebula which is now on the screen, taken by Mr. Roberts.

Here is another photograph of the same object, taken with a much longer exposure. You see how over-exposed, in fact burnt out, the brightest part of the picture is, and yet what a wonderful development of faint additional nebulous matter is revealed.

But I do not think that many persons in this room have seen *this* picture, and probably very few have any idea what it represents. It is from the original negative taken by Prof. Pickering, with a small photographic lens of short focus, after six hours' exposure in the clear air of the Andes, 10,000 feet above sea-level.

The field embraces the three well-known stars in the belt of Orion, on the one hand, and β Orionis (Rigel) on the other. You can hardly recognize these great white patches as stars; their ill-defined character is simply the result of excessive over-exposure. But mark the wonders which this long exposure with a lens of high intrinsic brilliancy of image has revealed. Here is the great nebula, of course terribly over-exposed; but note its wonderful fainter ramifications. See how the whole area is more or less nebulous, and surrounded as it were with a ring fence of nebulous matter. This nebulosity shows a special concentration about β Orionis.

Well, when Prof. Pickering got this wonderful picture, knowing that I was occupied with investigations on the distances of the fixed stars, he wrote to ask whether I had made any observations to determine the distance of β Orionis, as it would be of great interest to know, from independent evidence, whether this very bright star was really near to us or not. It so happens that the observations were made, and their definitive reduction has shown that β Orionis is really at the same distance from us as are the faint comparison stars. β Orionis is, therefore, probably part and parcel of an enormous system in an advanced but incomplete state of stellar evolution, and that what we have seen in this wonderful picture is all a part of that system.

I should explain what I mean by an elementary or by an advanced state of stellar evolution. There is but one theory of celestial evolution which has so far survived the test of time and comparison with observed facts, viz. the nebular hypothesis of Laplace. Laplace supposed that the sun was originally a huge gaseous or nebulous mass, of a diameter far greater than the orbit of Neptune. I say *originally*—do not misunderstand me. We have finite minds; we can imagine a condition of things which might be supposed to occur at any particular instant of time however remote, and at any particular distance of space however great, and we may frame a theory beginning at another time still more remote, and so on. But we can never imagine a theory beginning at an infinite distance of time or at an infinitely distant point in space. Thus, in any theory which man with his finite mind can devise, when we talk of *originally* we simply mean at or during the time considered in our theory.

Now, Laplace's theory begins at a time, millions on millions of years ago, when the sun had so far disentangled itself from chaos, and its component gaseous particles had by mutual attraction so far coalesced, as to form an enormous gaseous ball, far greater in diameter than the orbit of the remotest planet of our present system. The central part of this ball was certainly much more condensed than the rest, and the whole ball revolved. There is nothing improbable in this hypothesis. If gaseous matter came together from different parts of space, such coalition would unquestionably occur, and as in the meeting of opposite streams of water or of opposite currents of wind, vortices would be created, and revolution about an axis set up, such as we are familiar with in the case of whirlpools or cyclones. The resultant would be rotation of the whole globular gaseous mass about an axis.

Now this gaseous globe begins to cool, and as it cools it necessarily contracts. Then follows a necessary result of contraction, viz the rotation becomes more rapid. This is a well-known fact in dynamics, about which there is no doubt. Thus, the cooling and the contracting go on, and, simultaneously, the velocity of rotation becomes greater and greater. At last the time arrives when, for the outside particles, the velocity of rotation becomes such that the centrifugal force is greater than the attractive force, and so the outside particles break off and form a ring. Then, as the process of cooling and contraction proceeds still further, another ring is formed, and so on, till we have, finally, a succession of rings and a condensed central ball. If from any cause the cooling of any of these rings does not go on uniformly, or if some of the gaseous matter of the ring is more easily liquefied than others, then probably a single nucleus of liquid matter will be formed in that ring, and this nucleus will finally, by attraction, absorb the whole of the matter of which the ring is composed—at first as a gaseous ball with a condensed nucleus, and this will finally solidify into a planet. Or, meanwhile, this yet unformed planet may repeat the history of its parent sun. By contraction, and consequent acceleration of its rotation, it may throw off one or more rings, which in like manner condense into satellites like our moon, or those of Jupiter, Saturn, Uranus, or Neptune. Such, very briefly outlined, is the celebrated nebular hypothesis of Laplace. No one can positively say that the hypothesis is true, still less can anyone say that it is untrue. Time does not permit me to enter into the very strong proofs which Laplace urged in favour of its acceptance.

But I beg you for one moment to cast your imaginations back to a period of time long antecedent to that when our sun had begun to disentangle itself from chaos, and when the fleecy clouds of cosmic stuff had but commenced to rush together. What should we see in such a case, were there a true basis for the theory of Laplace? Certainly, in the first place, we should have a huge whirlpool or cyclone of cosmic gaseous stuff, the formation of rings, and the condensation of these rings into gaseous globes.

Remembering this, look now on this wonderful photograph of the nebula in Andromeda, made by Mr. Roberts. In the largest telescopes this nebula appears simply as an oval patch of nearly uniform light, with a few dark canals through it, but no idea of its true form can be obtained, no trace can be found of the significant story which this photograph tells. It is a picture that no human eye, unaided by photography, has ever seen. It is a true picture drawn without the intervention of the hand of fallible man, and uninfluenced by his bias or imagination. Have we not here, so at least it seems to me, a picture of a very early stage in the evolution of a star-cluster or sun-system—a phase in the history of another star-system similar to that which once occurred in our own—millions and millions of years ago, when our earth, nay, even our sun itself, "was without form and void," and "darkness was on the face of the deep."

During this lecture I have been able to trace but very imperfectly the bare outlines of an astronomer's work in a modern observatory, and to give you a very few of its latest results—results which do not come by chance, but by hard labour, and to men who have patience to face dull daily routine for the love of science—to men who realize the imperfections of their methods, and are constantly on the alert to improve them.

The mills of the astronomer grind slowly, and he must be infinitely careful and watchful if he would have them, like the mills of God, to grind exceeding small.

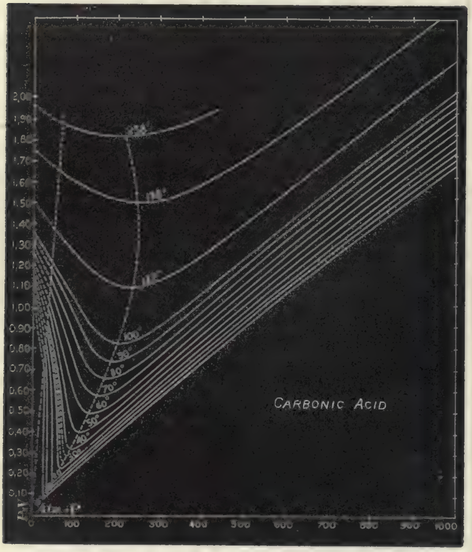
I think he may well take for his motto these beautiful lines—

> "Like the star
> Which shines afar,
> Without haste,
> Without rest,
> Let each man wheel
> With steady sway,
> Round the task
> Which rules the day,
> And do his best."

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 12.—M. Duchartre in the chair.—On the theory of the antagonism of visual fields, by M. A. Chauveau.—An apparatus for carrying out various experiments connected with the study of binocular contrast, by the same author. The instrument described is that used by M. Chauveau in the experiments the results of which were communicated to the Academy on September 7 and 21. In the main it consists of a stereoscope having arrangements by means of which exact equality of luminous impressions may be realized, and the colours of the two fields altered independently.—New *réseau* of isotherms for carbonic acid, by M. E. H. Amagat. The author has determined the isotherms of carbonic acid for every 10° from 0° to 100°, and also those corresponding to 32°, 35°, 137°, 198°, and 258°; the pressures having been taken up to 1000 atmospheres. The results obtained are graphically shown in the accompanying figure, in which the abscissæ represent pressures, and products of P × V furnish the ordinates.—Variation of the composition of Jerusalem artichokes at different periods of their growth; *rôle* of the leaves, by M. G. Lechartier. Analyses of the dried black leaves which appear on Jerusalem artichokes in the autumn have been made, and the results compared with analyses of green and yellow leaves. The effect of different fertilizers on their composition has also been studied. It appears that the black leaves must have had the same composition as the green leaves, and the substances which they lose are utilized for the nutrition of the higher leaves of the plants. They preserve their vitality as long as the soil furnishes the plant with sufficient phosphoric acid and potash. But if either of these fertilizers be absent, the leaves begin to dry up.—Observations of Tempel-Swift's periodic comet, made at Paris Observatory with the West Tower equatorial, by M. G. Bigourdan. Observations for position were made on October 8 and 9. It is remarked: "The comet is an excessively feeble nebulosity, at the extreme limit of visibility: it is round, from 1'·5 to 2' in diameter, and slightly brighter towards the centre."—Observations of the same comet, made at Paris Observatory with the East Tower equatorial, by Mdlle. D. Klumpke. An observation for position was made on October 9.—Experimental researches on "personal equation" in transit observations, by M. P. Stroovant. The author has determined his "personal equation" for observations of stellar points and disks under different conditions of illumination of the field of the telescope employed. His equation was very different when the preceding edge was observed to transit than when the passage of the following edge was noted. It was also subject to a slight variation. Observations by the "eye and ear method" show a tendency to choose certain tenths of a second in preference to others.—On conjugate systems and on the deformation of surfaces, by M. E. Cosserat.—On turbo-machines, by M. Rateau.—Variation of the electromotive force of piles with pressure, by M. Henri Gilbault. Taking the formula $q\frac{dE}{dp} = dv$, in which E = electromotive force, q the quantity of electricity developed and producing a variation of volume v, and p the pressure, the author has calculated the variations of the electromotive force of different piles, and finds that the results agree extremely well with those arrived at experimentally up to a pressure of 100 atmospheres.—A multitubular electric accumulator, by M. D. Tommasi.—Calculation of the specific heats of liquids, by M. G. Hinrichs.—Melting-point of certain binary organic systems, by M. Léo Vignon.—Calorimetric researches on the state of silicium and aluminium in cast-irons, by M. F. Osmond.—Heat of formation of platinic bromide and of its principal compounds, by M. Léon Pigeon.—Contribution to the study of hematozoaires; on the hematozoaires of the frog, by M. Alphonse Labbe.



BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Life-Romance of an Algebraist: G. W. Pierce (Boston, Cupples).—Blackie's Science Readers, III. (Blackie).—Recenti Progressi nelle Applicazioni dell' Elettricitá; Parte Prima, Delle Dinamo: Prof. R. Ferrini (Milano, Hoepli).—A Manual of Logic: J. Welton, vol. i. (Clive).—Text-book of Comparative Anatomy: Dr. A. Lang; translated by H. M. and M. Bernard. Part 1 (Macmillan).—Photography Applied to the Microscope: F. W. Mills (Iliffe).—Photographic Pastimes: H. Schnauss, translated (Iliffe).—Handy List of Books on Mining: H. E. Haferkorn (Gay and Bird).—A Treatise on Nitrogen (J. Heywood).—A Contribution to the History of Rain-Gauges: G. J. Symons.—The Constitutional Development of Japan, 1853–81: T. Ivenaga (Baltimore).—Praktisches Taschenbuch der Photographie: Dr. E. Vogel (Berlin, Oppenheim).—Bulletin of the American Geographical Society, vol. xxiii. No. 3 (New York).—Encyklopaedie der Naturwissenschaften, 65 and 66 Liefg. (Williams and Norgate).

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THURSDAY, OCTOBER 29, 1891.

COPTIC PALÆOGRAPHY.

Album de Paléographie Copte pour servir à l'Introduction Paléographique des "Actes des Martyrs de l Égypte." Par Henri Hyvernat. (Paris: Leroux, 1888.)

IN all the wide range of subjects connected with archæology, it would perhaps be difficult to find one so little studied as that the name of which stands at the head of this article. It is not that it is unimportant; on the contrary, it is most important; it cannot be said to be uninteresting, for the most elementary study of the subject shows it to possess considerable attractions for the philologist, historian, and antiquary. The little interest which, until the last few years, has been shown in matters relating to the Coptic language and literature is probably to be attributed to the fact that printed Coptic texts are scarce, and that the comparatively few manuscripts which exist are scattered throughout the libraries of Europe.

It will be remembered that in the year 1885 M. Hyvernat began to publish the martyrdoms of famous Coptic saints, with a translation in French entitled "Les Actes des Martyrs de l'Égypte"; the Coptic texts were edited chiefly from manuscripts in the Vatican and Borgian Libraries. Considerable interest was aroused by his work, and it was hoped that scholars would soon possess accurate copies of the texts of the martyrdoms which form so large a section of the rich collections of Coptic manuscripts at Rome. It may be argued that the narratives of the sufferings and deaths of Coptic martyrs have much in common, and that a few examples of this class of literature would have been sufficient; but it must be remembered that the historical allusions and incidental remarks made in them give them a value far beyond their importance as religious documents; while the uncommon words, and unusual forms of the Greek words which their writers borrowed, enrich the Coptic lexicon, and afford material for the student of hieroglyphics who makes a comparative study of the dialects spoken by the Copts and by their ancestors the subjects of the Pharaohs. The first volume of the work, in four fasciculi, has appeared, and it is hoped that the second volume, which is promised to contain a critical introduction, &c., will not be long delayed.

Meanwhile, however, M. Hyvernat has given us his "Palæographic Album," and it is to this important publication that we must now give our attention; the scientific plan which he has followed in setting before scholars facts and nothing but facts, and his systematic arrangement of them, make his work most welcome. The first Coptic scholar who gave his attention to the subject of Coptic palæography was Zoega, the Dane, and in his famous "Catalogus Codicum Copticorum," published (after his death) at Rome in 1810, are given seven plates containing specimens of the writing found in Coptic manuscripts of various periods; since that time *facsimile* specimens of important manuscripts have been published, as, for example, a page of the famous Gnostic work, "Pistis Sophia," in the "Facsimiles of Ancient Manuscripts, &c.," issued by the Palæographical Society (Oriental Series, plate 42, 1878).

The work before us contains fifty-seven large folio plates, upon which are reproduced by photography about one hundred examples of Coptic writing; the execution of these plates is perfect, and M. Hyvernat has shown great knowledge and judgment in making the selection. The original manuscripts are preserved in Rome, Milan, Turin, Naples, Paris, London, and Oxford; and the time and labour spent by him in reading and examining them must have been very considerable. The manuscripts—that is, books made of parchment and paper, for M. Hyvernat excludes inscriptions upon stones, and papyri, whether contracts or otherwise—belong to all periods; the earliest cannot be later than the sixth century A.D., and the latest dates from the last century. We have thus for palæographical investigation a field of not less than twelve hundred years.

The specimens of the writings anterior to the ninth century have been taken from manuscripts which are, by the common consent of the best authorities, admitted to belong to this period; all those after the ninth century are taken from dated manuscripts, and thus there is no doubt possible as to their age. The wisdom of this plan is evident, for, in the case of uncial writing, the character of which practically remained unchanged among the Copts for centuries, it is almost impossible to assign an exact date to a manuscript unless a dated standard is forthcoming. Coptic manuscripts which are to be attributed to the sixth or seventh century are rare, and as examples of them M. Hyvernat has selected the Gnostic treatise called "Pistis Sophia"[1] (Brit. Mus., No. 5114) and the life of St. Pachomius;[2] the pages are small quarto in size, with two columns of writing to the page, and ornamentation is rare. In the seventh and eighth centuries the writing becomes firmer and bolder, the pages are larger, and the sides of the columns are ornamented with graceful designs and birds (doves?). The picture of Job and his three daughters (Pl. 5), wearing Byzantine costumes and ornaments, is very instructive. Pl. 6 gives a leaf from a palimpsest manuscript, inscribed in Coptic with verses from the Old Testament, and in Syriac with the martyrdom of St. Peter of Alexandria.

Of the tenth and eleventh centuries we have fine specimens of manuscripts containing homilies, canons, sermons, martyrdoms, &c.; the pages are large, the writing, in two columns, is bold and handsome, the initial letters of paragraphs are large, and stand away from the columns, which are often profusely decorated with birds, flowers, ornaments in the shape of vases, &c. The last pages of works of this period often contain portraits of those who are referred to in them, and the larger manuscripts have full-page illustrations of the subject-matter; as, for example, Theodore the General overthrowing the dragon and rescuing the widow's children (Pl. 16), St. Mercurius destroying Julian the Apostate (Pl. 17), and "Moses the Prophet" standing with bare feet by the side of the burning bush (Pl. 19). On Pls. 14, 21, and 32 are some interesting examples of Coptic cryptography and cursive writing. At the end of the tenth century the first page of each work in a manuscript is ornamented with deep borders of tracery and interlacing in various colours, and the initial letters are very large (Pls. 34, 38).

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[1] The text, with Latin translation, was published by Schwartze at Berlin in 1851.

[2] The text, with French translation, was published by Amélineau, "Histoire de Saint Pakhôme" (Paris, 1889).

A fine example of the writing and illumination of the thirteenth century is that given on Pl. 1, from a Coptic and Arabic Evangelarium written A.D. 1250; in it St. Mark, seated, is about to receive in a napkin the book of the Gospels from St. Peter, and by his side is a stand in the shape of that used to hold a Koran; opposite is a scene in which John the Baptist is baptizing Christ in the Jordan, in the presence of two angels, who hold napkins, and above them is descending from blue heavens the Holy Ghost in the form of a dove. Behind John the Baptist is a tree, in the trunk of which an axe has been struck. Of illustrated Gospels of this period we have excellent specimens on Pls. 44–47, where the Transfiguration, the devils entering the swine, the Marriage at Cana, the Last Supper, the Crucifixion, &c., display a quaint mixture of ancient Coptic, Byzantine, and Arab methods of illumination and ornamentation. Of manuscripts of the thirteenth and fourteenth centuries good examples are given on Pls. 50 foll., with *facsimiles* of the elaborate crosses of the period and of the portraits of the four Evangelists in circles. The space at our disposal will not allow a more detailed description of the contents of the "Album de Paléographie Copte" than that given above, which will serve to indicate the great value of the work to scholars.

The Copts, or "Egyptian" Christians, played no unimportant part in the history of Egypt after the preaching of St. Mark at Alexandria, A.D. 64; and from that time until the present day they have steadily and consistently maintained their religious opinions without change. They clung fast to their language, in spite of the widespread use of Greek in Egypt in the earlier centuries of this era; and although they adopted the Greek alphabet, with the addition of some few signs from the demotic, and borrowed largely from the Greek vocabulary, they did not cease to write their books in Coptic nor to celebrate the services of their Church in that language. After the conquest of Egypt by the Arabs, the Copts held positions of dignity and importance there for some hundreds of years; but about the twelfth century they seem to have fallen into poverty and contempt, and about a century later it seems that they ceased to produce literary works; moreover, the growing custom of adding Arabic translations by the side of the Coptic texts proves that the knowledge of Coptic was dying out. During the next few centuries it probably became the study of the learned. In the course of the last two centuries, travellers in the East have brought to Europe numbers of Coptic manuscripts, and among those deserving special mention are Pietro della Valle, and Huntingdon, Assemani, Curzon, and Tattam. The revival of Coptic learning was begun by Abela, a Maltese; and his work was carried on by Kircher, Petræus, Jablonski, Renaudot, Wilkins, Vansleb, Lacroze, Tuki, George, Zoega, Quatremère, Tattam, and Peyron: among those who have done much excellent work in Coptic during the present century are Schwartze, Lagarde, Revillout, and Rückert. The recent works of Amélineau and Hyvernat show that serious attention is now being paid to the Coptic language for philological and ecclesiastical purposes, and that the publication of new material is going on rapidly.

In conclusion, all lovers of Coptic literature owe a debt of gratitude to M. Henri Hignard, formerly President of the Académie de Lyon, for his liberality in undertaking the expense of publishing this work, and to M. Hyvernat for the excellent way in which he has made use of the funds so generously placed at his disposal.

BRITISH MUSEUM (NATURAL HISTORY) CATALOGUES.

Systematic List of the Frederick E. Edwards Collection of British Oligocene and Eocene Mollusca in the British Museum (Natural History); with References to the Type Specimens from similar Horizons contained in other Collections belonging to the Geological Department of the Museum. By Richard Bullen Newton, F.G.S. Pp. xxviii. and 365, with a large Folding Table. (London: Printed by order of the Trustees. Sold by Longmans and Co.; Quaritch; Dulau and Co.; Kegan Paul, Trench, Trübner, and Co.; and at the Natural History Museum. 1891.)

THE interest which attaches to the records of past periods of our earth's history is greatly enhanced when we find them in the strata forming the very ground beneath our feet. Such is the explanation of the origin of the well-known Edwards Collection of Eocene Mollusca, which forms the subject of the volume before us. Mr. Frederick Edwards resided at Hampstead some fifty years ago, at a time when the Primrose Hill tunnel of the London and North-Western Railway was formed, and the Archway Road, Highgate, had lately been cut, and, later still, the Great Northern tunnel under Copenhagen Fields. These, and many brick-field excavations in the north of London, led to the discovery of abundant fossil-remains around his residence, and attracted the attention not only of Mr. Edwards, but of Dr. Bowerbank, Mr. Wetherell, Prof. John Morris, Mr. Searles V. Wood and his son, Mr. Sowerby, Mr. White, Mr. Page, and other geologists living in Highbury, Highgate, Hampstead, and Kentish Town, who formed among themselves a small Naturalists' Society, known as the "London Clay Club," the members of which met periodically at each other's houses, to compare and exchange specimens, and to name the fossils they had discovered in the London clay. Mr. Wetherell, Dr. Bowerbank, and Mr. Frederick Edwards made most extensive collections; but, whilst Wetherell and Bowerbank collected from the London Clay, the Chalk, and other formations, Mr. Frederick Edwards devoted all his attention to the Mollusca of the London Clay and other Tertiary beds of the south-east of England. All his summer holidays were spent in such spots as the New Forest (where, at Brockenhurst, Bramshaw, Lyndhurst, and many other spots, assisted by Mr. Henry Keeping, he opened numerous trial-pits), or at Barton and Hordwell on the coast of Hampshire, Colwell Bay, Headon Hill, Osborne, Hempsted, Bembridge in the Isle of Wight, and Bracklesham Bay, Sussex. He collected at all these places, and *carefully recorded* the localities from whence his specimens were derived. With infinite care he mounted and named these delicate Tertiary shells, and the beautiful specimens so prepared have been preserved in their entirety in the National Museum.

After the formation of the Palæontographical Society, a large number of Mr. Edwards's Mollusca were monographed by him from 1849 to 1860 (five parts), and continued by S. V. Wood, 1861 to 1877 (four parts); and papers were published in the *London Geological Journal*, the *Geologist*, the *Geological Magazine*, and the Quarterly Journal of the Geological Society of London.

The unpublished labour which Mr. Edwards expended on his cabinets greatly exceeded that which he devoted to the publication of a part of their contents, as may readily be seen by a study of his collection; and when it is known that this work was all performed in the leisure hours of a busy life as a Master-in-Chancery, hearing and deciding law cases in Chambers all day, one is astonished to find how much he was able to accomplish.

The collection contains no fewer than 39,191 specimens, referred to 1805 species of Mollusca, divided into the following classes:—

85	genera and	648	species of	Lamellibranchiata,
162	,,	1127	,,	Gasteropoda,
2	,,	14	,,	Scaphopoda,
6	,,	16	,,	Cephalopoda.
		1805		

Of this number 585 are manuscript species, proposed by F. E. Edwards, which have not yet been described; so that nearly one-third has to be deducted from the above total if we would arrive at the actual number of species already figured and described.

It may be objected that these manuscript names ought not to have been printed; but Mr. Newton points out, in the preface to his catalogue, that these have got into circulation abroad in lists published by German and French palæontologists, with whom Mr. Edwards had corresponded, until, like some paper-currencies, they have obtained for themselves an artificial value, and it would be inconvenient to omit to mention them in a list of Mr. Edwards's own collection. Mr. Newton, moreover, promises shortly to describe and figure them, thus giving them their full *specie-value*, a promise which we sincerely trust he will find leisure to perform.

In addition to the specimens in F. E. Edwards's own collection, figured and described by himself and others, all those in the Brander, Sowerby, Dixon, Bowerbank, and Wetherell collections are duly recorded; so that much valuable information as to the whereabouts of these types, and references to the works in which they are recorded, has been carefully brought together in this volume by Mr. Newton.

Apart from the vast variety, as well as the rare beauty of form, by which the Mollusca of the Eocene period at once arrest the attention of even the most unlearned, to the student of palæontology they afford unmistakable evidence of the existence in this earliest Tertiary period of subtropical marine conditions over this portion of the earth's surface, which now forms South-eastern England. Several extinct forms of Nautilus and Cuttlefishes, associated with huge species of *Cerithium*, Cowries, Cones, Volutes, and such genera as *Rostellaria*, *Mitra*, *Marginella*, *Cancellaria*, *Oliva*, *Ovula*, and *Seraphs*, with *Terebra*, *Pirenia*, *Phorus*, *Solarium*, *Nerita*, and *Chiton*, make up a rich display of Mollusca belonging to the warmer seas of the globe, and if we add such genera as *Pholadomya*, *Spondylus*, *Crassatella*, and many of the other bivalves, they tell the same tale. Crustacea, Echinodermata, and Corals were also present, together with numerous Turtles, whilst along the shores of the rivers huge Crocodiles patiently awaited the *Palæotheria* and *Anoplotheria* from the neighbouring lands. Terrestrial vegetation, washed down from the Eocene continent, also proves to be of a tropical kind —Palms, Cacti, Dryandra, Maple, Azalea, Acacias, with others, belonging to more temperate latitudes, forming a part of the vegetation of our island to-day. Nor were the terrestrial Mollusca unaffected by the increased temperature, for we find large *Bulimi* and *Helices* unlike those now living in this country, whilst the species of *Limnea* and *Planorbis* were both large and very abundant, and were associated with *Potamides*, *Melania*, and other exotic genera in its streams. That there must have been at that time a close connection between our English Eocene area and the much larger Eocene area of France, cannot be doubted, for the beds of the Paris basin and those of Hampshire and London are capable of close correlation, and many genera and species are common to both areas.

Mr. Newton has fortunately obtained the co-operation of Mr. George F. Harris, who has, in an appendix, added some valuable tables, showing the probable equivalent horizons of our several English Tertiary beds with those on the Continent, in France, Belgium, and Germany, and as far east as Austria and Italy, and southwards to Spain. These tables will prove of the greatest value to the student who seeks to understand, and even to map out, the former geographical extent of the several successive Tertiary deposits of Europe, with their varied land, freshwater, and marine records of past life, both animal and vegetable.

Most of the points dealt with by Mr. Newton in the introduction to his list have reference to questions of priority in names, and explanatory notes in justification of some which have been abolished—either because the name had been pre-occupied for a genus of fishes, or birds, or reptiles, &c., or because it had been discovered that another author had previously described the same shell, and had at an earlier date given it another name. Many old favourites have thus been relegated to obscurity, whilst fresh names, dug up from some forgotten corner, have, by the law of priority, taken their places. Thus:—*Meretrix*, Lamarck, 1799, takes the place of his better known *Cytherea* of 1806, the latter having been applied by Fabricius, in 1805, to a dipterous insect. *Triton*, De Montfort, 1810, gives place to *Lampusia*, Schumacher, 1817, "having been applied by Linnæus to a Cirripede in 1767." But as no genus of Cirripedes is known by that name at present, this is a needless and undesirable alteration, especially as Mr. Newton remarks, "the genus *Triton* still continues a favourite name among conchologists"; we would add, "long may it continue" so. Darwin says: "I cannot doubt that the *Triton* described by Linnæus was only the *exuviæ* of some *Balanus* (probably *B. porcatus*), Linnæus mistaking the proboscidiformed penis for the mouth of his imagined distinct animal" (Darwin's *Balanidæ*, Ray Soc., 1854, p. 158).

It would be an immense gain if every name proposed to be altered had to pass through a regularly-constituted committee of investigation before it was accepted and allowed to pass current; as it is, endless confusion must arise, and needless alterations will for ever be made, serving no good end to science.

Mr. R. B. Newton's systematic list of the Eocene and Oligocene Mollusca of our British strata will prove extremely valuable to all those who take an interest in our Tertiary deposits and their contained organisms. Every curator of a palæontological collection must have it, as a work of reference, by his side, as, for this section of fossils, it takes the place of "Morris's Catalogue," now long out of date. We shall be very glad to see other sections treated in a similar manner—indeed, Messrs. A. Smith Woodward and C. D. Sherborn have already catalogued the fossil Vertebrata of the British Isles in 1890, and the work has been published by Dulau and Co.

THE LIFE AND WORK OF A NORFOLK GEOLOGIST.

Memorials of John Gunn: being some Account of the Cromer Forest Bed and its Fossil Mammalia. Edited by H. B. Woodward and E. T. Newton. Pp. xii, 120; 13 Plates (Portrait and Fossil Mammalia). (Norwich: W. A. Nudd, 1891.)

ALL students of the geology of the eastern and central parts of Norfolk and Suffolk will welcome this book, as giving the well-matured opinions of a geologist whose life-work was chiefly concerned with the Forest Bed and its associated formations, Crag and Drift. Those too who knew Mr. Gunn must be glad to have this memorial of so courteous, kindly, truth-seeking a man. No one enjoyed his friendship but was the better for it, and the writer looks back on days spent in his company, both in the field and at meetings of the Norwich Geological Society, as amongst the happiest events of a long sojourn in the Eastern Counties. Until reading this book he did not know the politics of Mr. Gunn, and he is glad to find another of many instances in which such matters are kept in the background, as regards scientific intercourse and personal friendship.

To those who, like the writer, are not greatly enamoured with biography and its multiplicity of personal details it is satisfactory to find this part of the book artistically treated, by Mr. Woodward, in only 27 pages, which are full of interest. The best memorial of a scientific man is the work that he has done and by which he will be known in the time to come, and it is to Mr. Gunn's work that the editors chiefly direct our attention. After the memoir and about 13 pages of notes on some of his geologic papers, the book takes the form of a short essay on the Cromer Forest Bed and its fossil Mammalia, by the hand of Mr. Gunn himself; that is to say, from notes practically completed by him shortly before his death.

For the task of bringing these matters before the public no better editors could have been chosen. One of them, who, in his Geological Survey work, was brought much in contact with Mr. Gunn, may be called the hereditary geologist of Norfolk. The other has for some years given great attention to the study of the fossil Mammalia of the Forest Bed, and indeed has made himself the chief authority on the subject.

In 1864, Mr. Gunn helped to found the Norwich Geological Society, of which he was the first and the last President, retiring from that post only for six years (1877–83) in order that it should be filled by officers of the Geological Survey who were stationed in Norfolk and Suffolk: a graceful compliment. He was also one of the founders of the Norfolk Archæological Society, an active member of the Norwich Science Gossip Club, and a member of the Norwich Museum, which he enriched by his fine collection of fossil mammals.

Now that coal has been found underground at Dover, and that there may be some chance of a search for it being made in the Eastern Counties, it should be remembered that Mr. Gunn was the first to advocate trial-work in Norfolk.

On the ground that "unanimity does not prevail in regard to the nomenclature of the strata" of the Norfolk cliffs, Mr. Woodward gives a useful table, on p. 40, showing the classifications of Gunn, of Prestwich, and of C. Reid; but that of Wood might have been added with advantage; and he draws attention to the fact that the cliffs are cut back greatly year by year, so that earlier observers may have seen something different from later ones. As the loss of coast is still going on, and the Forest Bed seems not to reach far inland, a happy time may come when that Series will cease to furnish any ground for contention: in this matter the geologists of the future may have to take the work of their foregoers, without the luxury of upsetting it.

In his account of the Forest Bed Series, Mr. Gunn holds to the view that, as a rule, the trees grew on the spots where the stumps are now found. He describes firstly the Estuarine Soil, then the Forest Bed proper, then the Reconstructed Forest Bed (a division not hitherto recognized, and hardly likely to be, reconstruction seeming to occur in various parts of the Series), and lastly the Unio and Rootlet Bed; but it should be noted that other observers take the Forest Bed and the Rootlet Bed to be one. His use of the term Laminated Beds, for the immediate successor of the Forest Bed Series, is unfortunate, as such names usually are, for lamination is common in the Chillesford Clay below and in some of the Glacial Drift above.

Mr. Gunn's notes conclude with remarks, in some detail, on the Proboscidea of the Norwich Crag and of the Forest Bed Series, and on the Cervidæ of the latter, chiefly based, with the plates, on the specimens which he so liberally gave to the Norwich Museum. The notes are followed by a list of his geological and archæological papers, ranging over forty-eight years, from 1840 to 1887.

The plates of Mammalian fossils are well executed; but it is a pity that those of Proboscidea and those of Cervidæ are not numbered consecutively, instead of independently. The portrait that forms the frontispiece is a good one, and the book is well printed.

Few geologists can expect their names to be handed down to posterity by so fine a set of specimens as those of the Gunn Collection in the Norwich Museum, and by so interesting a literary accompaniment as that now noticed.

W. W.

OUR BOOK SHELF.

The Melanesians: Studies in their Anthropology and Folk Lore. By R. H. Codrington, D.D. (Oxford: Clarendon Press, 1891.)

IN this book Dr. Codrington gives us the results of observations and inquiries made in the Melanesian Islands from 1863, when he first visited them, to 1887, when he left the Melanesian Mission. He does not profess to offer a complete account of the Melanesian people; nevertheless, the work is one of great value, for it is in the main a record, not of what Europeans say about the natives, but of what the natives say about themselves. The most careful of European inquirers may, of course, mistake the real significance of what natives tell them; but Dr. Codrington seems to have been at all times fully conscious of this danger, and to have done his best to guard against it.

He begins with a chapter on the discovery of the Melanesian Islands, and on their geology and zoology. The ethnology of Melanesia he does not attempt to deal with; but he discusses thoroughly the facts relating to kinship and marriage connection among the Melanesians, starting with the proposition that the division of the people into two or more classes, which are exogamous, and in which descent is traced through the mother, is the foundation of native society. He also gives a good account of the position of the chiefs. A chapter is devoted to property and inheritance, and this is followed by a description of secret societies and clubs, a knowledge of both of which is essential to a proper comprehension of Melanesian life.

The religion of the Melanesians, like that of all savage and barbarous peoples, is a subject of great difficulty; but Dr. Codrington is able to present clearly what seem to be at least its main outlines. Students of the evolution of religious conceptions will read with especial interest what he has to say about "mana," a supernatural power or influence which is supposed to act in all kinds of ways for good and evil, and which everyone tries to possess or control. The objects of worship are spirits, some of which were formerly men, while others belong to an independent and higher class. All these beings are full of "mana," and many suggestive facts about the popular belief in them will be found in the chapters on sacrifices, prayers, spirits, sacred places and things, magic, possession, and intercourse with ghosts. There are also good chapters on birth, childhood, and marriage; death, burial, and "after death."

The chapters on the arts of life, and on dances, music, and games, contain an immense number of interesting facts, well arranged; and in a chapter entitled "Miscellaneous," the author treats of several disconnected subjects, such as cannibalism, head-taking, and castaways. The concluding chapter is in some respects the best of all. It consists of stories, divided into three groups—animal stories, myths and tales of origins, and wonder tales. These stories are not only pleasant to read, but provide excellent materials for those who devote themselves to the comparative study of folk-tales.

We may note that there are some very good illustrations, especially in the chapter on the arts of life.

Guide to Examinations in Physiography, and Answers to Questions. By W. Jerome Harrison, F.G.S. (London: Blackie and Son, 1891.)

THE author of this little work of forty-eight pages is well known as a successful teacher, of wide experience in connection with classes recognized by the Science and Art Department. It is avowedly a guide to the art of passing an examination, the author giving it as his opinion that "knowledge of any subject is not the *only* requisite to successfully passing an examination in it." Unfortunately, this is, to a certain extent, true. Some candidates are apt to make an injudicious choice of questions, while others, again, spend too little time in studying them, and consequently wander from the point. Few who read Mr. Harrison's notes will fail to profit by the sound advice which he gives.

The first part gives general information about the Science and Art Department and its objects, and applies equally to all the subjects in which its examinations are held. The questions which have been given in the elementary stage since 1882 are answered in Part III. They appear to be sufficiently good to satisfy the examiners.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

A Difficulty in Weismannism.

WEISMANN'S theories of heredity and sexual reproduction have been criticized from many *a priori* points of view. The following remarks are an attempt to apply to his theory of reproduction a test familiar to the mathematician; and assuming its truth, to follow out the deductions from this assumption. The result is a startling one. I believe the following theses will be accepted as an impartial statement of the main points of the theory:—

I. Each primitive germ-cell, of either sex, contains a number of ancestral germ-units, the Ahnenplasmas; and this number is constant, for the species at least.

II. These ancestral germ-units are far more constant and unchangeable in character than the species itself.

III. They lie associated together in the germ-cell without loss or alteration of their individual personalities.

IV. The number contained in the mature ovum and spermatozoon is reduced by one-half; and in the fertilized ovum or oosperm the number is restored to the normal by the summation of the Ahnenplasmas of the two fusing cells. This process is comparable to the shuffling of two packs of cards by taking half from each and joining the talons or remainders to form a new pack.

V. The possible combinations under this process are so numerous as to explain the variations among the offspring of sexual union.

Accepting these statements, we next inquire, How are we to conceive of these ancestral units, the Ahnenplasmas? Two hypotheses may be given in answer to this question:—

A. Each Ahnenplasma unit corresponds to an individual of the species itself; and if put under proper trophic conditions would, singly, reproduce such an individual.

B. The Ahnenplasmas correspond to the primitive Protozoan ancestors, which, according to theory,[1] could alone reproduce modifications due to external causes (acquired modifications).

According to hypothesis *A*, the Ahnenplasmas of living man are Anthropic; those of our Simian forebears were Simian; and so we get Protochordate, and finally Protometazoan Ahnenplasmas in the germ-cells of our more and more remote ancestors. In other words, the *Ahnenplasms have varied indefinitely, and at the same rate with the race.* This inference not only renders the shuffling process unnecessary to explain variation; but it is inconsistent with thesis II., the very foundation of Weismann's theory of heredity.

According to hypothesis *B*, the Ahnenplasmas of all Metazoa being similar and Protozoan, if the *numbers are equal* and the shuffling fair any two parents may beget any offspring whatever; on the plane of thesis V., a lioness might be expected to bring forth a lobster or a starfish or any other animal, which, as we know, does not take place in Nature. The only escape from this result is to assume the postulates—(1) that the number of Ahnenplasmas varies from species to species; (2) that the *number* in the combination and not the *character* of the Ahnenplasmas determines the species. And as there is not a particle of evidence for the latter postulate, we may say that on hypothesis B the theory breaks down by its non-conformity with the facts.

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[1] "Hereditary variability . . . can only arise in the lowest unicellular organisms; and . . . necessarily passed over into the higher organisms when they first appeared" (Weismann, "On Heredity," English edition, p. 279). This passage would seem to render hypothesis *B* necessary for the theory.

We have then the dilemma, from which I see no escape, that the theory is inconsistent, on A with itself, on B with the facts. When once worked out and fairly put into words, which was not so easy as it may appear, this argument seemed so obvious that I felt sure it must have been long since urged, confuted, and dismissed. But not having found any reference to it, I now state it fully, in the hope that the question raised may be thoroughly discussed. Marcus Hartog.

Dublin, October 12.

Rain-making Experiments.

Your last number contains an article by Prof. Curtis on the "rain-making" experiments in Texas, in which no reference is made to the report published in the October number of the *North American Review* by General Dyrenforth, who directed the operations. I wish to call attention to the remarkable differences which exist between the statements of Prof. Curtis, the meteorologist of the expedition, and General Dyrenforth, its director. On August 10, Prof. Curtis, who had not yet arrived at the scene of the experiments, believes that only sharp showers or "good grass-rain" fell; General Dyrenforth says the amount was nearly 2 inches. On August 18, Prof. Curtis says that only 0·02 inch of rain fell; General Dyrenforth says that "drenching rain fell in torrents for two and a half hours," and that driving from the encampment to Midland, a distance of 25 miles, the road traversed was covered for 6 or 8 miles under 4 to 40 inches of water. It is impossible, under these circumstances, for those interested to come to any conclusion at present with regard to the actual results of the experiments. May I draw your attention further to an article which appeared in the *Manchester Guardian* of the 13th inst., in which a suggestion was made precisely similar to that put forward by Prof. Giglioli in your last number. If, as seems probable, the experiments of Mr. Aitken amply suffice to explain any positive results obtained, it is evident that the explosions of hydrogen and oxygen, on which General Dyrenforth relies so much, are useless, and that the smoke-producing rackarock does all the work. In an extremely sceptical and very justly critical article, which follows that of General Dyrenforth in the *North American Review*, Prof. Simon Newcomb, while scouting the "concussion" theories of General Dyrenforth, says, indeed, that smoke particles may possibly serve as nuclei for the condensation of water vapour; but he is evidently unacquainted with the remarkable work of Mr. Aitken, which throws so much light on the matter. H.

Manchester, October 24.

A Rare Phenomenon.

Having just returned from Norway, it may be of interest to record that the band of light which was observed by many of your correspondents on September 11, was remarkably brilliant in N. lat. 62°, extending from the horizon to the zenith, but not beyond. It was nearly, but not quite, equal in width throughout the 90°, and therefore must either have been much wider at the base than at the apex, or else at an immense altitude. Some clue to the estimation of this altitude would be afforded by an accurate record of the zenith distance as observed in England.

I may add that the aurora borealis was distinctly visible in the north and north-west at the same time, but this band rose from the north-east, which led me to conjecture that it might belong to a comet; however, on the following night it did not recur, and I then thought it might have been caused by some sun-lit cirri at a great elevation, but it is now obvious that this was not the case. The remarkable feature was its concurrence with, and yet apparent difference from, the ordinary aurora.

Richmond, Surrey, October 24. W. Duppa-Crotch.

The phenomenon observed by Dr. Copeland (Nature, September 24, p. 494) at 11.18 p.m. on September 10 at Dunecht, by Mr. W. E. Wilson at 9 p.m. on September 11 in Co. Westmeath, and by other observers on the 11th in several parts of England, was observed by a party of three, including myself, at 9.30 p.m. on September 25 at Ballater, Aberdeenshire.

It appeared as an intense white beam of light stretching from east to west and directly overhead, of uniform width and perfectly steady. It seemed quite low down, almost as if it might light up the summit of the church spire were it moved a little further towards the south. At 11.30 the light had become diffuse, and it appeared at a much greater elevation, though maintaining its general direction from east to west. W. N. Hartley.

October 23.

Earthquake at Bournemouth.

We had a sharp momentary shock of earthquake here at four o'clock this afternoon. I happened to have my eyes fixed on a plant with long variegated leaves on my dining-room table. Suddenly there was a heavy sound as of some subterranean fall, and simultaneously the leaves of this plant were violently agitated—waved up and down—for some seconds. It was as if it had risen vertically and then fallen. It was wholly unmoved by so much as a tremor the rest of the afternoon. I tried to reproduce anything like the same disturbance by hand, but without success. Henry Cecil.

Bregner, Bournemouth, October 25.

$W = Mg$.

I had read Mr. Slate's letter (Nature, vol. xliv. p. 445), and admired it; moreover, I found myself in agreement with him. But it seems to me strange that Prof. Greenhill should approve of it. For Mr. Slate takes as his gravitational unit of force "*the weight of one pound under circumstances specified* . . . (*locality, vacuum*)." Surely this implies that he agrees with the theorists (Prof. Greenhill's foes) when they say that "the weight of a given body depends on the local value of g." Prof. Greenhill, on the contrary, speaking of *goods*, says that "*the weight cannot be said to vary with the local value of* g" (Nature, vol. xliv. p. 493), I would ask him, then—

(1) What name does he give to the earth's pull on a given body? Or, what is it that a spring balance measures when the said body is hung from it? He cannot say "its weight"; for the pull referred to varies with g, while Prof. Greenhill's "weight" does not. I conclude that he has no special name for it. The theorists have; and they thereby gain in brevity without losing by ambiguity, since they do not employ the word "weight" in any other sense in their text-books.

I would also repeat the still unanswered question—(2) How does Prof. Greenhill give the expression for hydrostatic pressure at a given depth in any locality, if he banishes "g"? (Nature, vol. xliv. p. 341). And does he conclude that Mr. Slate does not use "g" in hydrostatics?

Again . . . (3) Does Prof. Greenhill, in common with Mr. Slate and the theorists, use the word *mass* in speaking of the fundamental units; and, if so, in what sense?

In the science of dynamics we recognize two properties of matter: . . . (i.) its *inertia*; . . . (ii.) *the attraction between it and other matter*. The theorists use the word *mass* when they refer to quantity of matter as measured by its *inertia*; and they use the word *weight* when they refer to the *attraction* of a given body to the earth. For commercial purposes it is convenient to measure quantity of matter by balancing its weight against that of the standard lump of platinum, its multiples, and sub-multiples. Hence the every-day, slightly ambiguous, use of the word "weight" in matters in which we are not concerned with inertia. But in the science of dynamics, of which Newton's laws are the foundation, we are concerned *primarily* with *inertia*. The theorists, therefore, in their text-books, regard the well-known lump of platinum as *the standard pound, the British unit of mass*. They thus have the word "weight" free, and say (*e.g.*) that *the weight of the standard pound is measured by the resultant pressure that it exerts* (in vacuo) *on the bottom of the box in which it lies*. It requires more than general expressions of condemnation to show that any other system of nomenclature is clearer or less free from ambiguity, or that the equation $W = Mg$ has not as much meaning as any other dynamical equation. (I may refer back to my letter, Nature, vol. xliv. p. 493). W. Larden.

Devonport, September 26.

SOME NOTES ON THE FRANKFORT INTERNATIONAL ELECTRICAL EXHIBITION.[1]

IV.

Alternate Current Motors.

ALTERNATE current motors constitute one of the most striking features at the Frankfort Exhibition, and the commercial use of such motors will probably date from this year, so that the one great objection to the employment of alternating currents for the electric transmission and distribution of power will soon disappear.

It is well known that the direction of rotation of an ordinary series, or shunt, direct current motor is the same whichever way the direct current passes round the motor, in spite of a patent of Mr. Edison's to utilize the contrary fact on electric railways; hence it follows that if an alternate current be sent round such a motor it will start rotating and develop mechanical power. Only a comparatively small power and efficiency, however, will be obtained: first, because the large self-induction of the field magnet of the motor will seriously diminish the strength of the alternating current; secondly, because, in consequence of the rapid reversals of the magnetism, much power will be wasted in heating the iron core of the field magnet, even although this core be laminated like that of the armature.

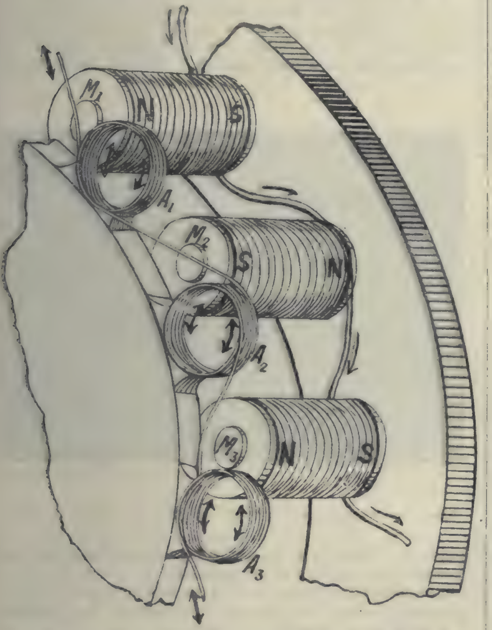

Fig 10.—Alternate current synchronizing motor. ← Direct current. ⟷ Alternating current.

If, on the other hand, a direct current be sent round the field magnet, M_1, M_2, M_3, of an *alternate* current machine, and an alternating current round the armature, A_1, A_2, A_3 (Fig. 10), the armature will not move, because at every two of the successive rapid reversals of the current the armature receives an impulse in opposite directions. To enable such a machine to work as a motor, it is necessary to first make the armature rapidly rotate by mechanical means at such a speed that any armature coil, A_2, moves forward by the distance between two of the poles M_2, M of the field magnet in half the periodic time of the alternation of the current. When this speed has been once attained, the machine will go on running as a powerful and efficient alternate current motor, at a perfectly definite speed, depending simply on the rate of alternation of the current, and independent within wide limits of the load put on the motor.

So that when the armature of the motor is once "in step" with that of the dynamo the two will continue "in step," whatever be the amount, within wide limits, of the power transmitted.

When a considerable amount of power has to be sent from a source to a distant town, and has there to be distributed for light or for driving machinery, it will certainly be best (as far as our present knowledge goes) to use alternating currents in the transmission of the power between the two distant places, because with alternating currents the pressure can so easily be transformed up at the source, and transformed down again at the other end of the line.

But in the distribution of the received power direct currents are the more convenient, since they can be utilized for light, for electroplating and electrotyping, as well as for small and large direct current electromotors, both of which have already reached a considerable degree of perfection, and are of course self-starting. Hence it is probable that there will be employed a synchronizing alternate current motor, coupled mechanically to a direct current dynamo, the latter being used to supply current to the town and excite the field magnets of the motor. Such combinations, seen in Fig. 11, are exhibited by Messrs. Siemens and Halske in the Frankfort Exhibition, the alternate current motor being to the left and the direct current dynamo to the right in the figure.

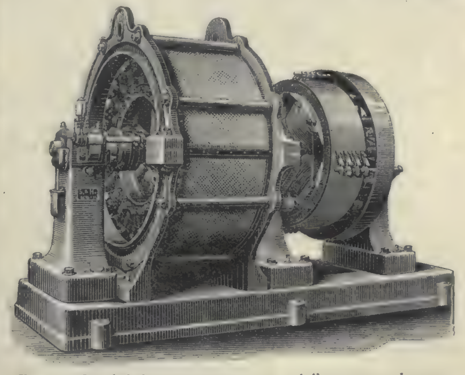

Fig. 11.—Coupled alternate current motor and direct current dynamo.

In the particular form of direct current dynamo shown in Fig. 11, and which represents a type much used now on the Continent, the field magnets are inside the rotating armature, and the wires on the outside of the Gramme ring itself are bare, and act as the commutator.

The impossibility of starting the simple synchronizing motor with an alternating current will be of little consequence when a large amount of power has to be transmitted, seeing that in the receiving station there will be several sets of geared alternate current motors and direct current dynamos, some of which will be always running day and night. Hence, to start any alternate current motor, all that need be done will be to send round the direct current dynamo, attached to the motor to be started, a portion of the direct current that is being produced by one of the running dynamos. This will cause the stationary direct current dynamo to start running as a motor, and when the right speed has been attained—that is, when the motor is in step with the distant alternate current dynamo—the alternate current can be switched on to the alternate current motor.

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[1] Continued from p. 546.

Actual plans are being seriously got out at the present time, for using this exact method to transmit 5000 horse-power over forty miles in Tasmania, the received power being transformed by ten such combinations as are seen in Fig. 11, each of 500 horse-power.

This subdivision of the machinery at the receiving end, if accompanied by a similar subdivision of the generating plant at the sending end of the line, will have another most important advantage, viz. that a breakdown of a dynamo or of a motor will not cause a stoppage in the supply of power. A factory is, no doubt, worked at present with a single large engine; the propulsion of a steamer depends on the turning of a single powerful screw; but neither the unexpected stoppage of the factory engine for say half-an-hour once every two or three months, nor the delay of an Atlantic liner in mid-ocean for the same time once in every half-dozen voyages, would necessarily mean ruin. Were, however, the 10,000 horse-power dynamo at Deptford to be ever finished and worked at its full output, it would be necessary, in order to avoid a temporary hitch leading to the turning off the current from many thousands of glow lamps, and the plunging of a neighbourhood into darkness, to always have dynamos of a capacity of 10,000 horse-power kept idle in reserve.

Experience has shown that the size of each dynamo in a central station should be something like one-tenth of the maximum output, and that it is sufficient to keep one, or at the most two such dynamos, as a reserve, to prevent temporary breakdowns interfering with the steady supply of current. Until, then, a single central station is lighting some 500,000 glow lamps—or more than ten times the total number at present attached to the mains of the London Electric Supply Corporation—no one but the Brunel of electricity would have had the courage to embark on a 10,000 horse-power machine.

At any rate, when during the next year or two it is required to transmit a large amount of power over a considerable distance, it is probable that several alternate current synchronizing motors, each coupled to a direct current dynamo, will be employed at the receiving end of the line.

In cases, however, where there already exists an extended system of distributing alternate currents for electric light, the introduction of motors into small workshops and private houses will hardly be possible, unless the motors can be made self-starting. Mr. Zipernowski's motors, employed for driving the tools in a carpenter's shop at the Frankfort Exhibition, have been made self-starting, and also fairly efficient, by adopting a compromise between the simple direct current motor, which is self-starting but inefficient when used with alternating currents, and the alternate current synchronizing motor, which is efficient but not self-starting.

The device employed by Mr. Zipernowski, and which is based on a communication made by Prof. G. Forbes to the Royal Society of Edinburgh some eight years ago, is as follows:—Send the alternating current round the field magnet as well as round the armature of an alternate current motor (Fig. 10), and attach a commutator to the armature so as to reverse the current flowing round the field magnet every time the armature coils A_1, A_2, A_3 pass the field magnet coils M_1, M_2, M_3. On sending the alternate current round such a motor, the motor will start, but since at first the rapidity of alternation of the current will be far greater than the rapidity of commutation there will be much sparking at the commutator and waste of power. As, however, the armature turns more and more quickly, the commutation will be effected more and more rapidly, until at last the armature will attain such a speed that every time the current is reversed by the distant dynamo the portion of the current flowing round the field magnet of the motor will be commutated by the rapidly rotating armature. Hence the current flowing round this field magnet will now be always in the same direction. But as it will not be always of the same strength there will be more waste of power than with a simple synchronizing motor.

Such an arrangement as that adopted by Mr. Zipernowski, then, furnishes a motor which, although not as efficient and powerful for its weight as the synchronizing motor previously described, has the advantage of synchronizing fairly well, of being self-starting, and of giving far better results than a direct current motor with laminated field magnets used with alternating currents.

It is possible, however, as proved by Prof. Ferraris in 1885, to design an alternate current motor on totally different principles, and to construct a machine which will work not merely without a commutator, but without even any sort of rubbing contact. So that, in fact, the ends of all the wires on a Ferraris motor may be permanently soldered, and the motor left in the hands of a person who knows how to oil a machine but who is quite ignorant of the trimming and adjustment of the brushes of an ordinary direct current motor.

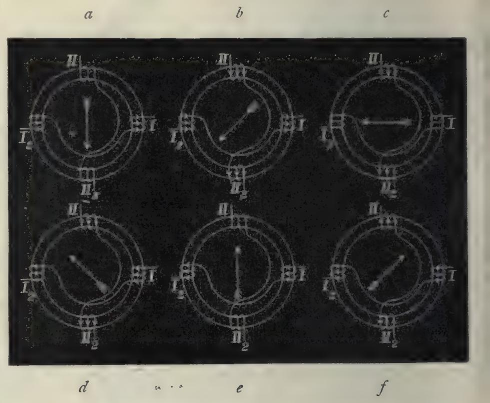

FIG. 12.—Rotating magnetic field produced by two alternating currents.

Round an iron ring are wound four coils, as seen in Fig. 12, and through the two distinct circuits are sent two harmonic alternating currents having the same periodic time and maximum amptitude, but differing by 90° in phase. The ring will therefore receive two magnetizations along two fixed diameters at right angles to one another, the two magnetizations alternating approximately according to the sine function of the time, and differing by 90° in phase. And the composition of these two magnetizations will give a "*rotating magnetic field*," which will make one complete rotation in the periodic time of alternation of the current.

Six values of these two currents are indicated in Fig. 12, the currents in *a*, *c*, and *e*, being of their maximum value in coils I, I_2, and nought in coils II, II_2; while in *b*, *d*, and *f*, the currents in the four coils are equal, being each $\frac{1}{\sqrt{2}}$ of the maximum value. The arrow indicates the position which in each case would be taken up by a suspended compass needle, the point of the arrow indicating the north-seeking pole of the compass needle.

If in place of the suspended compass needle there be a piece of copper, currents will be induced in this copper by the rotating magnetic field, tending to make the cylinder follow the field. Hence, if the copper take the form of a cylinder, with its axis coinciding with the axis of the ring, and supported so that it can rotate about this axis, the cylinder will run after the rotating field until it catches it up, when the two will move nearly synchronously together. On applying a resistance to the rotation of this cylinder—that is, on making the motor do work—the speed of the cylinder will be checked, but a small diminution of speed will cause large currents to be induced in the copper, and a pulling force to be exerted between the rotating field and the lagging cylinder, tending to drag the cylinder round. Hence this arrangement of Prof. Ferraris produces not merely a self-starting alternate current motor, but one which runs almost synchronously with the dynamo for wide variations in the load, and which has neither commutator, rubbing contacts, brushes, nor the possibility of sparking.

Within the past few weeks we have learnt that the idea of obtaining a rotating magnetic field was mentioned by M. Marcel Deprez, in a French patent dated May 1883. In that patent, when speaking of the magnetic field produced by the current flowing round a Gramme ring, he says: "*Cette rotation du champ magnétique peut être obtenue sans faire mouvoir aucune pièce; pour cela on fera naitre le champ à l'aide de deux courants dont les points d'entrée sont sur deux diamètres perpendiculaires; l'aimentation de ce champ sera alors une résultante dont la position dépend des intensités relatives des deux courants, ainsi que cela a été décrit ci-dessus pour le comparateur des courants; il suffit de faire varier le rapport de ces intensités pour faire tourner cette résultante, et avec elle le champ magnétique.*"

It does not, however, appear to have occurred to M. Deprez that this rotation of a magnetic field might be employed to induce currents, and thus give motion to a piece of metal placed inside the Gramme ring; nor does he say anything about two harmonic alternate currents differing by 90° in phase producing the exact variation of current required. Although, then, what may be called the geometrical idea of producing a rotating magnetic field was certainly clearly described by M. Deprez, the credit of rediscovering this principle, and, what is far more important, of applying it in the design of the two-phase alternate current motor, is due to Prof. Ferraris.

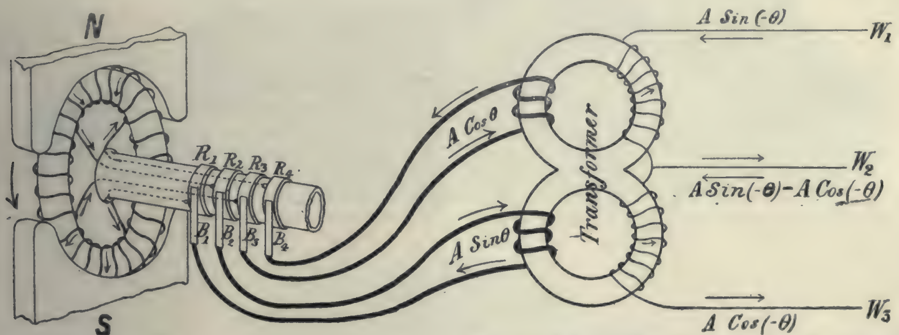

FIG. 13.—Schuckert two-phase alternate current generator and transformer. The arrows indicate the actual direction of the currents for the position of the armature shown.

To increase the strength of the rotating magnetic field, the copper cylinder originally used by Prof. Ferraris was next made hollow, and the interior filled with soft iron, the iron being laminated in planes at right angles to the axis, to prevent currents being induced in the iron; and to make the currents induced in the copper cylinder follow the most useful path the next step was to make a number of cuts through the hollow copper cylinder parallel to the axis of rotation. Practically, then, the rotating portion becomes a laminated cylinder of iron, on which is wound insulated wire parallel to the axis, as in a Siemens armature, but with this difference, that all the wires are electrically joined together at each end of the cylinder.

A two-phase alternate current motor was constructed and used by Prof. Ferraris in his laboratory at Turin in 1885. But not appreciating the practical importance of his own invention, and thinking that no motor requiring more than two wires could interest anyone but the natural philosopher, Prof. Ferraris occupied himself with attempts to utilize the rotatory magnetic field in measuring the resistance of conductors and with mathematical investigations on alternate currents. It was not, therefore, until the spring of 1888 that the results of his researches were published; when, a few months later, commercial motors based on exactly the same principles were brought out with considerable *éclat* by Mr. Tesla, of Pittsburg, who had been working independently in the same direction.

To produce two alternate currents, differing by 90° in phase, the following device (Fig. 13) may be adopted, and is the one employed by Messrs. Schuckert in transmitting power at 2000 volts from the Palm Garden at Frankfort to the Exhibition, and by Messrs. Siemens and Halske for experiments on rotatory field alternate current motors in the Exhibition; the latter firm, however, not employing the special form of transformer shown symbolically in Fig. 13. In addition to the armature of a Gramme dynamo being joined up in the well-known way with the ordinary direct current commutator (this commutator and brushes rubbing on it not being shown in Fig. 13), four points at equal distances on the armature are permanently connected with four metal rings, R_1, R_2, R_3, and R_4, which rotate with the armature. Then it is easy to prove that while the machine is producing a direct current, used for exciting the field magnets as well as for any other purpose desired, the current passing through the wires attached to the brushes B_1, B_2, and the current passing through the wires attached to the brushes B_3, B_4, each alternate very nearly as the sine function of the time, the one reaching its maximum value when the other is nought.

The actual machine employed for this purpose by Messrs. Schuckert is the multipolar dynamo shown in Fig. 14, the direct current commutator and brushes, as well as the four rings and brushes for the two alternating currents, being here seen. If rotated mechanically, it will produce a direct current, as well as two alternating currents differing by 90° in phase; if supplied with a direct current, it will rotate as a motor generating the two alternate currents, and also doing mechanical work if required; lastly, if supplied with the two alternate currents, it will work as a two-phase alternate current motor generating a direct current, as well as doing mechanical work.

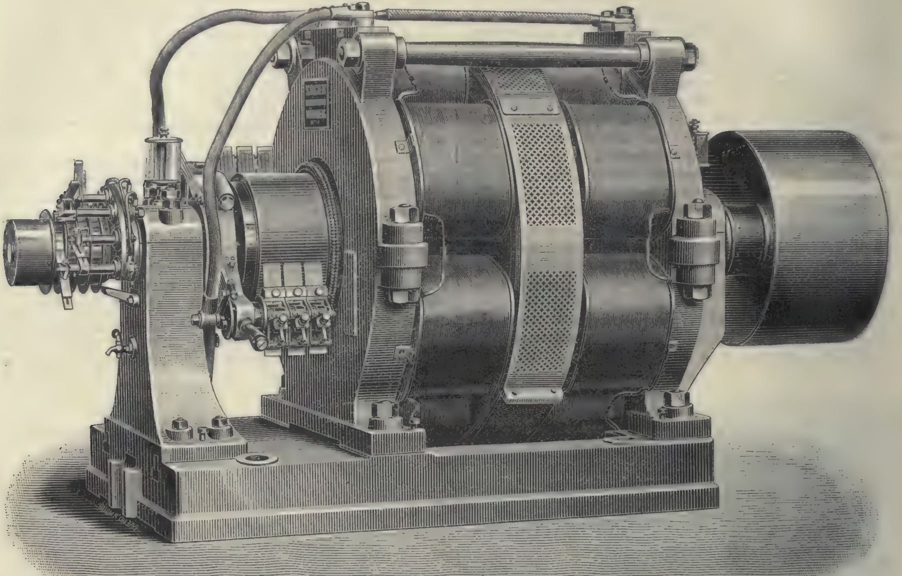

Fig. 14.—Schuckert's two-plane alternate current generator, or motor, or apparatus for transforming two alternate currents into a direct current.

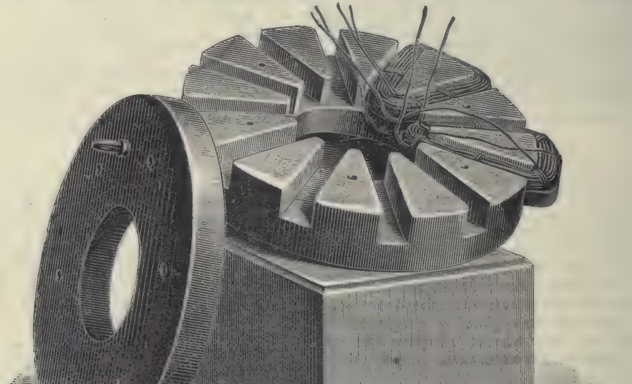

Fig. 15.—Schuckert two-phase alternate current transformer (method of construction).

When transmitting power to a distance, the two-phase alternate potential differences are transformed up from about 100 to 2000 volts; and to enable the transmission to be effected with three wires instead of four, Messrs. Schuckert arrange the transformer at each end of the line as shown symbolically in Fig. 13. Hence, if the currents produced by the dynamo be represented by A sin θ and A cos θ, the currents in the main wires, W_3, W_1, and W_2, will be represented by A cos $(-\theta)$, A sin $(-\theta)$, and A $\{\sin(-\theta) + \cos(-\theta)\}$ respectively.

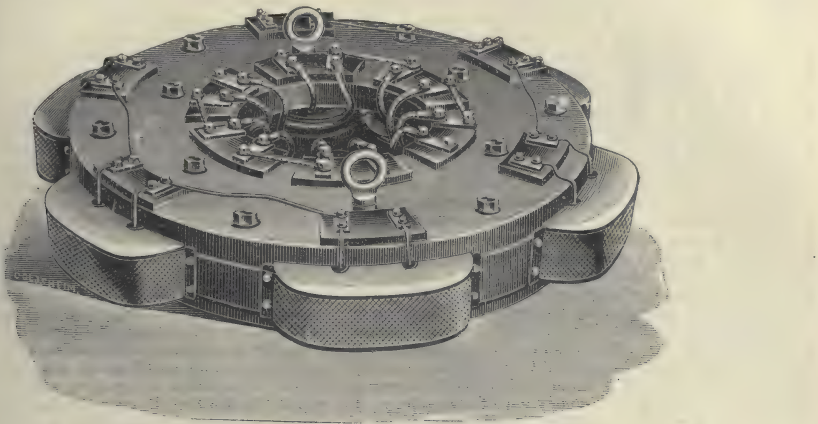

Fig. 16.—Schuckert two-phase alternate current transformer (completed).

The actual method employed by Messrs. Schuckert for winding this special transformer, as well as its appearance when completed, are seen from Figs. 15 and 16. This transformer, then, instead of consisting of merely a double ring of laminated iron as indicated in the symbolical diagram, Fig. 13, may be regarded as being composed of a connected series of laminated iron rings, each of a wedge-shaped cross-section.

(*To be continued.*)

THE OXFORD UNIVERSITY MUSEUM.[1]

THE following memorandum is based, not only upon observations made during a recent visit to Oxford, but also upon a fairly intimate knowledge of the origin and progress of the different departments of the Museum, acquired at various intervals of time extending over more than thirty years.

In entering upon the consideration of the subject which you have referred to me, it will first be necessary to define the purposes for which the Museum is maintained. These I take to be somewhat manifold, but they may be classed as follows:—

A. The first and main purpose is undoubtedly to assist in the educational work of the University, by illustrating the teaching of the professors and lecturers.

Besides this, however, it subserves, to a greater or less degree, other and what may be considered, as compared with the first, secondary, but nevertheless important functions. These are—

B. The exhibition of a collection, arranged in a systematic, orderly, and attractive manner, open to the inspection, under proper regulations, of all members of the University, and also of residents in and visitors to the town, which shall tend to awaken and keep up an interest in various subjects of which most educated persons, besides those actually engaged at the moment in obtaining instruction, desire to possess some knowledge. Such a collection is a most legitimate adjunct to the University as a place of general culture.

C. Certain collections have already, and possibly will in future, become added to the general Museum, the aim and scope of which reach beyond either of the above, being of value, not to the ordinary student, not to the man or woman of average general culture, but only to the advanced student who wishes to enter seriously into the pursuit of some special branch of knowledge. Such is the Hope Collection of Insects, and to a certain extent the Pitt-Rivers Ethnographical Collection.

It is a grave question how far such collections should be maintained at the cost of the University. On the one hand, they must be a cause of expense, without which no collection of any value can be maintained; and the larger and better ordered they are, the greater must be the cost of maintaining them. Unless properly cared for, not only as regards actual preservation of the objects contained in them, but also as regards the continual re-arrangements and augmentations necessitated by the advance of science, they will become comparatively valueless in the course of time. If the care of many such collections were undertaken unaccompanied by special endowments for their maintenance, the burden would become such as only a national institution could afford.

On the other hand, looking at the University, not merely as a place for the education of youth, but also as a centre of culture for the whole country, the possession of some such collections is of great importance. As they contain in them objects which can be found nowhere else, they attract men of learning and science, not only from other parts of the country, but also from distant places, to visit the University, or even to become permanent residents. The value of collections of rare books, even upon subjects interesting to scholars whose numbers are very limited, have long been recognized. From the same point of view, special collections of rare specimens of natural history or works of art may take their place in the general scheme of a University Museum, but the care of such collections should not be undertaken without full consideration as to whether the means will be forthcoming to maintain them in a state of efficiency.

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[1] Prof. Flower's Report to the Committee on Collections appointed by the Delegates of the University Museum, Oxford, dated March 14, 1891.

I have alluded to the Pitt-Rivers Collection as coming partly under this head, but, admirably and instructively displayed as it now is, it may also be considered as belonging to my second category; and the numerous human interests awakened by a study of its contents, and the many branches of culture it comes in contact with, make it an adjunct to the Museum, of the great importance of which no one should entertain a doubt. I should be glad to remark, in passing, that the building in which it is housed appears to me the most successful, as regards economy of space, capacity for orderly arrangement, and good lighting, of any with which I am acquainted.

The next point for consideration is the nature and extent of the subjects to be illustrated in the Museum (excluding the special Pitt-Rivers Collection just referred to). These seem already to have been determined as including physiology, human anatomy, comparative anatomy, animal morphology, zoology, pathological anatomy, palæontology, geology, and mineralogy; therefore the whole of animal biology (botany being provided for elsewhere), with the addition of geology and mineralogy. The teaching of these subjects is divided between the Regius Professor of Medicine, the Waynflete Professor of Physiology, the Linacre Professor of Human and Comparative Anatomy, the Lecturer in Human Anatomy, the Hope Professor of Zoology, the Professors of Geology and of Mineralogy. It must be recognized by everyone that the boundaries of these subjects are most difficult to define, and must be constantly shifting with the advance of knowledge. For instance, comparative anatomy and palæontology may both be included under the broad general heading of zoology, which without the aid of both can be but imperfectly understood. Whatever dividing lines are drawn between different sections of the collection, identical specimens are often required to illustrate more than one subject. The remains of extinct animals are required to complete the story of their living representatives; they are also required to illustrate the ancient history of the earth, and to define the progress of geological time and the order and succession of strata. The relation between the collections used to illustrate the teaching of the Waynflete, the Linacre, and the Hope Professors, must also be more or less arbitrary and artificial. In all these matters mutual convenience must be studied, and the specimens which lie on the borderland of two subjects should be made in some way available for the teaching of both, otherwise a great duplication will be necessary.

With regard to general administration, it appears to me desirable that there should be a governing body for the whole Museum, comparable to the standing committee of the Trustees of the British Museum, or the Museum Committee of the Royal College of Surgeons, or the Museums Syndicate of the University of Cambridge. The Delegates constitute such a body at Oxford, but possibly their constitution or powers might be modified and more clearly defined than they seem to be at present.

This body should be composed of members of the University specially selected for fitness for the office; seven or nine would probably be the most convenient number, so that representatives may be found upon it of various branches of science included in the Museum, and also some members of general business or administrative capacity. They should meet at occasional and stated intervals, and should determine general questions affecting the Museum as a whole, the relations of its component elements one to another, the allotment of space and the apportionment of the grants for the service of each department, the general control of expenditure, and also the care of the building, furniture, &c. It is not advisable that they should interfere with the details of the arrangement of each department as long as these appear to be satisfactorily carried out. The Keeper of the Museum should be the active executive officer of this governing body, carrying out their views in the intervals of the meetings, and bringing before their notice any subjects which seem to require their consideration.

Each professor, as the representative of the most advanced state of knowledge of his subject, should be the responsible curator of the specimens belonging to his department, having such assistance provided him as may be needful. He should be called upon to present to the governing body an annual report of the condition of the collections under his care, and of the accessions which have been made to it during the year.

The actual specimens in the various collections will naturally arrange themselves, both as regards the purpose for which they are kept, and their mode of conservation, under three distinct classes.

1. A working set, mostly of common objects, which, if damaged, can be readily replaced, and which can be put at the disposition of the ordinary student to examine and handle. Such collections are absolutely essential to practical teaching, but they should form no part of the permanent Museum of the University, and should be kept in the rooms specially devoted to study.

2. The permanent exhibited series displayed in the grand court and corridors of the Museum, the use of which, in addition to teaching students, is referred to under the heading B, near the beginning of this report. Great care is required in selecting and arranging these, as well as in their preservation and display. Every specimen exhibited should have a definite object, and should be so placed that it can be thoroughly well seen. As a general rule they should be so arranged as to show what they are intended to teach without moving them from their places, and if this must be done under proper restrictions, all due precautions should be used that they do not become damaged or destroyed. Although for the purposes of custody, arrangement, and nomenclature, these must be under the care of a particular professor, they are in a certain sense the common property of all who have a right of access to the Museum. This is another reason for not removing them from their places (apart from the injury that might thereby accrue to them) without definite cause, as they should be always available for study, the professors and demonstrators rather bringing their classes to them than removing them to the class-rooms.

3. The collections kept for advanced researches. Although these are not exhibited in the ordinary sense of the word, they should, if retained at all, be kept in a situation and under conditions which make them readily accessible to all who can profit by their examination under suitable regulations. Their preservation is of the utmost importance in the progress of science, as among them are often to be found zoological "types," or the individual specimens upon which the name of the species was instituted, and which must be referred to by zoologists for all future time in cases of difficulty in determining that name. To permit the loss or deterioration of a "type" specimen is a serious offence in the eyes of the zoologist. The Hope Collection abounds in such types.

Nothing more need be said at present about the first and third of these sections of the Museum, but the second, the exhibited series occupying the body of the great hall, requires consideration in a little more detail.

It is divided at present into—

(1) Mineralogy. Of the value and arrangement of this section I am not competent to speak.

(2) Geology. This collection is mainly palæontological, and the arrangement appears to be partly stratigraphical and partly zoological. In many groups the collection is rich, but taking it altogether there appears to be a number of unnecessary duplicates, and much rearrangement is

required to bring it into good exhibition and teaching order. I would suggest that in a collection illustrating *geology* (and not the zoology of extinct animals, so often in museums confounded with that science) the stratigraphical arrangement should be followed as strictly as possible, and also that there should be a good series illustrating dynamical geology, or the processes by which the materials forming the earth's crust have been fashioned and arranged as we now see them.

(3) Animal Biology. This section occupies about two-thirds of the floor space of the Museum, and is at present broken up into various small series involving much repetition and duplication, and also difficulty of finding any particular object or illustration required.

In the middle of the hall is a series of specimens merely showing the external appearance of certain groups of animals, stuffed vertebrates and the shells of mollusks, and stony skeletons of corals, &c. If this collection were incorporated in the general series of animal biology, not only would much duplication be avoided, but a more instructive and scientific exhibition would be provided. Many of the present specimens of this series, especially the mounted mammals and birds, are in such bad condition that they have no educational value—they only mislead instead of teaching; but before destroying them they should all be submitted to the examination of some expert in the group to which they belong, as there may be interesting or rare specimens among them, though their value is scarcely to be recognized by the ordinary observer in their present condition.

The imperfection of any zoological series that does not illustrate extinct as well as recent forms is continually becoming more apparent as science advances; some attempts have already been made to remedy this defect in the zoological series, but a considerable transfer of specimens to it from the department of geology will result in advantage to both.

By a rearrangement of the biological series, with incorporation of the so-called zoological specimens (excluding the Hope Collection, which I presume is always to be kept apart) much economy of space could be effected, and some of the confusion which now appears to exist in this department of the Museum in consequence of the numerous apparently independent series of specimens will be obviated.

The great question of the primary arrangement of the biological collection, whether on the physiological or Hunterian system, or upon a system based upon zoological classification, will have to be carefully considered. Much is to be said for either, but whichever is adopted should follow the method of teaching of the professor and his assistants. The point to be aimed at is that every specimen should be readily found, and be in juxtaposition with other specimens which are related to it, and which should be studied in conjunction with it. As the classification of animals, except as regards the greater divisions, is still a matter of much uncertainty, and continually changing according to the advance of knowledge, or the opinions of individual zoologists, it is not a satisfactory basis for the arrangement of a collection intended to illustrate principles rather than details. On the other hand, the Hunterian system often brings into juxtaposition specimens related only by some remote analogy of function, and having no real correspondence or homology. Probably a zoological arrangement for the main divisions, and one based upon a comparison of organs or systems for the secondary divisions, will, on the whole, be found most convenient.

I am hardly in a position to say how far the Professor of Physiology requires a special collection to illustrate his teaching. Probably the general biological series will supply all that is necessary to refer to in illustration of his lectures, especially as the tendency of modern physiology seems to be to separate itself from morphology, and confine itself more to biological chemistry and dynamics.

Another question which has been raised is, whether human anatomy, as distinguished from general biology, requires a separate section of the Museum, and how the great and important collection of crania of the races of men, which under Prof. Rolleston became one of the special features of the Museum, should be treated and utilized for instruction. These are questions that time will probably solve. Much depends upon the view taken of the duties of the Lecturer on Human Anatomy, whether he should teach upon a broad and philosophical basis, or whether he should aim mainly at enabling his pupils to pass the standard now required by the examining bodies. But this trenches upon the larger and more complex subject of what should be the aim of the University in keeping up a Medical School.

The Pathological Collection will, of course, remain as at present under the care of the Professor of Medicine.

In looking round the Museum at the present time, one of its greatest wants appears to me to be proper labelling. The different sections of the Museum should be distinctly marked off from each other. Every case should have a conspicuous label on the top of it, indicating the nature of its contents. Every specimen should have one indicating why it is there and what it teaches. This will involve a large amount of labour and expense in printing, but it is absolutely necessary, if the collections are to fulfil the purpose for which they are formed. It is a mistake to spend much time, labour, and cost in obtaining, preparing, and preserving a specimen, and then to stop short of the one thing needed to make it of use. Better have fewer specimens in a complete state. A printing press might be established in the building and kept constantly at work, but as it would be difficult to apportion the claims upon its services of the different curators, it might be better to make an arrangement with the University Press by which labels (of a uniform character) for the whole Museum would be printed at a fixed charge, and paid for out of the funds of the department requiring them. As in a large number of cases only a single copy of a label is required, it is possible that some system of type-writing might be more economical, and nearly, if not quite, as effectual.

Of the importance of complete catalogues of every department of the Museum, it would seem almost superfluous to speak, were it not obvious that much is needed in this respect.

Lastly, it appears to me that, although more work-rooms and class-rooms may be necessary for the growing needs of the scientific departments of the University, there is ample space in the present building for some time to come for the exhibited portion of the Museum. The collections are rich, contain many instructive and valuable objects, and do great credit to the zeal and energy of those by whom they have been brought together. What is really required now is, not so much that they should be increased, as that they should be better arranged, better cared for, and that all inferior and defective specimens should be gradually replaced by better ones. Oxford has done very much in past times to initiate and keep up a high standard of museum work, but it must not be overlooked that great advances are being made in this respect, not only in this country but all over the Continent, and the standard is being continually raised. All such work is both laborious and costly, but when done the result is fully commensurate to the labour and expense bestowed upon it. An ill-arranged museum has been well compared to the letters of the alphabet tossed about indiscriminately, meaning nothing; a well-arranged one to the same letters placed in such orderly sequence as to produce words of counsel and instruction.

FURTHER RESEARCHES UPON THE ELEMENT FLUORINE.

SINCE the publication by M. Moissan of his celebrated paper in the *Annales de Chimie et de Physique* for December 1887, describing the manner in which he had succeeded in isolating this remarkable gaseous element, a considerable amount of additional information has been acquired concerning the chemical behaviour of fluorine, and important additions and improvements have been introduced in the apparatus employed for preparing and experimenting with the gas. M. Moissan now gathers together the results of these subsequent researches—some of which have been published by him from time to time as contributions to various French scientific journals, while others have not hitherto been made known—and publishes them in a long but most interesting paper in the October number of the *Annales de Chimie et de Physique*. Inasmuch as the experiments described are of so extraordinary a nature, owing to the intense chemical activity of fluorine, and are so important as filling a long existing vacancy in our chemical literature, readers of NATURE will doubtless be interested in a brief account of them.

IMPROVED APPARATUS FOR PREPARING FLUORINE.

In his paper of 1887, the main outlines of which were given in NATURE at the time (1887, vol. xxxvii. p. 179), M. Moissan showed that pure hydrofluoric acid readily dissolves the double fluoride of potassium and hydrogen, and that the liquid thus obtained is a good conductor of electricity, rendering electrolysis possible. It will be remembered that, by passing a strong current of electricity through this liquid contained in a platinum apparatus, free gaseous fluorine was obtained at the positive pole and hydrogen at the negative pole. The amount of hydrofluoric acid employed in these earlier experiments was about fifteen grams, about six grams of hydrogen potassium fluoride, $HF.KF$, being added in order to render it a conductor. Since the publication of that memoir a much larger apparatus has been constructed, in order to obtain the gas in greater quantity for the study of its reactions, and important additions have been made, by means of which the fluorine is delivered in a pure state, free from admixed vapour of the very volatile hydrofluoric acid. As much as a hundred cubic centimetres of hydrofluoric acid, together with twenty grams of the dissolved double fluoride, are submitted to electrolysis in this new apparatus, and upwards of four litres of pure fluorine is delivered by it per hour.

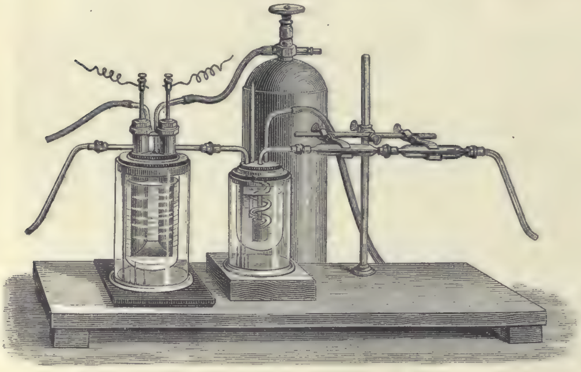

Fig. 1.

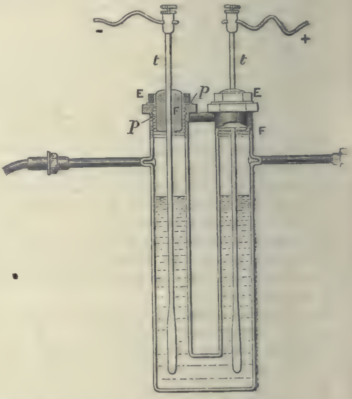

Fig. 2.

This improved form of the apparatus is shown in the accompanying figure (Fig. 1), which is reproduced from the memoir of M. Moissan. It consists essentially of two parts—the electrolysis apparatus and the purifying vessels. The electrolysis apparatus, a sectional view of which is given in Fig. 2, is similar in form to that described in the paper of 1887, but much larger. The U-tube of platinum has a capacity of 160 c.c. It is fitted with two lateral delivery tubes of platinum, as in the earlier form, and with stoppers of fluor-spar, F, inserted in cylinders of platinum, *p*, carrying screw threads, which engage with similar threads upon the interior surfaces of the limbs of the U-tube. A key of brass, E, serves to screw or unscrew the stoppers, and between the flange of each stopper and the top of each branch of the U-tube a ring of lead is compressed, by which means hermetic closing is effected. These fluor-spar stoppers, which are covered with a coating of gum-lac during the electrolysis, carry the electrode rods, *t*, which are thus perfectly insulated. M. Moissan now employs electrodes of pure platinum instead of irido-platinum, and the interior end of each is thickened into a club shape in order the longer to withstand corrosion. The apparatus is immersed during the electrolysis in a bath of liquid methyl chloride, maintained in tranquil ebullition at -23°. In order to preserve the methyl chloride as long as possible, the cylinder containing it is placed in an outer glass cylinder containing fragments of calcium chloride; by this means it is surrounded with a layer of dry air, a bad conductor of heat.

The purifying vessels are three in number. The first consists of a platinum spiral worm-tube, of about 40 c.c. capacity, immersed also in a bath of liquid methyl chloride, maintained at as low a temperature as possible, about -50°. As hydrofluoric acid boils at $19^{\circ}\cdot5$ (Moissan), almost the whole of the vapour of this substance which is carried away in the stream of issuing fluorine is condensed and retained at the bottom of the worm. To remove the last traces of hydrofluoric acid, advantage is taken of the fact that fused sodium fluoride combines with the free acid with great energy to form the double fluoride $HF.NaF$. Sodium fluoride also possesses the advantage of not attracting moisture. After traversing the worm condenser, therefore, the fluorine is caused to pass through two platinum tubes filled with fragments of fused sodium fluoride, from which it issues in an almost perfect state of purity. The junctions between the various parts of the apparatus are effected by means of screw joints, between the nuts and flanges of which collars of lead are compressed. During the electrolysis these leaden collars become, where exposed to the gaseous fluorine, rapidly converted into lead fluoride, which, being greater in bulk, causes the joints to become hermetically sealed. In order to effect the electrolysis, 26 to 28 Bunsen elements are employed, arranged in series. An ampere-meter and a commutator are introduced between the battery and the electrolysis apparatus; the former affording an excellent indication of the progress of the electrolysis.

As the U-tube contains far more hydrofluoric acid than can be used in one day, each lateral delivery-tube is fitted with a metallic screw stopper, so that the experiments may be discontinued at any time, and the apparatus closed. The whole electrolysis vessel is then placed under a glass bell-jar containing dry air, and kept in a refrigerator until again required for use. In this way it may be preserved full of acid for several weeks, ready at any time for the preparation of the gas. Considerable care requires to be exercised not to admit the vapour of methyl chloride into the U-tube, as otherwise violent detonations are liable to occur. When the liquid methyl chloride is being introduced into the cylinder, the whole apparatus becomes surrounded with an atmosphere of its vapour, and as the platinum U-tube is at the same instant suddenly cooled, the vapour is liable to enter by the abducting tubes. Consequently, as soon as the current is allowed to pass and fluorine is liberated within the U-tube, an explosion occurs. Fluorine instantly decomposes methyl chloride, with production of flame and formation of fluorides of hydrogen and carbon, liberation of chlorine, and occasionally deposition of carbon. In order to avoid this unpleasant occurrence, when the methyl chloride is being introduced the ends of the lateral delivery-tubes are attached to long lengths of caoutchouc tubing, supplied at their ends with calcium chloride drying tubes, so as to convey dry air from outside the atmosphere of methyl chloride vapour. If great care is taken to obtain the minimum temperature, this difficulty may be even more simply overcome by employing a mixture of well-pounded ice and salt instead of methyl chloride; but there is the counterbalancing disadvantage to be considered, that such a cooling bath requires much more frequent renewal.

CHEMICAL REACTIONS OCCURRING DURING THE ELECTROLYSIS.

In the paper of 1887, M. Moissan adopted the view that the first action of the electric current was to effect the decomposition of the potassium fluoride contained in solution in the hydrofluoric acid, fluorine being liberated at the positive pole, and potassium at the negative terminal. This liberated potassium would at once regenerate potassium fluoride in presence of hydrofluoric acid, and liberate its equivalent of hydrogen:

$$\mathrm{KF} = \mathrm{K} + \mathrm{F}.$$
$$\mathrm{K} + \mathrm{HF} = \mathrm{KF} + \mathrm{H}.$$

But when the progress of the electrolysis is carefully followed, by consulting the indications of the amperemeter placed in circuit, it is found to be by no means as regular as the preceding formulæ would indicate. With the new apparatus, the decomposition is quite irregular at first, and does not attain regularity until it has been proceeding for upwards of two hours. Upon stopping the current and unmounting the apparatus, the platinum rod upon which the fluorine was liberated is found to be largely corroded, and at the bottom of the U-tube a quantity of a black, finely-divided substance is observed. This black substance, which was taken at first to be metallic platinum, is a complex compound, containing one equivalent of potassium to one equivalent of platinum, together with a considerable proportion of fluorine. Moreover, the hydrofluoric acid is found to contain a small quantity of platinum fluoride in solution. The electrolytic reaction is probably therefore much more complicated than was at first considered to be the case. The mixture of acid and alkaline fluoride furnishes fluorine at the positive terminal rod, but this intensely active gas, in its nascent state, attacks the platinum and produces platinum tetrafluoride, $\mathrm{PtF_4}$; this probably unites with the potassium fluoride to form a double salt, possibly $\mathrm{2KF.PtF_4}$, analogous to the well-known platinochloride $\mathrm{2KCl.PtCl_4}$; and it is only when the liquid contains this double salt that the electrolysis proceeds in a regular manner, yielding free fluorine at the positive pole, and hydrogen and the complex black compound at the negative pole.

PHYSICAL PROPERTIES OF FLUORINE.

Fluorine possesses an odour which M. Moissan compares to a mixture of hypochlorous acid and nitrogen peroxide, but this odour is usually masked by that of the ozone which it always produces in moist air, owing to its decomposition of the water vapour. It produces most serious irritation of the bronchial tubes and mucous membrane of the nasal cavities, the effects of which are persistent for quite a fortnight.

When examined in a thickness of one metre, it is seen to possess a greenish-yellow colour, but paler, and containing more of yellow, than that of chlorine. In such a layer, fluorine does not present any absorption-bands. Its spectrum exhibits thirteen bright lines in the red, between wave-lengths 744 and 623. Their positions and relative intensities are as follows:—

λ = 744	very feeble.	λ = 685·5	feeble.
740	,,	683·5	,,
734	,,	677	strong.
714	feeble.	640·5	,,
704	,,	634	,,
691	,,	623	,,
687·5	,,		

At a temperature of − 95° at ordinary atmospheric pressure, fluorine remains gaseous, no sign of liquefaction having been observed.

METHODS OF EXPERIMENTING WITH FLUORINE.

When it is desired to determine the action of fluorine upon a solid substance, the following method of procedure is adopted. A preliminary experiment is first made, in order to obtain some idea as to the degree of energy of the reaction, by bringing a little of the solid, placed upon the lid of a platinum crucible held in a pair of tongs, near the mouth of the delivery-tube of the preparation apparatus. If a gaseous or liquid product results, and it is desirable to collect it for examination, small fragments of the solid are placed in a platinum tube connected to the delivery-tube by flexible platinum tubing or by a screw joint, and the resulting gas may be collected over water or mercury, or the liquid condensed in a cooled cylinder of platinum. In this manner the action of fluorine upon sulphur and iodine has been studied. If the solid, phosphorus for instance, attacks platinum, or the temperature of the reaction is sufficiently high to determine the combination of platinum and fluorine (towards 500°), a tube of fluor-spar is substituted for the platinum tube. The fluor-spar tubes employed by M. Moissan for the study of the action of phosphorus were about twelve to fourteen centimetres long, and were terminated by platinum ends furnished with flanges and screw threads in order to be able to connect them with the preparation apparatus. If it is required to heat the fluor-spar tubes, they are surrounded by a closely wound copper spiral, which may be heated by a Bunsen flame.

In experimenting upon liquids, great care is necessary, as the reaction frequently occurs with explosive violence. A preliminary experiment is therefore always made, by allowing the fluorine delivery-tube to dip just beneath the surface of the liquid contained in a small glass cylinder. When the liquid contains water, or when hydrofluoric acid is a product of the reaction, cylinders of platinum or of fluor-spar are employed. If it is required to collect and examine the product, the liquid is placed along the bottom of a horizontal tube of platinum or fluor-spar, as in case of solids, connected directly with the preparation apparatus, and the product is collected over water or mercury if a gas, or in a cooled platinum receiver if a liquid.

During the examination of liquids a means has accidentally been discovered by which a glass tube may be filled with fluorine gas. A few liquids, one of which is carbon tetrachloride, react only very slowly with fluorine at the ordinary temperature. By filling a glass tube with such a liquid, and inverting it over a platinum capsule also containing the liquid, it is possible to displace the liquid by fluorine, which, as the walls are wet, does not attack the glass. Or the glass tube may be filled with the liquid, and then the latter poured out, leaving the walls wet; the tube may then be filled with fluorine gas, which, being slightly heavier than air, remains in the tube for some time. In one experiment, in which a glass test-tube had been filled with fluorine over carbon tetrachloride, it was attempted to transfer it to a graduated tube over mercury, but in inclining the test-tube for this purpose, the mercury suddenly came in contact with the fluorine, and absorbed it so instantaneously and with such a violent detonation that both the test-tube and the graduated tube were shattered into fragments. Indeed, owing to the powerful affinity of mercury for fluorine, it is a most dangerous experiment to transfer a tube containing fluorine gas, filled according to either the first or second method, to the mercury trough; the tube is always shattered if the mercury comes in contact with the gas, and generally with a loud detonation. Fluorine may, however, be preserved for some time in tubes over mercury, provided a few drops of the non reacting liquid are kept above the mercury meniscus.

For studying the action of fluorine on gases, a special piece of apparatus, shown in Fig. 3, has been constructed.

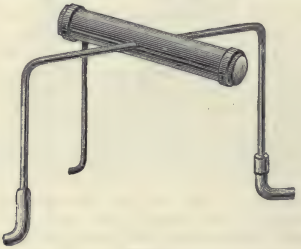

Fig. 3.

It is composed of a tube of platinum, fifteen centimetres long, closed by two plates of clear, transparent, and colourless fluor-spar, and carrying three lateral narrower tubes also of platinum. Two of these tubes face each other in the centre of the apparatus, and serve one for the conveyance of the fluorine and the other of the gas to be experimented upon. The third, which is of somewhat greater diameter than the other two, serves as exit-tube for the product or products of the reaction, and may be placed in connection with a trough containing either water or mercury. The apparatus is first filled with the gas to be experimented upon, then the fluorine is allowed to enter, and an observation of what occurs may be made through the fluor-spar windows. One most important precaution to take in collecting the gaseous products over mercury is not to permit the platinum delivery-tube to dip more than two or at most three millimetres under the mercury, as otherwise the levels of the liquid in the two limbs of the electrolysis U-tube become so different owing to the pressure, that the fluorine from one side mixes with the hydrogen evolved upon the other, and there is a violent explosion.

ACTION OF FLUORINE UPON THE NON-METALLIC ELEMENTS.

Hydrogen.—As just described, hydrogen combines with fluorine, even at − 23° and in the dark, with explosive force. This is the only case in which two elementary gases unite directly without the intervention of extraneous energy. If the end of the tube delivering fluorine is placed in an atmosphere of hydrogen, a very hot blue flame, bordered with red, at once appears at the mouth of the tube, and vapour of hydrofluoric acid is produced.

Oxygen.—Fluorine has not been found capable of uniting with oxygen up to a temperature of 500°. On ozone, however, it appears to exert some action, as will be evident from the following experiment. It was shown in 1887 that fluorine decomposes water, forming hydrofluoric acid, and liberating oxygen in the form of ozone. When a few drops of water are placed in the apparatus shown in Fig. 3, and fluorine allowed to enter, the water is instantly decomposed, and on looking through the fluor-spar ends a thick dark cloud is seen over the spot where each drop of water had previously been. This cloud soon diminishes in intensity, and is eventually replaced by a beautiful blue gas—ozone in a state of considerable density. If the product is chased out by a stream of nitrogen as soon as the dense cloud is formed, a very strong odour is perceived, different from that of either fluorine or ozone, but which soon gives place to the unmistakable odour of ozone. It appears as if there is at first produced an unstable oxide of fluorine, which rapidly decomposes into fluorine and ozone.

Nitrogen and *chlorine* appear not to react with fluorine.

Sulphur.—In contact with fluorine gas, sulphur rapidly melts and inflames. A gaseous fluoride of sulphur is formed, which possesses a most penetrating odour, somewhat resembling that of chloride of sulphur. The gas is incombustible, even in oxygen. When warmed in a glass vessel, the latter becomes etched, owing to the formation of silicon tetrafluoride, SiF_4. Selenium and tellurium behave similarly, but form crystalline solid fluorides.

Bromine vapour combines with fluorine in the cold with production of a very bright but low-temperature flame. If the fluorine is evolved in the midst of pure dry liquid bromine, the combination is immediate, and occurs without flame.

Iodine.—When fluorine is passed over a fragment of iodine contained in the horizontal tube, combination occurs, with production of a pale flame. A very heavy liquid, colourless when free from dissolved iodine, and fuming strongly in the air, condenses in the cooled receiver. This liquid fluoride of iodine attacks glass with great energy, and decomposes water when dropped into that liquid with a noise like that produced by red-hot iron. Its properties agree with those of the fluoride of iodine prepared by Gore by the action of iodine on silver fluoride.

Phosphorus.—Immediately phosphorus, either the ordinary yellow variety or red phosphorus, comes in contact with fluorine, a most lively action occurs, accompanied by vivid incandescence. If the fluorine is in excess, a fuming gas is evolved, which gives up its excess of fluorine on collecting over mercury, and is soluble in water. This gas is phosphorus pentafluoride, PF_5, prepared some years ago by Prof. Thorpe. If, on the contrary, the phosphorus is in excess, a gaseous mixture of this pentafluoride with a new fluoride, the trifluoride, PF_3, a gas insoluble in water, but which may be absorbed by caustic potash, is obtained. The trifluoride, in turn, combines with more fluorine to form the pentafluoride, the reaction being accompanied by the appearance of a flame of comparatively low temperature.

Arsenic combines with fluorine at the ordinary temperature with incandescence. If the current of fluorine is fairly rapid, a colourless fuming liquid condenses in the receiver, which is mainly arsenic trifluoride, AsF_3, but which appears also to contain a new fluoride, the pentafluoride, AsF_5, inasmuch as the solution in water yields the reactions of both arsenious and arsenic acids.

Carbon —Chlorine does not unite with carbon even at the high temperature of the electric arc, but fluorine reacts even at the ordinary temperature with finely-divided carbon. Purified lampblack inflames instantly with great brilliancy, as do also the lighter varieties of wood charcoal. A curious phenomenon is noticed with wood charcoal: it appears at first to absorb and condense the fluorine, then quite suddenly it bursts into flame with bright scintillations. The denser varities of charcoal require warming to 50° or 60° before they inflame, but if once the combustion is started at any point it rapidly propagates itself throughout the entire piece. Graphite must be heated to just below dull redness in order to effect combination; while the diamond has not yet been attacked by fluorine, even at the temperature of the Bunsen flame. A mixture of gaseous fluorides of carbon are produced whenever carbon of any variety is acted upon by fluorine, the predominating constituent being the tetrafluoride, CF_4.

Boron.—The amorphous variety of boron inflames instantly in fluorine, with projection of brilliant sparks and liberation of dense fumes of boron trifluoride, BF_3. The adamantine modification behaves similarly if powdered. When the experiment is performed in the fluor-spar tube, the gaseous fluoride may be collected over mercury. The gas fumes strongly in the air, and is instantly decomposed by water.

Silicon.—The reaction between fluorine and silicon is one of the most beautiful of all these extraordinary manifestations of chemical activity. The cold crystals become immediately white-hot, and the silicon burns with a very hot flame, scattering showers of star-like, white-hot particles in all directions. If the action is stopped before all the silicon is consumed, the residue is found to be fused. As crystalline silicon only melts at a temperature superior to 1200°, the heat evolved must be very great. If the reaction is performed in the fluor-spar tube, the resulting gaseous silicon tetrafluoride, SiF_4, may be collected over mercury.

Amorphous silicon likewise burns with great energy in fluorine.

ACTION OF FLUORINE UPON METALS.

Sodium and *potassium* combine with fluorine with great vigour at ordinary temperatures, becoming incandescent, and forming their respective fluorides, which may be obtained crystallized from water in cubes. Metallic *calcium* also burns in fluorine gas, forming the fused fluoride, and occasionally minute crystals of fluor-spar. *Thallium* is rapidly converted to fluoride at ordinary temperatures, the temperature rising until the metal melts and finally becomes red-hot. Powdered *magnesium* burns with great brilliancy. *Iron*, reduced by hydrogen, combines in the cold with immediate incandescence, and formation of an anhydrous, readily soluble, white fluoride. *Aluminium*, on heating to low redness, gives a very beautiful luminosity, as do also *chromium* and *manganese*. The combustion of slightly warmed zinc in fluorine is particularly pretty as an experiment, the flame being of a most dazzling whiteness. *Antimony* takes fire at the ordinary temperature, and forms a solid white fluoride. *Lead* and *mercury* are attacked in the cold, as previously described, the latter with great rapidity. *Copper* reacts at low redness, but in a strangely feeble manner, and the white fumes formed appear to combine with a further quantity of fluorine to form a perfluoride. The main product is a volatile white fluoride. *Silver* is only slowly attacked in the cold. When heated, however, to 100°, the metal commences to be covered with a yellow coat of anhydrous fluoride, and on heating to low redness combination occurs, with incandescence, and the resulting fluoride becomes fused, and afterwards presents a satin-like aspect. *Gold* becomes converted into a yellow deliquescent volatile fluoride when heated to low redness, and at a slightly higher temperature the fluoride is dissociated into metallic gold and fluorine gas.

The action of fluorine on *platinum* has been studied with special care. It is evident, in view of the corrosion of the positive platinum terminal of the electrolysis apparatus, that nascent fluorine rapidly attacks platinum at a temperature of − 23°. At 100°, however, fluorine gas appears to be without action on platinum. At 500°–600° it is attacked strongly, with formation of the tetrafluoride, PtF_4, and a small quantity of the protofluoride, PtF_2. If the fluorine is admixed with vapour of hydrofluoric acid, the reaction is much more vigorous, as if a fluorhydrate of the tetrafluoride, perhaps $2HF.PtF_4$, were formed. The tetrafluoride is generally found in the form of deep-red fused masses, or small yellow crystals resembling those of anhydrous platinum chloride. The salt is volatile and very hygroscopic. Its behaviour with water is peculiar. With a small quantity of water a brownish-yellow solution is formed, which, however, in a very short time becomes warm and the fluoride decomposes; platinic hydrate is precipitated, and free hydrofluoric acid remains in solution. If the quantity of water is greater, the solution may be preserved for some minutes without decomposition. If the liquid is boiled, it decomposes instantly. At a red heat platinic fluoride decomposes into metallic platinum and fluorine, which is evolved in the free state. This reaction can therefore be employed as a ready means of preparing fluorine, the fluoride only requiring to be heated rapidly to redness in a platinum tube closed at one end, when crystallized silicon held at the open end will be found to immediately take fire in the escaping fluorine. The best mode of obtaining the fluoride of platinum for this purpose is to heat a bundle of platinum wires to low redness in the fluor-spar reaction tube in a rapid stream of fluorine. As soon as sufficient fluoride is formed on the wires, they are transferred to a well-stoppered dry glass tube, until required for the preparation of fluorine.

ACTION OF FLUORINE UPON NON-METALLIC COMPOUNDS.

Sulphuretted hydrogen.—When the horizontal tube shown in Fig. 3 is filled with sulphuretted hydrogen gas and fluorine is allowed to enter, a blue flame is observed on looking through the fluor-spar windows playing around the spot where the fluorine is being admitted. The decomposition continues until the whole of the hydrogen sulphide is converted into gaseous fluorides of hydrogen and sulphur.

Sulphur dioxide is likewise decomposed in the cold, with production of a yellow flame and formation of fluoride of sulphur.

Hydrochloric acid gas is also decomposed at ordinary temperatures with flame, and, if there is not a large excess of hydrochloric acid present, with detonation. Hydrofluoric acid and free chlorine are the products.

Gaseous *hydrobromic* and *hydriodic acids* react with fluorine in a similar manner, with production of flame and formation of hydrofluoric acid. Inasmuch, however, as bromine and iodine combine with fluorine, as previously described, these halogens do not escape, but burn up to their respective fluorides. When fluorine is delivered into an aqueous solution of hydriodic acid, each bubble as it enters produces a flash of flame, and if the fluorine is being evolved fairly rapidly there is a series of very violent detonations. A curious reaction also occurs when fluorine is similarly passed into a 50 per cent. aqueous solution of hydrofluoric acid itself, a flame being produced in the middle of the liquid, accompanied by a series of detonations.

Nitric acid vapour reacts with great violence with fluorine, a loud explosion resulting. If fluorine is passed into the ordinary liquid acid, each bubble as it enters produces a flame in the liquid.

Ammonia gas is decomposed by fluorine with formation of a yellow flame, forming hydrofluoric acid and liberating nitrogen. With a solution of the gas in water, each bubble of fluorine produces an explosion and flame, as in case of hydriodic acid.

Phosphoric anhydride, when heated to low redness, burns with a pale flame in fluorine, forming a gaseous mixture of fluorides and oxyfluoride of phosphorus. *Pentachloride and trichloride of phosphorus* both react most energetically with fluorine, instantly producing a brilliant flame, and evolving a mixture of phosphorus pentafluoride and free chlorine.

Arsenious anhydride also affords a brilliant combustion, forming the liquid trifluoride of arsenic, AsF_3. This liquid in turn appears to react with more fluorine with considerable evolution of heat, probably forming the pentafluoride, AsF_5. *Chloride of arsenic*, $AsCl_3$, is converted with considerable energy to the trifluoride, free chlorine being liberated.

Carbon bisulphide inflames in the cold in contact with fluorine, and if the fluorine is led into the midst of the liquid a similar production of flame occurs under the surface of the liquid, as in case of nitric acid. No carbon is deposited, both the carbon and sulphur being entirely converted into gaseous fluorides.

Carbon tetrachloride, as previously mentioned, reacts only very slowly with fluorine. The liquid may be saturated with gaseous fluorine at 15°, but on boiling this liquid a gaseous mixture is evolved, one constituent of which is carbon tetrafluoride, CF_4, a gas readily capable of absorption by alcoholic potash. The remainder consists of another fluoride of carbon, incapable of absorption by potash, and chlorine. A mixture of the vapours of carbon tetrachloride and fluorine inflames spontaneously with detonation, and chlorine is liberated without deposition of carbon.

Boric anhydride is raised to a most vivid incandescence by fluorine, the experiment being rendered very beautiful by the abundant white fumes of the trifluoride which are liberated.

Silicon dioxide, one of the most inert of substances at the ordinary temperature, takes fire in the cold in contact with fluorine, becoming instantly white-hot, and rapidly disappearing in the form of silicon tetrafluoride. The *chlorides* of both *boron* and *silicon* are decomposed by fluorine, with formation of fluorides and liberation of chlorine, the reaction being accompanied by the production of flame.

ACTION OF FLUORINE UPON METALLIC COMPOUNDS.

Chlorides of the metals are instantly decomposed by fluorine, generally at the ordinary temperature, and in certain cases, antimony trichloride for instance, with the appearance of flame. Chlorine is in each case liberated, and a fluoride of the metal formed. A few require heating, when a similar decomposition occurs, often accompanied by incandescence, as in case of chromium sesquichloride.

Bromides and *iodides* are decomposed with even greater energy, and the liberated bromine and iodine burn in the fluorine with formation of their respective fluorides.

Cyanides react in a most beautiful manner with fluorine, the displaced cyanogen burning with a purple flame. Potassium ferrocyanide in particular affords a very pretty experiment, and reacts in the cold. Ordinary potassium cyanide requires slightly warming in order to start the combustion.

Fused *potash* yields potassium fluoride and ozone. Aqueous potash does not form potassium hypofluorite when fluorine is bubbled into it, but only potassium fluoride. *Lime* becomes most brilliantly incandescent, owing partly to the excess being raised to a very high temperature by the heat developed during the decomposition, and partly to the phosphorescence of the calcium fluoride formed.

Sulphides of the alkalies and alkaline earths are also immediately rendered incandescent, fluorides of the metal and sulphur being respectively formed.

Boron nitride behaves in an exceedingly beautiful manner, being attacked in the cold, and emitting a brilliant blue light which is surrounded by a halo of the fumes of boron fluoride.

Sulphates, *nitrates*, and *phosphates* generally require the application of more or less heat, when they too are rapidly and energetically decomposed. Calcium phosphate is attacked in the cold like lime, giving out a brilliant white light, and producing calcium fluoride and gaseous oxyfluoride of phosphorus, POF_3. *Calcium carbonate* also becomes raised to brilliant incandescence when exposed to fluorine gas, as does also normal *sodium carbonate;* but curiously enough the bicarbonates of the alkalies do not react with fluorine even at red heat. Perhaps this may be explained by the fact that fluorine has no action at available temperatures upon carbon dioxide.

ACTION OF FLUORINE UPON A FEW ORGANIC COMPOUNDS.

Chloroform.—When chloroform is saturated with fluorine, and subsequently boiled carbon tetrafluoride, hydrofluoric acid and chlorine are evolved. If a drop of chloroform is agitated in a glass tube with excess of fluorine, a violent explosion suddenly occurs, accompanied by a flash of flame, and the tube is shattered to pieces. The reaction is very lively when fluorine is evolved in the midst of a quantity of chloroform, a persistent flame burns beneath the surface of the liquid, carbon is deposited, and fluorides of hydrogen and carbon are evolved together with chlorine.

Methyl chloride is decomposed by fluorine, even at −23°, with production of a yellow flame, deposition of carbon, and liberation of fluorides of hydrogen and carbon and free chlorine. With the vapour of methyl chloride, as pointed out in the description of the electrolysis, violent explosions occur.

Ethyl alcohol vapour at once takes fire in fluorine gas, and the liquid is decomposed with explosive violence without deposition of carbon. Aldehyde is formed to a considerable extent during the reaction.

Acetic acid and *benzene* are both decomposed with violence, their cold vapours burn in fluorine, and when the latter is bubbled through the liquids themselves, flashes of flame, and often most dangerous explosions, occur. In the case of benzene, carbon is deposited, and with both liquids fluorides of hydrogen and carbon are evolved. *Aniline* likewise takes fire in fluorine, and deposits a large quantity of carbon, which, however, if the fluorine is in excess, burns away completely to carbon tetrafluoride.

Such are the main outlines of these later researches of M. Moissan, and they cannot fail to impress those who read them with the prodigious nature of the forces associated with those minutest of entities, the chemical atoms, as exhibited at their maximum, in so far as our knowledge at present goes, in the case of the element fluorine.

A. E. Tutton.

THE HUXLEY LABORATORY FOR BIOLOGICAL RESEARCH, AND THE MARSHALL SCHOLARSHIP.

SCIENTIFIC friends and former pupils of Prof. Huxley will alike be gratified to learn that an appropriate method has been devised for establishing a permanent memorial of his great services to the institution with which his name has been so long identified. The late Sir Warington Smyth, whose loss we had to deplore rather more than a year ago, was the last surviving member of the original staff of the School of Mines, as founded by Sir Henry de la Beche in 1851. Prof. Huxley, who, as long ago as 1854, succeeded Edward Forbes in the Chair of Natural History, continues to hold the post of Honorary Dean of the Royal College of Science, with which the School of Mines is now incorporated; and although, since 1885, compelled by ill-health to discontinue the work of lecturing, he is still, we are happy to say, able to take a kindly interest in, and to exercise a general supervision over, the biological studies carried on in the school.

How much the Central Institution for training teachers in science, which is now located at South Kensington, owes to the organizing faculty and unremitting labours of Prof. Huxley, only those who have been associated with him in the work can form any just estimate. During the first twenty years of its existence all attempts at practical teaching in the School of Mines were restricted to the subjects of chemistry and metallurgy, the space available in the Jermyn Street buildings only permitting of the existence of very small and inconvenient laboratories in connection with those two branches of science.

Soon after the first establishment of the school, larger and more convenient premises for carrying on the chemical instruction had to be obtained in Oxford Street; and in 1872, on the unanimous recommendation of the Council, the teaching of chemistry, physics, and biology, was transferred to the building at South Kensington, which had been originally designed as a School of Naval Architecture. At subsequent dates, as the inadequacy of the Jermyn Street buildings to accommodate both the school and the Geological Survey made itself more strongly felt, the divisions of geology, mineralogy, metallurgy, applied mechanics, and mining, were successively removed to the same place.

No sooner did Prof. Huxley find an opportunity afforded to him, than he energetically devoted himself to the realization of a long-cherished scheme for establishing a system of practical laboratory-instruction in biology, including both its zoological and its botanical aspects. The ground was broken by a short vacation course, in which an attempt was made to supply such practical instruction to persons engaged in teaching; this course was given in the summer of 1871, and in the following year the same system of laboratory-instruction in biology was introduced into the ordinary School of Mines curriculum. In establishing at South Kensington the biological laboratory which has become the model of so many similar institutions at home and abroad, Prof. Huxley sought and obtained the advice and co-operation of many of his fellow-workers in science, among whom may be specially mentioned Profs. Michael Foster, Thiselton Dyer, Ray Lankester, and Rutherford, with Dr. Martin and Dr. Vines. In carrying on and further developing the work, he has had the assistance of Profs. Jeffrey Parker and F. O. Bower, in the zoological and botanical departments respectively, and, in succession to them, of Mr. G. B. Howes and Dr. D. H. Scott.

From the period of the first foundation of the School of Mines, the importance had been kept in mind of combining original research with the work of teaching. No one at the present day needs to be reminded of the numerous important investigations which have been prosecuted by Prof. Huxley, both at Jermyn Street and South Kensington. Memoirs of the highest value on various branches of comparative anatomy and palæontology have been interspersed with notable contributions to geology, to anthropology, and to botany; and from time to time excursions have been made still farther afield (predatory excursions they were regarded by some), into realms of thought more remote from the ordinary domain of the zoologist. But in all these varied avocations the interests of the teaching work were never forgotten; and it was made evident that the teacher, while carrying on investigations himself, was ever ready to suggest, stimulate, and supervise the investigations of others.

When, in 1885, ill-health compelled Prof. Huxley to relinquish his daily occupations in the school, it was found that, during the more than thirty years' occupancy of his post, he had accumulated a most valuable library of research, composed of treatises and journals dealing with every branch of biological science. This library he generously determined to present to the institution, the interests of which he had so long and earnestly laboured to promote. The Council of the School, in accepting this valuable gift, recommended that the room where these books were kept, and in which Prof. Huxley had so long carried on his work, should be entirely set apart for biological research; and the proposal at once met with the sanction of the Lords of the Committee of Council on Education.

The Huxley Laboratory for Biological Research is now arranged to accommodate two students, who will undertake investigations in connection with some branch of zoology, botany, or palæontology, the work being carried on under the supervision of the professors and assistant professors of the school. With a valuable library and all necessary appliances for work supplied to them, it may be hoped that the *genius loci* will not be without its influence upon these research students, and that a long series of important observations may be made, which will constitute an enduring and a worthy memorial of Prof. Huxley's connexion with the school.

It happens, very opportunely, that something in the way of a small endowment has already been provided to aid this scheme of biological research. As long ago as 1882, Miss Sarah Marshall, of Warwick Gardens, Kensington, wrote to Prof. Huxley, informing him of her intention to bequeath the sum of £1000, and her scientific books and instruments, to the Department of Science and Art, with a view to the establishment of a prize or scholarship in biology, in memory of her father, the late Mr. Marshall of the Bank of England. By the recent death of Miss Marshall, this bequest has now passed into the hands of the Lords of the Committee of Council on Education, and, by the advice of the Council of the Royal College of Science, it has been decided that the interest of the legacy shall be annually paid as a scholarship to a meritorious student, to aid him in carrying on some biological investigation in the Huxley Laboratory. We can only hope that this modest attempt at the endowment of research may be attended with success; and that this success may be so conspicuous as to encourage others to imitate the example of Miss Marshall, so that bequests of a similar character may be made in connexion with this and other institutions where scientific researches can be carried on.

ON VAN DER WAALS'S TREATMENT OF LAPLACE'S PRESSURE IN THE VIRIAL EQUATION: IN ANSWER TO LORD RAYLEIGH.

MY DEAR LORD RAYLEIGH,—From the heading of your first letter, and from the wide scope of the passage you quoted from my paper, I imagined that you intended to raise the whole question of Van der Waals's treatment of Laplace's pressure. Otherwise I should not, in my answer, have referred to his b or to the unfortunate results of comparing his formula with experiment. I should, in fact, have contented myself with the acknowledgment that you had given an accurate account of the contents of a portion of Van der Waals's earlier chapters, which I had carelessly missed on the first hasty perusal; and that these contents justified the expression $3Kv/2$ as the virial of Laplace's pressure. But to this I should certainly have added that, even had I been fully cognizant of that portion of the pamphlet when I wrote my paper, I should probably not have modified (at least to any serious extent) the passage you quoted.

For (1) that passage contains the distinct statement that, from the statical point of view, reasons "satisfactory on the whole" were given by Van der Waals for regarding Laplace's pressure as proportional to the square of the density. And it would have been illogical on my part to object, except on the ground of insufficient generality, to the equation

$$\left(p + \frac{a}{v^2}\right)v = \tfrac{1}{3}\Sigma(mu^2),$$

though I might have regarded the mode of its establishment as obscure or even doubtful.

In fact, the equation which is one of the main features of my own paper, viz. :—

$$pv + \frac{A}{v+a} = \tfrac{1}{3}\Sigma(mu^2) \,.\, \left(1 + \frac{e}{v+a}\right),$$

includes it as the particular case when

$$e = 0, \quad a = 0.$$

What I objected to was a totally different thing :—viz. the above equation manipulated by the introduction of the factor $(v - b)/v$ in the left-hand member.

Again (2) the equation

$$p(v - \beta) = \tfrac{1}{3}\Sigma(mu^2)$$

is obtained in my paper (§ 64), and is there spoken of as "perfectly *legitimate*," but *only* on the distinct condition that

$$\beta\Sigma(mu^2)/3v,$$

where β is four times the sum of the volumes of the particles (§ 30), "*be small in comparison with the other terms in the* [virial] *equation.*" As one of these terms is the quantity $\Sigma(mu^2)/3$ itself, this implies that for the truth of the equation β/v must be a small fraction; and it is most certainly not so at the critical point of carbonic acid, which furnished the first and one of the most important cases for the application of the virial method. In fact the equation above, when correctly obtained, comes originally in the form (in which it ought to be preserved)

$$pv = \tfrac{1}{3}\Sigma(mu^2) \,.\, \left(1 + \frac{\beta}{v}\right);$$

again a particular case of my own equation, viz. when

$$A = 0, \quad a = 0, \quad e = \beta.$$

Here the factor $1/v$ is (roughly) proportional to the number of collisions per particle per second, and it is in that capacity that it appears in the equation. As I said in my former letter, it is impossible (at least with Van der Waals's mode of interpreting $\Sigma(mu^2)$) to derive from this a cubic in v; even when the term a/v^2 is introduced as a simple addition to p :—unless, for the express purpose of obtaining the indispensable cubic, we write $v/(v - \beta)$ in place of $(v + \beta)/v$, on the right-hand side; which is, practically, what Van der Waals does. The true mode of getting a cubic here, if we keep to Van der Waals's interpretation of $\Sigma(mu^2)$, is to write $\beta/(v - \gamma)$ instead of β/v. This can, to a certain extent at least, be justified; the other method can not.

On the question of the introduction by Van der Waals of the factor $(v - b)/v$, whether or not it is applied alike to p and to K, I regret to find that our views must continue to differ. For it appears to me that when once the various terms of the virial equation have been correctly extracted from the expression $\Sigma(Rr)$, we have no right to modify any of them. There seems therefore to be no doubt whatever that the procedure in Van der Waals's sixth chapter is entirely wrong in principle :—except in so far as (in the German version) he borrows some correct expressions from Lorentz. The meanings of v and of p, in the term pv of the virial equation, are (from the very beginning of the inquiry) definitely assigned as total volume and external pressure :—so that this term cannot in any way be altered. No more can the term $\Sigma(mu^2)/3$, or the ratio of these two terms. Van der Waals's argument seems (for his pamphlet is everywhere somewhat obscure) to be that (when there is no molecular force) *in consequence of the finite diameters of the particles the pressure, for a given amount of kinetic energy, will be greater than if these were mere points.* Perfectly true :—but we must seek the expression for this increase of pressure in the remaining parts of the term $\Sigma(Rr)$, and *not* artificially introduce it by diminishing the multiplier of p in a term already definitely extracted. And further, if this procedure of Van der Waals were allowed to pass without protest in so far as the term pv is concerned, I think that we should logically be forced to treat the term Kv (not to the same but) to a very different factor :—for *here* the consideration of the finite volumes of the particles would appear to call for a *reduced* rather than an increased value of K; and therefore analogy would require a multiplication of the term Kv by some such expression as $(v + \gamma)/v$, where γ is essentially *positive.*—Yours very truly,

P. G. Tait.

Edinburgh, 17/10/91.

NOTES.

To-day the Senate of Cambridge University will decide whether official inquiry shall be made as to the expediency of allowing alternatives for one of the two classical languages in the Previous Examination, either to all students or to any classes of students other than those already exempted. Everyone who devotes attention to questions connected with the higher education recognizes the importance of the issue, and the discussion of the subject has been followed with wide-spread interest.

The ordinary general meeting of the Institution of Mechanical Engineers began yesterday evening, and will be continued this evening, at 25 Great George Street, Westminster. The papers to be read and discussed, as we have already stated, are by Mr. Samuel Boswell and Prof. W. C. Roberts-Austen, F.R.S.

The Geologists' Association will hold a *conversazione* at University College, Gower Street, on Friday evening, November 6. Members are invited to send exhibits, and to let the secretary know the nature of the object or objects they propose to show.

At the meeting of the Royal Horticultural Society in the Drill Hall, Westminster, on Tuesday, there was an interesting display of autumn foliage arranged for æsthetic effect. A lecture was delivered by Mr. H. J. Veitch, who urged that trees and shrubs in gardens and plantations should be selected, not only with a view to their summer beauty, but also with regard to their autumn hues; and he had many suggestions to offer as to the various ways in which these hues may be most effectively contrasted.

Prof. Boys has arranged his apparatus for the repetition of the Cavendish experiment in the basement of the Clarendon Laboratory, Oxford. The experiment will be proceeded with immediately.

We regret to have to record the death of Dr. Philip Herbert Carpenter, F.R.S., the fourth son of the late Dr. W. B. Carpenter, C.B., F.R.S. He was found dead in his dressing-room at Eton College, on Wednesday, October 21. At the inquest it was found that he had killed himself by the administration of chloroform during temporary insanity. Dr. Carpenter was in his fortieth year, and had been a science master in Eton since 1877. The following summary of his scientific work in given by the *Times*. He was a member of the scientific staff of the deep-sea exploring expeditions of Her Majesty's steamships *Lightning* (1868) and *Porcupine* (1869-70); and in 1875 he was appointed assistant naturalist to Her Majesty's ship *Valorous*, which accompanied Sir G. Nares's Arctic expedition to Disco Island, and spent the summer sounding and dredging in Davis Strait and the North Atlantic. Dr. Carpenter devoted himself continuously from 1875 to studying the morphology of the Echinoderms, more particularly of the Crinoids, both recent and fossil. In 1883 he was awarded the Lyell Fund by the Geological Society of London in recognition of the value of his work, and in 1885 was elected a Fellow of the Royal Society. His chief memoirs and papers were as follows:—"Notes on Echinoderm Morphology," i.-xi., 1878-87; "On the Genus Actinometra," 1877; "Report upon the Crinoidea dredged by H.M.S. *Challenger*," Part I. "The Stalked Crinoids," 1885, Part II. "The Comatulæ," 1888; "Report upon the Comatulæ dredged by the U.S. Coast Survey in the Caribbean Sea," 1890. In conjunction with Mr. R. Etheridge, Jun., he prepared the "Catalogue of the Blastoidea in the Geological Department of the British Museum," 1886; and he also wrote numerous papers published in the Proceedings or Transactions of the Royal, Linnean, and Geological Societies.

Mr. George Sibley, who was for many years well known as an engineer in India, and had also a considerable reputation as a traveller, died at his residence at Catherham on Sunday last at the age of ixty-seven. It is understood that Mr. Sibley has left a legacy for the purpose of founding engineering scholarships in the University of Calcutta.

Dr. J. Eduard Polak, who died at Vienna on October 8, at the age of seventy-one, was one of the most eminent Persian cholars of his time. He went in 1851 to Teheran, where he lectured at the medical school, and became physician to the Shah. During his nine years' residence in Persia he visited most parts of the country; and on his return to Vienna he wrote his well-known work, "Persien: das Land und seine Bewohner," in which he presented an excellent summary of the knowledge he had acquired. In response to an invitation from the Shah, he again visited Teheran. He read before the Geographical and Anthropological Societies of Vienna many valuable papers on Persia and its antiquities.

The International Geological excursion in America, which started on September 2 last, ended on October 9 after a most successful and interesting trip. In all there were ninety geologists, and the arrangements as regards trains, &c., left nothing to be desired. The route chosen lay through the petroleum districts of Pennsylvania, the prairies of Wisconsin, Minnesota, and Dakotah, the corn-lands of North America, and the twin centres St. Paul and Minneapolis. From the Yellowstone River the party journeyed to the beautiful geyser region of the National Park, where they made a stay of seven days, then to the rising mountain district of Butte, as well as to the Mormon town situated in the middle of the salt wastes of the Great Salt Lake. They then skirted the table-lands in South Utah, and turned towards the Rocky Mountains, where they visited the chief places of geological interest, including Pike's Peak, the Garden of the Gods, &c. At this point many of the party returned home, going by way of Chicago, Niagara Falls, and New York. The smaller number that remained undertook a laborious and exhausting expedition through the deserts of New Mexico and Arizona to the San Francisco mountains and to the Grand Cañon of Colorado; they visited a group of 165 volcanoes and craters, and also a deep valley the sides of which, with their many and various-coloured stones, fall 5800 to 6000 feet to the great Colorado River below. From this standpoint they had an excellent view of the materials composing the upper surfaces of the earth's crust, and they could not but be struck by the magnitude and grandeur of the work accomplished by Nature in digging out this enormous river cañon. The following are some of the places visited on the return journey: La Junta, Kansas City, Chicago, Niagara Falls, Albany, and Boston. Altogether the excursion was a thorough success, and the Americans deserve much credit for having arranged so good a programme for their visitors.

Prof. Russell and his party have returned from the Alaskan wilds, which they penetrated to a distance of forty miles inland, from Icy Bay to the base of Mount St. Elias. They constructed a camp, and remained there two months, making geological surveys and taking observations. Prof. Russell says:—"We began the ascent of Mount St. Elias on June 3. Our progress was not obstructed until we reached an altitude of nearly 10,000 feet. Then we found glaciers. After many perilous adventures we attained the height of 14,500 feet. This has been the estimated height of the mountain, but we found it nearly 5000 feet higher. It was impossible for us to proceed any further, as we were suffering too much from the hardships already endured. Many of the men were exhausted and very weak. The Alaskan Indians were most hospitable to us."

The report by Mr. James Dredge and Sir Henry Trueman Wood on their recent visit to Chicago is printed in the Journal of the Society of Arts (October 23). This report was presented last week to the Royal Commission which has been appointed to organize the English Section at the Chicago "World's Fair." The Commission have decided to appoint the following Committees: Finance, Fine Arts, Indian, Colonial, Engineering, General Manufactures, Electricity, Agriculture, Mines and Metallurgy, Textile Industries, Science and Education, Transportation; also a Committee of Ladies to correspond with the Ladies' Committee at Chicago. They propose to invite the assistance of Chambers of Commerce as Local Committees. A prospectus relating to the Chicago Exhibition has been issued by the Royal Commission

The Council of the Institution of Civil Engineers have issued for general circulation their regulations as to the admission of students. This is followed by an excellent account of the various educational institutions in the British dominions where instruction is given bearing on the profession of civil engineers.

In his report on the working of the Central Museum, Madras, during 1890-91, Mr. Edgar Thurston, the Superintendent, notes that he made two official tours in company with his taxidermists. During the first of these, as in several previous years, he stayed on Rámesvaram Island, where he was mainly engaged in the collection and preservation of marine worms and molluscan shells, which have since been sent to England and Germany to be worked up. Many specimens of the brightly coloured "coral-fishes," which abound over the fringing coral-reefs, were also preserved by the glycerine process introduced by Mr. A. Haly, of the Colombo Museum, for the preservation of colours. His stay on Rámesvaram Island completed, he paid a short visit to Tuticorin, to work out some doubtful points in connection with the anatomy of the pearl oyster. In his second tour he made large collections illustrative of the arts, industries, manufactures, and natural history of the places visited in the Bangalore, Hassan, Shimoga, and Mysore districts. These collections include Srávanbelgola brass-ware, Soráb and Sagar sandal-wood carving, Channapatna silk and toys, Mysore inlaid ware, gold jewellery from Bélur, butterflies, lizards, snakes, &c. A report on this tour will be published after a further visit to the Mysore province, a large area of which remains to be explored.

The other day, Mr. Flinders Petrie delivered at the Owens College, Manchester, a most interesting address on exploration in Egypt. It had been thought, he said, that the immense mounds of rubbish indicating the sites of towns had been made on purpose, but they resulted from the natural decay of the mud-brick buildings. These heaps of ruined walls and earth and potsherds rose even to eighty feet high in some places; but other ancient sites were much less imposing, and might even not attract notice on the open desert. The higher the mound the longer the place had been inhabited; and if the surface was of a late period, the earlier parts, which were most needed, were under such a depth of rubbish as to be practically inaccessible. Much could be known at first sight; and prospecting had now become as scientific a matter in antiquities as in geology. Knowing, by a glance at the sherds on the top, what was the latest period of occupation of the site, and knowing the usual rate of accumulation of a mud-brick town—about five feet in a century—we could guess how far back the bottom of the mound must be dated. Other remains had different indications. If in the midst of a great mound there was a wide flat crater, that was probably the temple site, surrounded by houses which had accumulated high on all sides of it. Speaking of the results of exploration, Mr. Petrie said that we now realized what the course of the arts had been in Egypt. In the earliest days yet known to us—about 4000 B.C.—we found great skill in executing accurate and massive stonework, such skill as had hardly ever been exceeded. We found elaborate tools used, jewelled saws and tubular drills. We saw the pictorial arts as fully developed as they were for thousands of years later. But what led up to this we were still feeling for.

To what uses did primitive men apply the stone hammers which they made in such large numbers? This question Mr. J. D. McGuire tries to answer in a paper in the *American Anthropologist* for October. His theory is that the hammer was probably "the tool upon which races living in the Stone Age relied more than upon any other object to fashion stone implements." It was used, he thinks, not only to peck an axe or celt into shape, but to rub or polish the implement after it had been shaped; and, to illustrate this, he gives a figure representing a typical hammer of quartzite, from McMinn County, Tennessee, the periphery of which is pitted by use, while the flattened sides show that it must have been a rubbing-stone as well. To prove that the work suggested could be done by a stone hammer, he represents an axe of close-grained black porphyry, which he himself pecked out and grooved by means of such an implement. The task occupied him about five hours. As ordinary stone axes are made of softer material, he thinks they were probably produced in a much shorter time.

Dr. H. von Wlislocki contributes to the current number of *Globus* a capital paper on the handicrafts of Hungarian gypsies, whom he has had many opportunities of observing. If we may judge from the illustrations, they have a considerable aptitude for design. In the summer they make bottles out of pumpkins, which they decorate with various drawings. On each bottle the space is divided into four zones, crosses being cut into the uppermost zone, serpents into the second one, circles into the third, and zigzag lines into the fourth. The crosses mean "May you be happy!"; the serpents, "May you have no enemies!"; the circles, "May you always have money!"; the zigzag lines, "May you be healthy!" Brandy is kept in the bottles; and when a guest is received, the first gypsy who drinks says, "May you be happy!"; the second, "May you have no enemies!"—and so on. Pretty walking-sticks are also among the things made by the Hungarian gypsies. On the top of one of those sketched in the article two female heads are admirably carved. These represent Ana, the Queen of the Keschalyis, or forest fairies, who dwell among the mountains, where they sit—three being always together—on rocks, spreading out their long hair over the valleys, thus giving rise to mists. Queen Ana lives in a black palace, and sometimes wanders over the world in the form of a frog. Frogs, toads, and serpents are her favourite animals. When she meets anyone in her natural form, she exclaims "Ana!", which means "Bring!" Should the person understand the cry and bring a frog, a toad, or a serpent, he is richly rewarded. If he fails to do so, he is either killed with a piece of a rock, or struck by some terrible malady.

The *Times* of October 22 has an interesting article on "Our Position with regard to Rainfall," compiled from the statistics published by Mr. Symons and the Meteorological Office. The rainfall during the present month has been so heavy that in many places the amount up to the morning of the 18th was in excess of the average for the whole month. In London this excess amounted only to 0·3 inch, while at Valentia Island and at Stornoway it amounted to nearly 2 and 3 inches respectively, and the amount which fell during the next few days has greatly increased the excess. But for the 10 years ending with 1889 the rainfall over the United Kingdom differed only by 1 per cent. from the average of the last 50 years. The values for the present year, up to the 18th instant (as shown by the last Weekly Weather Report then published), were rather in excess of the average over the southern, midland, and western parts of England, and the north of Scotland, while in the remaining districts there was still a deficiency. For the whole period since the end of 1889, there was only one district, viz. Scotland (N.), in which the total fall was in excess of the average. In Scotland and the midland and south-western counties of England, the deficiency was still very large. The question is asked—Are we likely to have in the years immediately advancing more or less rain than during the last few years? While the question cannot be answered with absolute confidence, the grouping of years into decades or other regular periods eliminates most of the non-periodic variations, and shows whether any secular alterations are taking place. There is no doubt that since 1887, at all events, the rainfall over England has been much below the average; and a consideration of all the facts leads to the conclusion that such a period of scarcity is very likely to be followed by one of abundance, and that the coming few years will probably be more rainy than those recently experienced, although possibly the increase will not occur in the summer months—at a time when it would be most noticed.

The new number of *Petermann's Mitteilungen* opens with some interesting extracts from the diary of the late Dr. Anton Stecker, written during his journey in Abyssinia and the Galla countries in 1880–83. Stecker died before he had an opportunity of writing a full and systematic account of his travels. In the present extracts he notes not only the physical characteristics of the regions to which they relate, but the manners and customs of the natives. A good map makes it easy for the reader to trace his route.

A Greek gardener lately expressed the opinion that oranges, figs, olives, and grapes grown in Australia are inferior to those grown at Smyrna and Athens. This having been brought to the attention of the Department of Agriculture, New South Wales, letters were addressed to the British Consuls at Naples and Marseilles asking for a consignment of the best varieties of grapes, figs, and olives grown in Italy and France. On receipt of these cuttings, experiments are to be carried out at the most suitable of the experimental stations about to be established throughout the colony, with a view to the propagation of the finest varieties of the respective fruits. With the same object in view application has been made to Mr. T. Hardy, of South Australia, for a number of cuttings of various vines he has cultivated, and to Sir Samuel Davenport, of Beaumont, South Australia, for cuttings of the olive and fig trees grown by him. The whole of these cuttings will go to form the standard collections of all the different kinds of fruit which it is intended to establish at each of the experimental stations.

IN the *Revue Agricole*, published in Mauritius, M. A. Daruty de Grandpré gives an account of his attempts to raise sugar-cane from seeds. The seeds were sent from Barbados by the Governor in March 1890. M. de Grandpré planted them with the greatest care, and after five days was fortunate enough to obtain five minute seedlings out of the hundred seeds used. The young plants he raised did not all prove equally vigorous, and he was able to save only one, which, at the time when his report was written, had formed a fine clump of twenty shoots with long ribbon leaves. "I believe," he says, "that we may with reason cherish the most sanguine hopes from the propagation of sugar-cane from seeds—more especially if we try an intelligent system of cross-fertilization of the varieties we possess—rather than by planting cuttings, which maintain without appreciable alteration the respective characteristics of the parent plants. Thus we shall be able to supplement the weak points in our best varieties of sugar-cane by crossing them with others which are remarkable for the qualities it is intended to infuse into them, and we shall moreover obtain, by a process of selection, a cane rich in saccharine matter, which will enable us to compete successfully against the highly improved sugar-beet."

MR. A. W. MORRIS contributes to the current number of the Journal of the Bombay Natural History Society an interesting paper on abnormal horns of the Indian antelope. We have as yet little definite information as to the cause or causes of such abnormalities. Mr. Morris suggests that severe injuries to the skull, inflicted either during battle or through some accident, are the main causes that produce abnormalities, the horn on the injured side being thrown out of its natural course by the concussion or damage sustained.

THE Academy of Natural Sciences of Philadelphia prints in its Proceedings a list of the Echinoderms obtained by Mr. Frederick Stearns, of Detroit, in the Bahama Islands in the years 1887 and 1888. The list has been drawn up by Mr. J. E. Ives. It includes a description of a new species of Amphiura.

A VALUABLE revised list of British Echinoidea, by Mr. William E. Hoyle, has been printed in the Proceedings of the Royal Physical Society, Edinburgh, and is now issued separately. The author gives a brief diagnosis of each species, such as will enable the collector to identify it on the spot.

MESSRS. J. AND A. CHURCHILL have published a second edition of the English translation of Dr. A. Chauveau's "Comparative Anatomy of the Domesticated Animals." Dr. George Fleming is the translator and editor. In preparing the new edition, Dr. Fleming has kept in view the necessities of advancing veterinary education in the English-speaking schools. He has introduced, therefore, a considerable number of "amendments, alterations, and additions."

MESSRS. HENRY SOTHERAN AND CO. propose to issue a work entitled "Game Birds and Shooting Sketches," by J. G. Millais, F.Z.S. The work will illustrate the habits, modes of capture, and stages of plumage of game birds, and the hybrids and varieties which occur among them.

THE University College of North Wales has issued its Calendar for the year 1891–92.

LECTURES on the following subjects will be given at the Royal Victoria Hall on Tuesday evenings during the month of November:—November 3, Mr. F. W. Rudler, "Some Very Ancient Britons"; November 10, Dr. Rideal, "London Fogs"; November 17, Dr. W. D. Halliburton, "Skin and Bones" (second lecture); November 24, Rev. C. E. Brooke, "A Holiday in the Far West."

THE additions to the Zoological Society's Gardens during the past week include a White-fronted Lemur (*Lemur albifrons* ♂) from Madagascar, presented by Mr. J. M. Nicholl; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Mr. A. D. Watson; a Buffon's Skua (*Stercorarius parasiticus*), North European, presented by Mr. Edward Hart, F.Z.S.; two Common Cuckoos (*Cuculus canorus*), British, presented respectively by Mr. H. Lindsay and Miss Ord; a Burbot (*Lota vulgaris*) from the Trent, presented by Mr. F. T. Burrows; a Macaque Monkey (*Macacus cynomolgus* ♀) from India, a Lion Marmoset (*Midas rosalia*) from South-East Brazil, an Australian Cassowary (*Casuarius australis*) from Australia, deposited.

OUR ASTRONOMICAL COLUMN.

THE ZODIACAL LIGHT AND AURORÆ.—On the supposition that the zodiacal light is an extension of the solar corona, and that the latter mainly consists of light reflected from meteoritic particles circling round the sun over the spot zones and parallel to the plane of the equator, Mr. M. A. Veeder explains (Rochester Academy of Sciences, January 26, 1891) why in middle latitudes the phenomenon is brightest in March and October, in the former case after sunset, and in the latter before sunrise, and also the fact that at these times one margin of the band is better defined than the other, and more exactly included within the plane of the ecliptic, whilst at other seasons there is decreasing brightness, and both edges become ill-defined.

An investigation of observations of auroræ and magnetic perturbations shows that they may be arranged in periods having the same length as that of a synodic rotation of the sun. And it appears that the areas most frequented by sun-spots are most actively concerned in the production of auroræ. Extending the research, Mr. Veeder believes that the belt-like distribution of atmospheric pressure about the magnetic poles as a centre is very largely dependent upon magnetic induction of solar volcanic origin, conveyed from the sun to the earth through the medium of the coronal extensions referred to above.

COMET *e* 1891.—The following orbit has been computed by Prof. Campbell for the comet discovered by Prof. Barnard on October 2:—

$$T = 1891 \text{ November } 8{\cdot}75 \text{ G.M.T.}$$

$$\left.\begin{aligned} \pi &= 117^\circ\ 44' \\ \text{☊} &= 215\ \ 38 \\ i &= \ \ 75\ \ 50 \end{aligned}\right\} \text{Mean Eq. 1891.}$$

$$q = 1{\cdot}0166.$$

On October 30 the comet is in the position R.A. 10h. 53m. 7s., Decl. − 54° 43′. It is therefore not visible in our latitudes.

TWO NEW ASTEROIDS.—A new minor planet, (319), of the thirteenth magnitude was discovered by M. Charlois on October 8, and another, (320), by Dr. Palisa on October 11.

The latter observer has given the name of Thora to (299), Olga to (304), and Fraternitas to (309).

DOUBLE STARS.—Mr. S. W. Burnham announces that he is preparing a general catalogue of all the double stars discovered by him, and would be glad to receive any unpublished measures of them, Nos. 1 to 1224.

JUPITER'S FIRST SATELLITE.—Some recent observations made at Lick Observatory show that the first satellite of Jupiter is ellipsoidal, and that one of its longer axes is directed to the planet's centre.

THE INTERNATIONAL METEOROLOGICAL CONFERENCE.

THIS meeting, which was more or less of a private character, as it was not organized in any way through diplomatic channels, took place at Munich from August 26 to September 2. It was held in the building of the Technical High School, and was attended by 32 members, representing most European and some extra-European countries. As to the latter, the United States contributed four members, while Brazil and Queensland sent one each. Roumania and Bulgaria for the first time took part in one of these meteorological gatherings. Dr. Lang, the head of the Bavarian meteorological system, was appointed President, and Prof. Mascart (Paris) with Prof. Harrington (Washington) Vice-Presidents. The Secretaries were Dr. Erk (Munich), Mr. Scott, and M. Teisserenc de Bort (Paris).

The following is a brief summary of the most important practical results and recommendations of the Conference.

All temperatures published after 1901 are to be referred to the readings of the air thermometer. Actinometrical observations are not held to be sufficiently certain to justify their general introduction. The application of a ventilating arrangement to wet-bulb thermometers was recommended. *Rain.*—It was decided to count as days of rain those on which 0·005 inch (0·1 mm.) of rain was measured, and to print monthly the number of days on which 0·05 inch (or 1 mm.) fell. *Snow.*—A note is to be made in monthly schedules of the number of days on which about half the country surrounding the station is under snow. *Clouds.*—A new classification of clouds to replace Howard's, proposed by Prof. Hildebrandsson and the Hon. R. Abercromby, was adopted by a large majority, England and the United States being dissentients. A committee was then appointed to consider the question of typical cloud pictures in general, taking the above classification more or less as a basis of arrangement. A report was also received and adopted on the observation of the motions, &c., of cirrus and other high-level clouds. *Wind.*—Robinson's anemometer was the only form of instrument discussed. It was decided that no instrumental results should be published unless the instrument had been previously compared with a standard, either directly or indirectly. *Time.*—A proposal to recommend the adoption of universal or zone time was emphatically rejected, on the ground that local time can alone be used for climatological inquiry. It was further decided in all publications to insist on commencing the day with midnight as 0 hours. *Gravity correction.*—It was decided to introduce the practice of correcting barometrical readings for the force of gravity at lat. 45° after the beginning of the year 1901.

Mr. Wragge, for Queensland, and Captain Pinheiro, for Brazil, gave interesting notices of what is being done for meteorology in their respective countries. It was resolved that an International Meteorological Committee should be constituted to prepare for a possible Congress in Paris in the year 1896. The Committee is to consist of 17 members, of whom 14 were elected, and it was decided to fill the 3 vacancies by the co-option of extra-European meteorologists. The officers of the Committee—Messrs. Wild and Scott—were reappointed.

The questions relating to terrestrial magnetism were referred by the Conference to a special sub-committee, whose decisions will appear in the published report of the proceedings.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 19.—M. Duchartre in the chair.—Memoir on the underground temperatures observed at the Muséum d'Histoire Naturelle, during the winter 1890–91, by M. Henri Becquerel. A thermo-electric arrangement was used for the determination of the temperatures beneath two surfaces, one of which was covered with sand and devoid of vegetation, whilst the other had grass and some plants growing upon it. The two soils were similar, and in each case the temperatures were taken at five points, having depths ranging between 5 cm. and about 60 cm. The observations extend from November 1, 1890, to March 31, 1891, the temperatures being taken at 6 a.m. and 3 p.m. daily. These have been plotted, and the resulting curves strikingly show the variations which occurred in the interval, and the extinction of detail with increased depth. The diurnal variation at the greatest depth was a few tenths of a degree, whilst that of the air was about 14°. At a depth of 18 cm. beneath the sandy covering the variation was the same as in air, but at all the other points the effect was reversed—that is, the temperature fell from 6 a.m. to 3 p.m., and rose during the night. It also appears from the observations that Fourier's theory of the differential relation existing between temperature, time, and depth of thermometer represents very well the propagation of heat in a superficial layer of soil, and that the coefficient of conductivity of this layer for determined conditions of humidity may be deduced from observations of underground temperatures. A certain thickness of earth protects the roots of plants from the effects of a sharp frost, but it may not be equally efficacious against a long one of less intensity, for the velocity of propagation of a variation of temperature, and the depth at which this variation is felt, depends upon the duration of its period. A layer of grass, covering soil, has the same protecting effect during the winter as that of about 50 cm. of mould.—Researches on the cause of rheumatic diathesis, by M. F. P. le Roux.—Observations of Wolf's periodic comet, made at Algiers Observatory with the telescope of 0·50 m. aperture, by MM. Rambaud and Sy. Observations for position were made on August 4, 5, 8, and 31, and on September 7.—On the reduction, to a canonical form, of equations from derived partials of the first order and the second degree, by Mr. Elliot.—On cyclic systems, and on the deformation of surfaces, by M. E. Cosserat.—Calculation of the magnetic rotation of the plane of polarization of light, by M. G. Hinrichs. The simple law connecting the rotation of the plane of polarization with the thickness of the medium traversed is shown to be applicable to the molecular rotation of a normal paraffin.—On a new method for estimating nitric acid and the total nitrogen, by M. E. Boyer. The method is founded upon the reduction of nitric acid to ammonia, by oxalates and sulphur, in the presence of soda-lime.—On the action of nitric acid on dimethyl ortho-anisidine, by M. P. van Romburgh.—On the *globulicide* power of blood serum, by M. G. Daremberg. The author terms "*pouvoir globulicide*" the power possessed by the serum of the blood of one animal to destroy the red corpuscles of the blood of another of a different species. And the destructive power of serum for microbes is called "*pouvoir microbicide.*" The effects produced in each case have been studied.—On the nature of the movement of the chromatophores of Cephalopods, by M. C. Phisalix.

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